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THE
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MEDICO-CHIRURGICAL REVIEW.

JANUARY, 1856.

PART FIRST.

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REVIEW I.

1. *Treatise on the Oleum Jecoris Aselli.* By JOHN HUGHES BENNETT, M.D., &c. *Edinburgh*, 1841. pp. 180.
2. *On the use of Cod Liver Oil.* By C. J. B. WILLIAMS, M.D., F.R.S. ('London Journal of Medicine,' 1849.)
3. *First Medical Report of the Physicians to the Brompton Hospital*, 1849.
4. *L'Huile de Foie de Morue, envisagée sous tous les Rapports, comme Moyen Thérapeutique,* Par L. J. DE JONGH, Docteur en Médecine à la Haye.—*Paris*, 1853. Seconde édition. 8vo. pp. 257.

Cod Liver Oil considered in all its bearings as a Therapeutic Agent. By Dr. DE JONGH.

5. *On the Changes produced in the Blood by the Administration of Cod Liver Oil and Cocoa Nut Oil.* By THEOPHILUS THOMPSON, M.D., F.R.S. ('Proceedings of the Royal Society,' vol. vii., No. 3, 1854.)
6. *Third Lettsomian Lecture on Medicine for* 1854. By Dr. T. THOMPSON. (Delivered before the Medical Society of London.)
7. *On Cod Liver Oil.* By JONATHAN PEREIRA, M.D., F.R.S. ('Pharmaceutical Journal,' vol. viii., 1849.)

As it may be a matter of doubt whether there is any remedial agent more extensively employed at the present day than cod liver oil, its consideration must necessarily be of much interest to all engaged in the practice of medicine. Many remedies there are, indeed, or substances proposed as such, which annually spring up, partly from the inability of the proposer to refer effects to their proper causes, partly from a desire of gaining popularity, even at the expense of strict integrity; these having acquired some ephemeral reputation, when brought to the test of experience are found worthless, and sink into merited oblivion. Cod liver oil, however, cannot be classed amongst such agents, for instead of losing, it daily gains reputation, and has already stood the severe ordeal of extensive clinical observation. It appears to us, therefore, desirable to lay before our readers, in a condensed form, the principal points that have been really made out with regard to the origin, varieties, composition, and therapeutic action of this drug; and the more so as conflicting opinions seem to exist, not as to the value of the remedy itself, but as to the comparative efficacy of the different varieties of this oil now found in English commerce. Among the works at the head of our article is one by Dr. de Jongh, which we shall have most frequent occasion to allude to, owing to its giving a *résumé* of the whole subject. At the same time, we think it right in this place to remind our readers that much relating to the history and preparation of cod liver oil, up to the time of its publication, will be found in Dr. Bennett's work, as also considerable information regarding the opinions which have been held in reference to the action of the drug upon the human economy, and the diseases for which it has been given with the greatest success.

In his preface, Dr. de Jongh states, that although his present work has been modelled upon a preceding one published by him in 1843, under the title of 'Disquisitio Comparativa Chemico-Medica de Tribus Olei Jecoris Aselli Speciebus,' yet, in fact, with the exception of the chemical analysis of the three commercial species of cod liver oil, it has been almost entirely re-written, and may be considered as a complete monograph upon the subject of which it treats. The work is divided into four parts. In the first, the history of the medical employment of the oil, and also the progress in its chemical analysis, is detailed; in the second is given an account of the origin and preparation of the different kinds of oil; the third part is devoted to its chemical analysis, and the adulterations to which it is subjected; and the fourth to the value of the drug as a therapeutical agent, its mode of action, and also the kind most advantageously employed.

From the history we gather that in very remote times fish oils have been medicinally employed; that according to Pliny the Romans used oil from the dolphin, both internally and externally; that seal and whale oils were likewise reputed remedies; that Pliny himself recommended the oil obtained from the liver of the Gadus lota, or Burbot, under the name of Liquor mustelæ fluviatilis hepaticus, &c. As to cod liver oil, it appears to have been used from time immemorial in Sweden, Norway, Holland, and Germany, as a popular remedy for chronic gout and rheumatism, but not to have been prescribed by physicians before 1766, when it was used at the Manchester Infirmary. The remedy, however, was not at all extensively administered in this country till 1841, when Dr. Bennett's work made its appearance; and even for several years after that time, its use was almost limited to chronic rheumatism and scrofulous affections; it was not till the publication of the paper by Dr. C. J. B. Williams, in the 'London Journal of Medicine,' in 1849, On the Use of Cod Liver

Oil in Pulmonary Consumption, that it became generally employed, although various papers by Dr. Thompson, Mr. Chalk, and others, showing its efficacy in phthisis, had appeared in the different medical publications.

In France, its general adoption was still later than in England; but in Belgium, Holland, and in some parts of Germany, its use was of an earlier date. We must, however, refer such of our readers who may desire full information on the history of cod liver oil, to the works of Drs. Bennett and De Jongh. With regard to the progress of its chemistry, the following short summary may suffice, as most of the results obtained until within the last few years are now interesting only as matters of history.

In 1822, M. Wurtzer separated, both from the brown cod liver oil and ordinary fish oil, a yellow watery extract, with disagreeable odour and bitter taste, but did not proceed further with the analysis.

In 1828, M. Spaarmann found stearic (margaric) and oleic acids, together with phocenic acid, and a colouring and aromatic matter.

In 1830, M. Marder made a more elaborate analysis of the oil, and besides margaric and oleic acids and glycerine, described the existence of certain resins, colouring matters, salts, and gelatine.

In 1836, iodine was suspected by M. Kopp, and discovered in the oil by M. Hopfer de l'Orme, and this discovery was subsequently confirmed by many other chemists. After this, bromine was found to be a constituent by M. Herberger, and phosphorus in an unoxidized state by M. de Vry. The other important additions to our knowledge of this substance are chiefly due to Dr. de Jongh, and will be fully described as we proceed.

On the origin and preparation of Cod Liver Oil.—Oleum morrhuæ, according to the London, Dublin, and United States Pharmacopœias, is the oil derived from the liver of the common cod fish (Gadus morrhua, or Morrhua vulgaris); but in commerce it may be considered as including not only the above oil, but also that derived from the various species of Gadus, as the Gadus callarias, Gadùs carbonarius, &c. &c. De Jongh classifies the most important of these, in a medical point of view, as follows:

Gadus morrhua, or *Asellus major*, the *common Cod-fish;* found in large quantities on the coasts of England, France, Iceland, and Norway, but especially off Newfoundland.

Gadus callarias, or *Asellus striatus*, the *Dorse;* found largely on the Norwegian coast, and principally near the Lofoden Islands.

Gadus molva, or *Asellus longus*, the *Ling;* found also on the coast of Norway, though less abundantly than the above two species. Plentiful near England.

Gadus carbonarius, or *Asellus niger*, the *Coal-fish;* inhabiting the same localities as the last.

Gadus pollachius, or the *Pollack;* found in Norway, especially near Tromsoe.

Gadus merlangus, or *Asellus albus*, the *Whiting;* inhabiting the coasts of England and France. Besides many other species of less importance.

De Jongh then gives various accounts of the early methods employed at different places for the preparation of cod liver oil, which resemble each other more or less; amongst these we may mention the account given by M. Tiedemann, who states that there are four sorts of genuine oil, which are thus obtained:—the livers are packed in tall vats, furnished with three taps, placed at different heights, and then exposed to the sun, which favours the separation of the oil. On opening the upper tap, a pale oil is obtained; from the middle one, a light brown; and a darker brown, yet transparent oil from the inferior. These three are for medicinal use; by pressure, however, of the remaining mass of livers, a very dark and thick product is separated, which is made use of by curriers.

Finding that much obscurity existed, both as to the sources of the different oils and to the modes of preparation, Dr. de Jongh endeavoured to clear up the subject, and for this purpose made the following inquiries of M. Konow at Bergen, and of M. Mack of Tromsoe:—

1. What fish are used in the preparation of cod liver oil?
2. How are the three species, known on the Continent by the names of the pale, brown, and black oils, prepared?
3. Are these three kinds of oil prepared from the livers only?

The answer received from M. Konow was to this effect. That the Gadus callarias, or Dorse, was principally made use of in the preparation of the oil. That the fishery was, chiefly during the winter time, near the northern coasts of the Lofoden Islands. That after the fish were landed, the livers were separated and heaped in vats or tubs, and there left to the end of the fishery. When the fishery went on regularly, a very pure, pale, and limpid oil was commonly obtained; but when the conditions were reversed, the oil did not possess the above properties. At the conclusion of the season, the oil which had separated from the livers was decanted and put into kegs, and formed the "pale oil" of commerce. Afterwards, the livers were submitted to the action of heat, and a black oil obtained; the brown oil resulting either from the pale oil being left too long in contact with the livers, or kept too long after its separation. That the more oil contained in the livers, the better the product. That the Gadus carbonarius, or Coal-fish, yielded a clearer pale oil than the dorse; but the black oil obtained from that fish was thicker. That the coal-fish oil was considered less efficacious as a therapeutic agent; it was, however, often mixed with dorse oil. And, lastly, that not unfrequently other oils, commonly from some species of Gadus, but sometimes from the seal and other Cetacea, were often mixed with the true oil, and when so were very difficult to be distinguished.

The substance of the answers received from MM. Mack of Tromsoe was to the effect, that in commerce there were three fish liver oils, from the dorse, the coal-fish, and the pollack; that from the first being by far the most important article of commerce.

Dorse oil is thus obtained:—The livers are placed in vats, and left till decomposition ensues. A large quantity of oil separates, which, when collected, forms the "pale oil;" the remaining livers are then heated in iron boilers, from six to twenty hours, and yield a dark-brown oil, known as the "black oil" of commerce.

Coal-fish oil is prepared in the same manner, the pale and dark oils obtained differing from those of the dorse, in congealing very readily when exposed to cold.

Pollack oil, both pale and black, is also obtained as above described; but as the livers of this fish, which are more greasy than those of the dorse and coal-fish, decompose less readily, they are heated when still comparatively fresh, and then yield the "brown oil" of commerce. The brown oil from the dorse and coal-fish results either from the pale oil being left too long in contact with the livers, or, a pale oil having been first separated, the second yield possesses these properties.

MM. Mack assert that the livers of the fish are the only parts employed in the manufacture of the oil.

Dr. de Jongh thinks that from these two independent sources of information, we may conclude, as far as the Norwegian oil is concerned—

1st. That the fish made use of are the Gadus callarias and the Gadus carbonarius, chiefly the former.

2nd. That the *pale oil* flows from the livers when they have undergone a certain degree of putrefaction.

3rd. That the *brown oil* owes its colour either to prolonged contact with the livers, or from being afterwards kept too long.

4th. That the *black oil* is obtained by heating the livers after the separation of a large quantity of pale oil.

5th. That the livers only of the fish are employed in the manufacture.

He explains the slight discrepancy which will be noticed in the two accounts regarding the *brown oil*, by the circumstance that it is only lately that the Gadus pollachius has been used as a source of oil, and the liver not readily putrefying, heat is often employed earlier in the process.

Dr. de Jongh afterwards speaks of his visits to England in 1849, and also in 1851, for the purpose of ascertaining the kind of fish made use of, and the methods employed in the manufacture of the oil used in this country. He states that in 1849 most of the druggists in London prepared their oil by boiling the livers of the Gadus morrhua, or common cod-fish, in water, and afterwards separating the oil from the surface, and filtering it from any albumen or cellular tissue mixed with it; but that some made use of steam heat only, applied outside the vessels containing the livers. By either process, fresh livers being used, an almost colourless oil, and one devoid of taste and odour, is obtained. We may here state that the liquid process is not at present employed in this country, but that the best English oil is thus prepared:—The livers are collected daily, so that no trace of decomposition may have occurred, carefully examined, in order to remove all traces of blood and impurity, and to separate any inferior livers; they are then sliced, and exposed to a temperature not exceeding 180° Fahr., till all the oil has drained from them. This is filtered, afterwards exposed to a temperature of about 50° Fahr. in order to congeal the bulk of the margarine, and again filtered, and put into bottles well secured from the action of the air. Messrs. Bell and Co., we know, effect this separation of the solid fat; we believe, however, that this is not done by all the English manufacturers. Of course an oil thus prepared is less tender, or remains fluid at a lower temperature, than any other.

De Jongh states that in 1850 the oil used medicinally in this country was chiefly obtained from Newfoundland, and that it is prepared by heating the livers in water. This, I am assured, is not the process now adopted, but a method similar in all essential respects to the one described for the manufacture of the English oil, is made use of. It is important to correct this error with regard to water being employed, because De Jongh lays much stress upon the point, and considers that the product thereby becomes wasted and deteriorated. Perhaps the oil named Huile de Hogg, in his work, was thus prepared. Dr. Wood, in the United States 'Dispensatory,' gives the following as the methods for making the oil for medicinal purposes upon the coasts of America:

> "Upon the coasts of Newfoundland, Nova Scotia, and New England, the boats which fish near the shore being small, soon obtain a load, and running in to land, deliver their cargoes to persons whose business it is to cleanse and salt the fish. The oil is prepared either in the huts of the fishermen, or more largely at establishments to which the livers are conveyed in quantities. These are put into a boiler with water, and heated until they are broken up into a pultaceous mass, which is thrown upon a strainer covering the top of a cask or tub. The liquid portion passes, and upon standing separates into two parts, the oil rising to the surface of the water. The oil is then drawn off, and, having been again strained, is prepared for the market. Another and improved method, which has come into use since the extensive employment of the oil as a medicine, is to heat the livers in a large tin vessel, by means of steam externally applied. The pultaceous mass resulting is drained as before mentioned; the livers themselves containing, besides oil, a considerable portion of watery fluid, which passes off with it in the form of emulsion, and separates on standing. The oil thus procured is called *shore oil*, and is the purest kind. The crews of the large boats, which fish upon the banks far from land, cleanse the fish on board, and throwing the offal into the sea, put the livers into barrels or other receptacles, where they undergo a gradual putrefactive decomposition, the oil rising to the surface as it escapes from the disintegrating tissue. The oil which first rises, before putrefaction has very decidedly commenced, approaches in purity to the shore oil, but is somewhat darker, and less sweet. This is sometimes drawn off, and constitutes the *straits oil* of the fishermen. The remaining mass, or the whole, if the portion which rises first be not separated, remains exposed a variable length of time to the heat of the sun, undergoing putrefaction, until the boat, having completed her cargo, returns to port. The contents of the casks are then put into boilers, heated with water, and treated as already described. Before being finally put into barrels, the oil is heated to expel all its water. Thus prepared, it is denominated *boat's oil*, and is of the darkest colour, and most offensive to the taste and smell. Much of the oil prepared by the fishermen is collected by the wholesale dealers, who keep it in very large reservoirs of masonry in their cellars, where it becomes purified by repose, and is pumped into barrels as wanted for sale. By the further exposure, however, which it thus undergoes, it acquires a still more offensive odour; while that which has been originally introduced into barrels, and thus kept excluded from the air, is better preserved."

The oil used in France is derived chiefly from the French fisheries at Newfoundland, partly, however, from those on the French coasts. In Holland and Belgium, Norwegian and English (Newfoundland) oil is employed; in Germany, Norwegian; and in Spain, Portugal, and Russia, partly Norwegian, partly Newfoundland; and, lastly, American oil is for the most part prepared at Boston.

The third part of Dr. de Jongh's work is devoted to the chemistry of the subject, and contains the detailed results of many elaborate analyses of the different kinds of cod liver oil. Those of the three species so often alluded to—viz., the pale, brown, and black—were contained in the first publication; but to these are now appended more recent analyses of oils found in England, &c., &c. We shall endeavour to put our readers in possession of the principal conclusions at which De Jongh arrives, and compare them with those obtained by others, in order that they may judge how far we are in possession of a knowledge of the true composition of this important therapeutic agent, and how far we can by this knowledge explain its medicinal effects.

The physical properties of the three oils are as follows:

Pale Oil.—Golden yellow colour; peculiar, not at all disagreeable taste, at first bland, afterwards more or less irritating; with a feeble acid reaction; sp. gr. 0·923; slightly soluble in cold alcohol, rather more so in hot, and soluble in all proportions in ether.

Brown Oil.—Colour very similar to Malaga wine; odour more disagreeable than the pale oil; taste bitter, irritating the throat; slight acid reaction; sp. gr. 0·924; more soluble in alcohol than the pale, soluble in ether.

Black Oil.—Dark brown colour, approaching black, with a greenish tint; of a very disagreeable empyreumatic odour; bitter taste, irritating the throat considerably; slight acid reaction; sp. gr. 0·929.

To these we may add, in order that a comparison may be made between the different kinds, the physical characters of those now commonly used in England—the pale oil prepared in this country in the manner above described, and the best Newfoundland oil.

English.—Pale, almost colourless when fresh prepared; odour by no means disagreeable; taste soft, and not in the least degree acrid; sp. gr. about 0·917.

Newfoundland.—Pale yellow; odour slightly fishy, not disagreeable; taste soft, not acrid. By being exposed the odour becomes more developed, as with all the other kinds, at the same time the colour becomes darker.*

It would be unnecessary in this article to enter into the details of the methods employed by De Jongh in the analysis of the oil; but we may perhaps, with advantage, give the reader a short outline of the process, in order that those who are not fully acquainted with such subjects may form some idea of the manner in which the results are arrived at.

At first a watery extract is obtained, by agitation for some days with cold water, or by boiling the oil for some hours with distilled water, and, after the solution has been thoroughly cleared by repeated filtration, drying in a water bath. This watery extract is afterwards treated with ether, and absolute and diluted alcohol, in order to separate the different constituents composing it. The components of this extract are certain *biliary matters*, or a *bile* more or less altered, traces of *fat*, some *ill-defined organic substances*, and a small amount of saline matter, the nature of which will be seen in the table. As deductions as to the value of the different kinds of oil have been made from the varying amounts of these principles, we will give the results obtained by De Jongh, calculated to the 100 parts:

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* A pale, almost colourless Norwegian oil has, within the last three months, been imported from Norway, resembling very closely that made in this country.

Black or dark brown oil	1·288
Brown	0·890
Pale	0·607
Oil prepared by heating the fresh livers without water	0·637
Oil prepared by heating the fresh livers in water	0·339

It will be observed that the quantity of the watery extract is rather greater in the oil prepared as our best English is at present, than in the pale variety of De Jongh; but that when water has been used in the process of manufacture, the extract is much diminished, as would naturally be expected to be the case.

The next part of the analysis is for the purpose of ascertaining the *glycerine* and *fatty acids*, which is effected by saponification with caustic soda and oxide of lead, and the glycerine compared with the corresponding products obtained from olive oil and mutton fat.

As far as the fatty acids of cod liver oil are concerned, it would appear from the analyses that they are identical with *margaric* and *oleic acids* obtained from other fatty substances; the former having the composition $C_{34} H_{33} O_{3}$, the latter $C_{44} H_{34} O_{4}$. The glycerine in most of its properties agreed with common glycerine; its ultimate analysis was not effected. The amounts of these principles will be found in the table.

There are still some other organic matters contained in the oil. One discovered by De Jongh, and named *gaduin*, a substance which, during the saponification, attaches itself firmly to the oleic acid, from which it can be with difficulty separated by re-saponification and precipitation from the mother liquor by sulphuric acid. When separated it is a brown matter, insoluble in water, but soluble in spirits; assuming, however, an insoluble condition when evaporated to dryness. It gives a precipitate with acetate of lead, and in combination appears to have the composition represented by the formula $C_{35} H_{23} O_{9}$. By the action of sulphuric acid it becomes blood-red in colour. Gaduin resembles closely some of the slightly soluble substances obtained during the decomposition of bile by the influence of strong acids.

Besides the gaduin, De Jongh has found in cod liver oil two organic *volatile acids*, obtained by saponifying with caustic soda, decomposing the soap with sulphuric acid, and distilling. From the analysis of the acids which passed over, they were found to be *butyric* and *acetic* acids, having the respective formulæ $C_8H_6O_3$ and $C_4H_3O_3$ in their dry salts. De Jongh is inclined to think that the *phocenic* acid discovered by Chevreuil in the fat of the dolphin, and which other observers have stated to exist in cod liver oil, is only a mixture of butyric and acetic acids. Since the above analyses, however, it has been found by M. Berthelot that phocenic acid is identical with Valerianic acid. The volatile acids were found to be in much greater quantity in the dark than in the pale Norwegian oil; and in mere traces in the oils prepared in the manner of the English and Newfoundland. They are doubtless the products resulting from the decomposition of the fixed fatty matters.

Lastly, we have to notice certain *inorganic* substances contained in cod liver oil, to the presence of which much importance has been given by some writers, although, from the mere traces in which many of them exist, it is very questionable whether they possess any appreciable influence upon the therapeutic value of the remedy. Amongst the most important are iodine, bromine, and phosphorus.

Iodine is stated by De Jongh to be contained in all genuine cod liver oil; and this seems very probable, considering the extreme frequency of its occurrence (which has been lately discovered) in organized bodies. If the oil be burnt, the ash gives no indication of the presence of this element, as it appears to volatilize, thus accounting for the statements which have been made by some chemists as to its non-existence. When, however, the oil is previously made into a soap with caustic potash, and calcined, the ash then yields to spirit sufficient of the principle to give a well-marked dark-blue precipitate with starch; from its not being contained in the ash from the unsaponified oil, it would appear probable that it is united with some of the organic elements, and not in the state of a metallic iodide. The method employed by De Jongh to determine the amount of iodine, was to obtain it in the form of an iodide of palladium, which is quite insoluble in water, and gives accurate results. As many of the profession may be interested in this subject, we will give the process followed by our author for its quantitative determination:

A weighed quantity of the oil is first saponified with a caustic alkali known to be free from all traces of iodine, the soap is then burnt in a closed iron crucible, and the black cinder completely calcined; after cooling, the ash is exhausted with absolute alcohol by percolation, the alcoholic solution evaporated, and the extract treated with water, and carefully neutralized with sulphuric acid. To estimate the amount of iodine, a solution of nitrate of palladium is added, and the precipitated iodide of palladium dried at 212° Fahr. and weighed: qualitatively, the presence of iodine may be shown by the addition of a solution of starch and free chlorine. It was found that the pale and light brown oils contained more than the dark oil, but the *greatest* amount was only $\frac{4}{100}$ths of a grain in 100 grains of oil.

Bromine was also detected by the process of M. Balard, and chlorine by the usual silver test; the amounts of these elements will be seen in the table.

Cod liver oil also contains *sulphuric* and *phosphoric* acids—obtained by saponification with caustic potash, decomposition of the soap by hydrochloric acid; and afterwards, from the watery solution, precipitating the phosphoric acid as phosphate of peroxide of iron, and the sulphuric as sulphate of baryta. It appears, also, that *phosphorus* is contained in the oil, in small quantities, in an unoxidized condition, for after treatment with strong nitric acid it yields more phosphoric acid than before; and the amount of this principle (probably united with the organic elements) is determined by the difference of the phosphoric acid yielded by the oxidized and unoxidized oils: the pale oils appear to contain the largest quantities of such phosphorus. It would seem from the analyses that no sulphur exists in such a state.

The other substances found in cod liver oil by De Jongh were the bases *lime, magnesia,* and *soda,* in small quantities only, and a trace of iron in the black oil, supposed to be due to the heating of the livers in iron vessels during its preparation.

Table showing the Composition of the principal Varieties of Cod Liver Oil, as determined by De Jongh.

	Norwegian.			Pale, prepared with water.	English and Newfoundland, prepared without water.
	Black.	Brown.	Pale.		
Oleic acid, with gaduin and two other peculiar matters	69·78500	71·75700	74·03300	—	—
Margaric acid	16·14500	15·42100	11·75700	—	—
Glycerine	9·71100	9·07300	10·17700	—	—
Butyric acid	0·15875	—	0·07436	0·0174	0·0198
Acetic acid	0·12506	—	0·04571	trace	trace
Fellinic and cholinic acids, with small quantities of oleine, margarine, and bilifulvine	0·29900	0·06200	0·04300	0·0310	0·0590
Bilifulvine, bilifellinic acid, and two other peculiar matters	0·87600	0·44500	0·26800	0·1970	0·3360
A peculiar matter, soluble in alcohol of 30°	0·03800	0·01300	0·00600	0·0030	0·0070
A peculiar matter, insoluble in ether, alcohol, and water	0·00500	0·00200	0·00100	0·0010	0·0010
Iodine	0·02950	0·04060	0·03740	0·0315	0·0354
Chlorine, with a small quantity of bromine	0·08400	0·15880	0·14880	—	—
Phosphoric acid	0·05365	0·07890	0·09135	0·0702	0·0890
Sulphuric acid	0·01010	0·08595	0·07100	—	—
Phosphorus	0·00754	0·01136	0·02125	—	0·0190
Lime	0·08170	0·16780	0·15150	—	—
Magnesia	0·00380	0·01230	0·00880	—	—
Soda	0·01790	0·06810	0·05540	—	—
Iron	trace	—	—	—	—
Loss	2·56900	2·60319	3·00943	—	—

Conclusions drawn from the chemical analyses of the different kinds of Cod Liver Oils.—It would scarcely repay the time it would occupy to compare the results obtained by De Jongh with the ancient analyses of this substance. It is a point of great interest, however, to compare the different kinds of oil with each other, in order that when we arrive at the therapeutic portion of our subject, we may be better prepared to discuss their relative merits. In the first place, it seems in a high degree probable, although we find no chemical proof of it, that the oils contained in the livers of the different species of *Gadus* resemble each other very closely in composition: the difference between the Norwegian and English and Newfoundland oils depending upon the different methods employed in their preparation.

In the table we have compared the three Norwegian oils, and appended to them the analyses of two other kinds prepared and analysed by De Jongh, both made from the *fresh* livers of the *Gadus morrhua*, or common cod fish, one by the aid of heat and water, the other by the aid of gentle heat only—the latter would correspond with our best English oil, and also the present Newfoundland, as made for medicinal use; the former to a species of oil formerly made in this country and elsewhere, perhaps the *Huile de Hogg* of the author.

With regard to the *biliary* principles, De Jongh asserts that the black oil contains them in larger quantities than any other kind, and that the English oil, and that obtained by him by the aid of water, possessed the smallest amount. It will be remembered that we have before stated De Jongh to be in error with regard to this point, as the English is not now prepared with water, but would correspond with the oil prepared by him from the fresh livers by the dry process, except that some manufacturers separate part of the margarine, or solid fat, and thus render the per-centage of the peculiar principles of the oil greater. As to the correctness of the inference drawn from the analyses we have considerable doubt. Does the watery extract, as prepared by De Jongh, contain all the biliary and other peculiar organic matters which exist in the oil? Our author assumes this to be the case—there is no proof of it; it certainly, according to his own statement, does not contain all the inorganic principles.

Next, with regard to the nature of this watery extract, it will be found that the greater amount is made up of that portion which has been extracted by absolute alcohol, and which is designated in the table "bilifulvine, bilifellinic acid, and two other peculiar matters." What are these two peculiar matters—are they biliary at all in their nature, or are they only undefined products resulting from the decomposition of the tissue of the liver by putrefaction and heat, or by putrefaction only? In fact, on carefully examining the analysis, we find no proof or even probability of the black and brown oils containing any more real biliary matters than the pale oils made without the use of water, and should, in fact, be inclined to think that these latter contain the bile in a much more perfect condition than the former, the fellinic, cholinic, and other like acids being only the products of its decomposition. When we have to speak of the tests for cod liver oil, we shall again have occasion to revert to this point.

As to the *gaduin* discovered by De Jongh in all these oils, we are ignorant of its real origin and nature, and it has not been shown in which oil it exists in largest amount. Berzelius was inclined to think it a bile product.

The volatile acids—viz., the butyric and acetic acids—existing in larger quantities in black and brown oils, is readily accounted for, as they are probably the result of the decomposition of the fixed fatty substances, margarine and oleine, by a species of putrefaction. It will be found that with regard to the iodine, phosphorus, and phosphoric acid, the black oil is less rich than the pale varieties. The oil obtained by boiling the livers in water was found to contain less of the peculiar organic principles, as well as rather less iodine and phosphoric acid; the cause is evident, for during the preparation a part of these were extracted.

Lastly, as to the main constituents of the oil, the margaric and oleic acids, De Jongh's ultimate analyses appear to prove that they resemble those obtained from other fats and oils; the analysis of the glycerine is much less satisfactory, and upon this point it will not be useless to occupy a few lines, as recent researches have thrown some light upon the constitution of this portion of the oil.

De Jongh does not seem to hesitate for a moment in considering the substance obtained by saponification as real glycerine, although mixed with certain matters giving it a dark colour. Dr. F. L. Winkler* has, however, thrown considerable doubts on the subject; he shows that when cod liver oil is saponified, oleic and margaric acids are obtained, together with—not glycerine, or oxide of glyceryle—but, in its place, an oxide of propyle; and that cod liver oil is an organic compound, differing entirely from common oils in this respect: the glyceryle (C_6H_3) being replaced by propyle (C_6H_7). Winkler also shows that a body, *propylamin* (NH_2, C_6H_7), can be obtained from cod liver oil by the action of ammonia, and that this takes place in no other officinal oil; it may be as well to observe that propylamin is contained in pickled herrings, spurred or ergotted corn, &c.

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* Buchner's Neues Repertorium für Pharmacie, band i., heft 4, p. 165.

In concluding our remarks on the chemistry of the subject, we may observe, that although we would desire to express our admiration at the laborious and elaborate analyses of Dr. de Jongh, yet we would wish our readers not to consider that our knowledge of the peculiar constitution of this oil has by any means been perfected, or even very greatly advanced by them. We might have expected that the oil obtained from the cells of the liver would contain small amounts of bile, and when extracted from that of a fish, traces of iodine also. De Jongh's analyses show this and little more; Winkler's opinion, as to the existence of propyle, if proved, would add much to our knowledge of the subject.

Adulterations of Cod Liver Oil: Tests to determine its purity.—There is no doubt but that other oils have often been substituted for, or mixed with, cod liver oil; and it is a subject of no little importance to ascertain if the true oil presents any characters or reactions by which its nature and purity can be at once determined. Some years since, Mr. Hockin proposed the use of oil of vitriol for the purpose, and this test, being often employed at the present time, we will describe its action, and give the explanations which have been offered :—

When a drop of oil of vitriol is allowed to fall upon a thin layer of cod liver oil spread upon a piece of white porcelain, a centrifugal movement is observed around the acid, and a violet colour is produced. When a drop of the acid is stirred with about ten drops of the oil, the same beautiful violet ensues, which soon changes to a brown. At first it was thought that this tint was due to the iodine in cod liver oil being set free by the action of the acid; this, however, is not correct, for other fish oils containing this principle do not give the colour, nor does oil to which iodine has been artificially added; again, if the presence of iodine was the cause of the violet tint when an acid is added, starch should then (as Dr. Pereira remarked) become affected, and converted into the blue iodide of amidin. De Jongh considers as satisfactory the following explanation of the late Dr. Pereira, which is contained in a paper on this subject in the 'Pharmaceutical Journal' :—

"It is well known that, in 1844, Pettenkofer pointed out a new test for bile. If to a liquid supposed to contain bile, about two-thirds of its volume of oil of vitriol be added, the liquid kept cool, a few drops of a solution of cane-sugar (four or five parts of water to one of sugar) be added, and the mixture shaken up, a violet-red colour is produced, provided bile be present. This test succeeds very well if we dissolve a little extract of ox-bile in water, and test the solution with sugar and oil of vitriol. The colour developed agrees with that produced by the addition of oil of vitriol to cod liver oil, which De Jongh has shown contains the essential constituents of the bile.

"Pettenkofer remarks that the presence of a very great excess of chlorides will change the violet-red colour into a brownish-red. This fact is deserving of notice, because it may aid in accounting for the fact that some specimens of cod liver oil strike a brownish-red, not a violet-red colour, with oil of vitriol.

"Strecker confirms Platner's observation, that both cholic and paracholic acids produce the same colour with sugar and oil of vitriol as bile does; so that Pettenkofer's test doubtless acts on one or both of these acids. Now, De Jongh has shown that cholic acid is contained in cod liver oil, and we have therefore good reason for believing that it is in part by the action of oil of vitriol on this acid that the violet-red colour is produced in cod liver oil. But it is well known that for the development of this colour in bile it is necessary to use, besides oil of vitriol, a third agent (sugar). Pettenkofer observes, that for cane sugar we may substitute grape sugar or starch, in fact, any substance which can by the action of oil of vitriol be converted into grape sugar. No such substance has hitherto been detected in cod liver oil, and therefore it may be said the necessary ingredient to produce this characteristic reaction of oil of vitriol on cholic acid is wanting. Strecker has recently supplied the wanting link. In his valuable paper On Ox-bile, to which I have already referred, he observes that acetic acid may be substituted for sugar. To the liquid supposed to contain bile, add a few drops of acetic acid, and then concentrated sulphuric acid, when a magnificent purple-red colour is developed. If the quantity of bile be small, it may be necessary to use heat. Now, as cod liver oil contains acetic acid, we have the requisite agent to enable the oil of vitriol to act on the cholic acid, and the development of the purple or violet-red colour is then readily accounted for.

"I have already noticed the red colour produced by the action of oil of vitriol on gaduin (supposed by Berzelius to be derived from the bile). Here, then, is another source for the red colour caused by the action of sulphuric acid on cod liver oil.

"It follows therefore, from what has been now stated, that oil of vitriol is a test for liver oils. It does not distinguish one liver oil from another, for it reacts equally with the oil of the liver of the ray, and with the oil of the liver of the common cod. Neither does it distinguish good cod liver oil from bad, for it produces its characteristic reaction both with common brown cod oil, and with the finest and palest qualities. But it serves to distinguish oil procured from the liver, from oil obtained from other parts of the animal."

We may remark here, that although we do not agree entirely with the explanation above given, there is little doubt but that the colouration of cod liver oil by sulphuric acid is due to the presence of some biliary principles. In the first place it is not the oil itself which gives the colour, for after boiling it with water for some hours, it ceases to yield the reaction, although it then becomes brown, like an ordinary animal oil; this proves that it is due to a superadded principle. Again, this principle seems intimately connected with the liver, for not only do cod and skate liver oils give the peculiar colour, but we believe all fish *liver* oils (we have examined several), and even the livers of mammiferous animals, do the same. We had within the last two years a good opportunity of testing this in the human subject, in the case of a patient dying with fatty degeneration of the liver, when it was found that the oil from this organ yielded the colour to perfection. We may also state that the English and Newfoundland pale oils which we have examined—as Messrs. Bell and Co.'s, Warner and Barclay's, Mr. Fox's, and that procured from Messrs. Langton, Scott, and Co.—give a much finer display of colour than the Norwegian brown oil, and hence another proof, if any were wanting, of the inaccuracy of the statement of the brown oil containing more real biliary matter than the pale variety made from undecomposed livers. In using this test, we should remember that most oils become altered by the acid—some assume a brown, others a dark yellow or red; but this colouration has no resemblance to the beautiful violet-red tint which at first occurs when a true liver oil is employed.

In examining the effects of several reagents upon cod liver oil, we have observed that strong nitric acid gives rise to a beautiful rose-red colour, but that no effect is produced by it on other animal or vegetable oils.

De Jongh is led to believe, from his experience, that the amount of contained iodine is the best means of distinguishing true from false oils, or from admixtures; he thinks that any oil not containing from 0·020 to 0·030 per cent. of this substance should be regarded with suspicion; and as iodine has been added to oils, he shows that the admixture can be readily discovered, for the true oil does not part with its iodine either to water or alcohol, whereas if artificially added, it is readily separated by these menstrua; again, the true oil, on saponification, does not yield a trace of iodine to the mother liquor, iodized oil always does; and lastly, in the ash of true cod liver oil, burnt before being made into soap, no iodine can be detected (the iodine not being in the state of a metallic iodide), but if iodide of potassium, &c., be added, it can be so discovered. De Jongh considers that other oils—as the common fish oils (purified), mixed or not with iodine—are often sold for the genuine, and that these, as well as olive and poppy oils, are frequently mixed with it. He quotes several instances where chemists have failed to detect a trace of iodine in oil sold as cod liver oil; the more common adulterations are seal and southern oils, which are sometimes purified for the purpose, either by being treated with water, steam, and alum, or exposed to the action of decoction of oak bark, chloride of calcium, and sulphuric acid, or common salt, sulphate of copper and animal charcoal, &c., &c. De Jongh is inclined to attribute the inequality observed in the therapeutic action either to the employment of other oils, or mixtures, and although he regards as exaggerated the statement which has been made, that scarcely a tenth part of that sold as such is genuine unmixed cod liver oil, yet he thinks that adulteration is carried on to a very considerable extent.

The fourth part of the treatise of Dr. de Jongh is devoted to the consideration of the therapeutic value of the different kinds of cod liver oil, the diseases in which the remedy has been found beneficial, together with the mode of its employment, and the explanations of its action. As the conclusions arrived at by De Jongh with regard to the kind of oil best suited for therapeutic purposes are different from those generally entertained in this country, we shall enter at some length upon the subject, it being at the present time a matter of considerable importance, and one which requires to be impartially discussed.

We have already spoken of the mode of preparation of the different kinds of oil, their physical properties, and chemical composition; those to which we shall now have to allude are the three Norwegian varieties, the Newfoundland, and the English, as prepared by the present improved process.

De Jongh remarks that the preference was formerly, without any plausible reason, at one time given to the pale oil, at another to the dark coloured; but that lately, since its chemical nature has become better understood, physicians prescribe one or other kind according to their opinion as to the principle which gives activity to the remedy. He instances the names of M. Gaurée, Dr. Williams, and Mr. Donovan; and considers that most English practitioners employ the pale varieties, some from considering that the efficacy is due to the oil itself, and that in the pale oils there is less admixture of other matters; others, from attributing the curative powers to the iodine, which here exists in larger amount; and both from the consideration that pale oils contain little or nothing injurious, as might be the case with the various products of putrid fermentation. De Jongh thinks that experience has sufficiently demonstrated the fallacy of this prejudice, for he cannot call it by any other name, and states that for many years, both in Germany and Holland, much more of the dark than light-coloured oil has been used in medicine, and with excellent results; and he asks why the products of putrid fermentation, as volatile acids, &c., should not themselves be valuable remedies, seeing that ammonia and carbonic acid, which are also often produced by such decomposition, are employed with success? We have no objection to this question being asked, but we require evidence to prove that the answer should be in the affirmative. De Jongh seems to think it necessarily must be so. He refers, however, to several writers, and especially to MM. Trousseau and Pidoux, M. Falker, M. Rösch, MM. Haas, Schupmann, and Osberghaus.

When some of the evidence referred to is carefully analyzed, we find that its value in support of this opinion has been much over-rated. For example, on reference to the article, Huile de Morue, in the 'Traité de Thérapeutique,' of MM. Trousseau and Pidoux, although it is indeed stated that the dark oil is more powerful than the pale varieties, yet at the same time this is asserted not from practical experience of the different oils, but as the prevailing opinion at the time the pale oil was first introduced in medicine. Beside which, these authors quote the evidence of M. Bretonneau (a high authority) as to the equal efficacy of whale and other oils in the same diseases. The statements of MM. Falker, Rösch, &c., are of little value, being founded on preconceived notions as to the nature of the active principles of this remedy. It becomes also an interesting question to determine whether the different varieties of oil may not be useful in different diseases. De Jongh brings forward some evidence in favour of this being the case: M. Osius, for example, considers that the *dark* or *black* oil acts principally upon the abdominal organs and ganglionic system, and that it is especially indicated when torpor of these parts and of the nervous system exists; this is explained to be due to the large amount of empyreumatic and biliary matters contained; the *brown* oil, he thinks, is especially efficacious in specific inflammations of the respiratory and intestinal mucous membranes, and also of the fibrous tissues; whilst the *pale* oil, having more emollient properties, recommends itself in specific inflammatory affections of the respiratory organs, when they present the character of erethism. We shall speak more of this as we proceed.

Our author then refers to his own trials, made for the purpose of ascertaining the relative therapeutic value of the three kinds of Norwegian oil: eighteen patients were selected from the wards of MM. les Professeurs Suerman and Loncq, suffering from rheumatic or scrofulous affections, the first six were treated with the *black* oil, the next six with the *brown*, and the remaining six with the *pale;* care having been taken as to diet and regimen, the following results were obtained:—1. All the varieties of cod liver oil acted efficaciously. 2. The black oil acted most quickly, next the brown, and lastly the pale.

These are the deductions of De Jongh. No value, however, can be placed upon the second, for the number of patients selected (considering they were suffering from chronic diseases) was far too small; and again, the results by no means justify the statement he makes when he says, "This is why, resting on this experience, I have attributed a greater value to the dark coloured oils than to the pale." For it appears from the tables that the average time required for the cure of those under the brown oil was 5·33 months, that for those under the pale oil was 5·5 months, whereas for patients under the black oil the duration of cure was only 3 months.

The difference between the pale and brown oils, therefore (if these results are to be relied on), would not be appreciable in chronic cases; whereas the black oil is very greatly superior to the other two. How, then, can Dr. de Jongh assert that brown oil is much more efficacious than pale? would not any impartial examiner of his results consider his a false conclusion? He ascribes the difference of action to a difference of composition, and from the results of the analyses above given, attributes no little efficacy to the biliary principles and volatile acids. Such being the case, he considers it almost superfluous to inform his readers that since the period he has become aware of the difference in the compositions of the dark and light oils, he has confined himself to the employment of the dark coloured; but seeing the difficulty of inducing patients to take the nauseous black variety, he has been obliged to resort to the brown, which has always, as his comparative trials demonstrated (?), been more efficacious than the light coloured.

Before we speak of our own experience and knowledge of the subject, we will refer to the opinion of Dr. Pereira, contained in the article from which we have before given an extract. It is there stated, that

> "The characters by which we judge of the genuineness, purity, and goodness of the oil are partly physical, partly chemical. The physical characters which are usually employed are principally colour, odour, and flavour. The finest oil is that which is most devoid of colour, odour, and flavour. The oil as contained in the cells of the fresh liver is nearly colourless, and the brownish colour possessed by the ordinary cod oil used by curriers is due to colouring matters derived from the decomposing hepatic tissues and fluids, or from the action of air on the oil.
>
> "Chemical analysis lends no support to the opinion, at one time entertained, that the brown oil was superior, as a therapeutic agent, to the pale oil. Chemistry has not discovered any substances in the brown oil which could confer on it superior activity as a medicine. On the other hand, the disgusting odour and flavour, and nauseating qualities of the brown oil, preclude its repeated use. Moreover, there is reason to suspect that, if patients could conquer their aversion to it, its free use, like that of other rancid and empyreumatic fats, would disturb the digestive functions, and be attended with injurious effects."

The information we have been able to gather from various sources accords almost entirely with the above; and the opinions of several eminent medical men who have been in the habit of employing the oil very largely in the treatment of phthisis, &c., and who have made extensive comparative experiments, have been to the effect, that all the good which cod liver oil is able to produce in arresting tubercular disease, is obtained from the administration of the pale oil, such as is prepared in England or imported from Newfoundland; that the dark oils are much more apt to cause nausea and other unpleasant symptoms, and that many patients whose stomachs absolutely forbid the use of the dark nauseous preparations, can take with advantage the odourless and tasteless kinds.

Such are the opinions of some of the physicians connected with the Brompton Hospital for Consumption—we may instance Dr. Theophilus Thompson and Dr. Quain. Such also the published opinions of Dr. C. J. B. Williams, Dr. Wood of Philadelphia, and others. Our own experience fully accords with the above. We have been informed by many patients who have taken the different varieties of cod liver oil for some period, that they have always experienced an equal amount of benefit from the pale kinds as from the dark. That the checking of the progress of phthisis—as evidenced by the diminution of the cough and expectoration, the abatement of the rapidity of the pulse, the increase of weight of the body—can be equally produced by the pale as by the dark oils, we have almost daily proofs in our own practice. We have often, also, had evidence of its equal value in other tubercular and scrofulous affections, and in some forms of chronic rheumatism; the only possible question in our own mind is, whether the matters resulting from putrefaction contained in the dark varieties have ever any additional good effect—whether they may not produce some stimulant action rendering these oils more powerful in the treatment of chronic rheumatic cases: as yet we have seen no proof ourselves; it would, however, be worth while to examine this point.

To sum up, we should prefer (until proof has been shown to the contrary) the pale cod liver oil as prepared at present in England and Newfoundland, to either the brown or black oils, because—

1st. It is the real oil, as contained in the liver of the cod fish—rich in biliary matters, and also in iodine and other inorganic principles.

2nd. It contains no products of putrefaction, such as are found in the dark oils.

3rd. It sits more easily on delicate stomachs than the other varieties.

4th. Experience has proved it to be a most effective therapeutic agent.

In speaking of the diseases most suitable for the successful exhibition of cod liver oil, Dr. de Jongh arranges them in three classes—those connected with a *rheumatic*, *gouty*, or *scrofulous* diathesis; we shall dwell but shortly upon this part of the subject, indicating only the forms of these affections in which the remedy has been advantageously given.

Rheumatism.—In the acute stages of this disease, with febrile disturbance, cod liver oil cannot be administered, and even if given it would probably not be assimilated by the stomach: it is in chronic rheumatism, affecting the ligamentous tissues of certain joints, or certain groups of muscles, and the pains which not unfrequently remain after the acute disease, which are most benefited by this agent. Mr. Darbey thus describes the results of Dr. Kay's trials at the Manchester Infirmary, where the oil was first prescribed:

> "Men and women advanced in years, whose fibres may be supposed to have acquired a degree of rigidity, find surprising effects from it. Some, who have been cripples for many years, and not able to move from their seats, have, after a few weeks' use of it, been able to go with the assistance of a stick; and, by a longer continuance, have enjoyed the pleasing satisfaction of being restored to the natural use of their limbs, which for a long time before had been a burden to them."

Again, Dr. Bardsley, in 1807, remarks on the value of cod liver oil in the following manner:

> "From long and repeated experience I am enabled to speak of it as a medicine of efficacious but limited powers. In some instances, when every other means has proved unsuccessful, it has operated in a manner so decidedly beneficial as to excite astonishment. But on the other hand, it has frequently failed in some of the mild and more common forms of rheumatic affections. The circumstances under which I have found it most advantageous, when used both externally and internally, are the following:—1st. In the chronic rheumatism of elderly persons, when the muscles and tendons have become rigid, and the joints nearly inflexible, in consequence of the disease having been brought on by excessive labour, hard fare, dampness, and cold. 2nd. In women whose constitutions have been worn out by repeated rheumatic attacks after parturition, and more especially in the decline of life. I have seen a few patients recover entirely by the exhibition of the oil, who, on their admission into the house, were unable either to preserve the body in an erect posture, or support its weight on the lower extremities."

Many quotations from other authorities on this subject are given by De Jongh, who perhaps may be considered a little too partial to his favourite remedy, and too much inclined to attribute any want of success to the exhibition of an inferior oil.

Our own experience on the subject amounts to this, that cod liver oil is a most valuable therapeutic agent in those forms of chronic rheumatism occurring in subjects in whom the general nutrition of the body is defective, and, as remarked by Dr. Bardsley, in that terrible variety of the affection which at times comes on after uterine hæmorrhage and other great depressing causes. In many of these cases it may be advantageously given with the iodide or bromide of potassium, or with tonics.

Gout.—There is much discrepancy in the accounts given of the effects of cod liver oil in the treatment of gout. By some it is asserted to be valueless in this disease; by others, that it is more efficacious than in rheumatic affections. M. Brefield carries his opinion of its inefficacy in gout even so far as to think a differential diagnosis might be made by observing the effects of its administration. De Jongh considers that these discrepancies may be explained by errors of diagnosis; that many cases of gout are treated upon the supposition that they are rheumatic in character; and, again, that the different forms of neuralgia often obtain the name of rheumatism. We have tried the remedy in many cases of true gout, and think that it possesses little or no power in directly arresting the disease, or ameliorating the diathesis; at the same time, we consider that there are many patients suffering from the chronic forms of this malady, with weakened powers of assimilation, in whom the affection of the joints is kept up by debility, who are materially benefited by the administration of the oil; for we must remember that around gouty deposits a low form of inflammation is apt to occur in debilitated subjects, and that this is by no means necessarily gouty in character. A case, exhibiting symptoms not unlike those above described, will be found in De Jongh, extracted from the appendix to Dr. Bennett's work.

Scrofula.—The value of cod liver oil in almost all forms of scrofulous affections appears to be undoubted, and many pages are devoted to the relation of the varieties assumed by this disease, and cases illustrating the value of the oil in their treatment. We may enumerate enlargements of the lymphatic glands, scrofulous ulcers and skin diseases, scrofulous ophthalmia, rhachitis and malacosteon, arthrocacæ, &c. In all these diseases experience has now fully demonstrated the great utility of this agent, either given alone or combined, as it may frequently most efficaciously be, with iodine, iron, and other tonics. Many of the illustrative cases will be found in Dr. Bennett's work. Lastly, the subject of the treatment of phthisis pulmonalis is entered upon, and the whole history of the employment of the oil fully detailed, from the time it was first used in this disease by Haenkel in 1833, up to the publication of De Jongh's work in 1853; containing, amongst other valuable matter, full notices of the communication of Dr. Williams in the 'London Journal of Medicine,' in 1849, and of the first Medical Report of the Physicians to the Brompton Hospital. These papers, however, are so well known to the majority of our readers, that we shall not dwell upon them here, but only give the opinion of Dr. Williams in the communication above alluded to, namely, that "Pure fresh oil from the liver of the cod is more beneficial in the treatment of pulmonary consumption, than any agent, medicinal, dietetic, or regiminal, that has yet been employed."

Besides the diseases enumerated in De Jongh's work, most practitioners must have observed that affections which cannot be classed in the above category, are often greatly ameliorated or cured by the use of this agent. These states of the system cannot perhaps, as yet, be clearly defined, but will be found more or less closely connected with mal-assimilation.

One chapter in De Jongh's work is devoted to the consideration of the therapeutic action of cod liver oil upon the animal economy. On perusing the accounts of the early trials of this remedy, such as those described by M. Carron du Villards and others, one is forcibly reminded of the first effects ascribed to the discharge of the Leyden phial upon the electrical philosophers. Thus we find nausea and vomiting, diarrhœa and colic, diaphoresis and diuresis, amongst the common occurrences after its exhibition. Symptoms, we should think, rather to be ascribed to the imagination of those looking for effects, than to the remedy made use of. De Jongh, however, agreeing with MM. Brefeld and Mayer, the former of whom had abundant experience of the drug, states that he has never seen the employment of the oil followed by critical evacuations, and that, far from acting as a purgative, its use has often checked diarrhœa when depending on debility of the digestive organs; the only effect produced has been an improvement in the general nutrition of the body. We believe that most practitioners in this country will agree with our author in this statement; from our own experience we can do so fully.

How is this improvement produced? Is it due to one or to many of the constituents of the oil? Such are the questions which are often asked concerning the action of this remedy, questions which have had many answers, few or none very satisfactory, and many very absurd.

When gelatin and resins were considered to form a considerable part of its composition, the good was ascribed to them, although when given by themselves no such amelioration took place. When iodine was first discovered to be a constituent, physicians immediately explained the therapeutic action of the drug by the presence of this principle, and got over the difficulty of the small amount contained in it by attributing homœopathic virtues to its peculiar mode of combination, and explained in like manner the differences between the effects of the oil and the ordinary preparations of iodine. Many at the present day, although they do not ascribe all the therapeutic virtues to the iodine contained in it, yet regard this principle as an important element of its composition. Among those who have endeavoured thus to explain the mode of action of cod liver oil, may be mentioned Dr. Bennett, M. Mayer, and others. Dr. Bennett considers that this drug possesses the power of regulating the function of nutrition, when in a certain abnormal state, by acting as a stimulant to the lymphatic and capillary systems, and consequently improving the appetite and digestion, and thus the quality of the blood and the general nutrition of the body.

M. Mayer's opinion is, that the oil acts in some occult manner, influencing insensibly the metamorphosis, absorption, secretion, and elimination of the products of the disease.

MM. Pank, Delcour, Donovan, Champouillon, and others, regard the action of cod liver oil as peculiar, and altogether independent of the contained iodine. In support of this view, they state that cod liver oil has not unfrequently effected a cure where the preparations of iodine have entirely failed, and that M. Louis and others have obtained no amelioration in numerous cases of pulmonary phthisis in its various stages by the use of either iodine, iodide of iron, iodide of amidon, or ioduretted oil; but, in fact, often found disagreeable symptoms to arise from their administration. Again, in the Report of the Academy of Medicine, On the Substitution of Artificially Ioduretted Oil for Cod Liver Oil, it is stated, that the quantity of this substance in the latter is so small that it can scarcely be considered as contributing any important influence to this therapeutic agent.

Other physicians, again, attempt to explain how the oil acts by looking upon it simply in the light of an oleaginous body; amongst these, M. Ascherson believes that the contact of an albuminous and greasy body gives rise to the formation of elementary cells, composed of an albuminous envelope and oily nucleus, in the same way as he found that the contact of albumen and oily matter causes coagulation of the former and production of a cell, as above described.

He considers the chyle as an albumino-oleaginous emulsion, containing the necessary materials for the formation of elementary cells.

Dr. Williams also believes that cod liver oil cannot owe its efficacy to the contained iodine, but that it rather acts as an oil. In the paper at the head of our article he makes the following remark:

"The explanation which I have given of the chief salutary action of the cod liver oil, is not that it supplies fat where it is wanting, but that it supplies fat of a better kind, more fluid, more divisible, less prone to change, and more capable of being absorbed into, and of pervading, the structures of the body; thus affording a fine 'molecular mass' in the chyle, and therein a material for a better plasma; and being conveyed into the blood, distributed through capillaries and around deposits (in such quantities as to soften and dissolve the crystalline and irregularly concreted fat scattered through them), it renders them more amenable to the processes of reparation and absorption. Hence its beneficial operation is more marked in those stages of tubercular diseases in which the deposits abound in fat; that is, at the period of maturation and softening; although, from the mischief already done, both to the part and to the system, the benefit may not be so lasting as in the early stages of the disease."

De Jongh, in explanation of the action of the oil, reiterates his conviction, arrived at ten years before, that it operates not by one or other of its contained ingredients, but by a combination of all. He attempts no explanation of its intimate mode of action, avowing his inability to do so; and considers, that although those which have been made are highly praiseworthy, yet, that in general they show the powerlessness of man to penetrate into the secrets of the Creator. For our own part, we should at present be inclined to evince the like caution, and not hazard any very strong opinion as to the manner in which this therapeutic agent produces its valuable results. Before doing so, we should desire more information on many points. We do not consider that even its composition is at present satisfactorily made out; we know but little as to its effects on the constitution of the blood, and still less of its influence on the metamorphoses of the tissues, and on the various secreting and excreting functions. Until these are more accurately determined, we are satisfied to remain almost silent on the subject, only expressing thus far our opinion, that cod liver oil is doubtless a powerful and valuable therapeutic agent, and that we believe it acts rather as a peculiar oily substance, than by any adventitious principles which it may contain. The little we know as to its influence on the blood may be thus summed up:—Simon made an analysis, showing the change which ensued in the blood of a phthisical patient, after the administration of cod liver oil for a lengthened period of time; and the points of interest were—the large amount of solids (250 parts in the 1000), the diminution of the fibrin and increase of albumen in that fluid. More recently, some examinations were made by Mr. Dugald Campbell for Dr. T. Thompson, in seven cases of phthisis, according to Andral and Gavarret's method, the results of which are contained in the 'Proceedings of the Royal Society' for 1854.

		Red corpuscles.	Fibrin.
First stage, before the use of cod liver oil	Female	129·26	4·52
	Male	116·53	13·57
First stage, after the use of cod liver oil	Female	136·47	5·00
	Male	141·53	4·70
Third stage, after the use of cod liver oil	Male	138·74	2·23
Third stage, after the use of cocoa nut oil	Male	139·95	2·31
	Male	144·94	4·61

From these analyses it would appear that cod liver oil possesses a considerable power of increasing the solids of the blood, and this increase is found in that portion which is estimated as blood globules; that the fibrin is probably rather diminished. It would be interesting still further to pursue these inquiries, and apply more rigid methods of analysis. The whole subject, however, is beset with great difficulties. It will be observed in the above table, that the same changes were produced in the blood by the influence of cocoa nut oil.

Proposed Substitutes for Cod Liver Oil.—Cod liver oil having been proved beyond doubt to be a most efficacious remedy, as might naturally be supposed, many endeavours were soon made to find some substitutes for it; and these were sought for from various sources, according to the views held by different physicians as to the mode of its action, and the peculiar principles to which its therapeutic effects were due.

Other Fish Liver Oils.—The reader will remember, that what is named cod liver oil is derived from different sources, and little beside the English and Newfoundland oils are strictly obtained from the Morrhua vulgaris, or true cod fish. But, at the same time, it seems probable that no perceptible difference exists between the oils from the different species of Gadus, either as to their therapeutic value or chemical constitution. We have examined by several tests the oils from the ling, whiting, and haddock, without finding any notable difference. And again, the oils from other fish, as the ray and shark, closely resemble that from the cod fish. Shark liver oil has been imported into Liverpool by Mr. M'Innes. It gave the test with sulphuric acid, and its peculiarity seemed to be, that its density was very low, only 0·866 (Mercer). It was stated to be derived from sharks on the African coast.

Fish or Cetaceous Oils (not Hepatic).—The oils of this class most frequently tried have been sperm, seal, and southern whale oil. We made use of sperm oil in 1849. The result was favourable as to its therapeutic powers, when it could be persevered in; but, in general, patients appeared to become nauseated, and were unable to continue it. Dr. Theophilus Thompson has also employed sperm and seal oil, and "the result was a conviction that fish oils generally resembled one another in their remedial powers, although differing in their aptitude for digestive assimilation in the human stomach." We have already alluded to M. Bretonneau's opinion as to the equal efficacy of whale and cod liver oils in the treatment of disease. If Winkler's opinion as to the substitution of oxide of propyle for glycerine in cod liver oil be correct, it is probable that the same may be the case in the so-called fish oils.

Other Animal Oils.—We have ourselves no experience as to the efficacy of animal oils of this class. Dr. Thompson, however, appears to have made trial of neat's foot oil, and an oil obtained from the fat found between the parchment and leather skin of animals. The former was given in 14 cases of phthisis; 7 were essentially benefited (in 3 the disease was arrested), in 5 there was no obvious improvement, and 2 only lost ground. These favourable results have been confirmed by Dr. Radclyffe Hall. Neat's foot oil, however, being more disagreeable than cod liver oil, offers no particular advantage. It would be desirable to try lard oil, which can be procured very fine in this country. It, or something equivalent, has been used with success in Germany.

Vegetable Oils.—Many of these oils possess properties rendering them quite unfit to be used as substitutes for cod liver oil. We need not mention castor oil, except as an example of one which is not assimilated, but passes through the alimentary canal without alteration; many other vegetable oils to a certain extent resemble that from the castor seeds, as linseed, olive, and even almond oils; for, when taken in quantities, they not unfrequently purge and nauseate, when cod liver oil produces no such symptoms. Others, however, as the poppy oil, are more readily assimilated; and Dubois gives the results of 24 cases of rickets and strumous diseases treated by it, and thinks that no greater benefit could have resulted from the use of cod liver oil. Recently, a new oleaginous substitute has been proposed by Dr. T. Thompson, in cocoa nut oleine, obtained by pressure from cocoa nut fat or oil, and subsequently purified. In his Lettsomian Lectures, Dr. Thompson gives the results of his treatment with this substance in 53 patients labouring under phthisis. Of the first 30, 19 were much improved, in 5 the disease remained stationary, and 6 became worse. Of the last 23, 15 were materially benefited, 3 remained the same, and 5 became worse. We have tried it in some cases; in one, a male, suffering from chronic phthisis, the result as to the increase of weight was as follows:

1855.		Weight of body at a fixed hour. st.	lbs.	Diet remaining the same.
May	20	8	12½	Taking half-ounce doses of cod liver oil (Newfoundland) three times a-day. No other medicine.
—	21	8	13	
—	22	9	0	
—	24	9	0	
—	26	9	1	
June	2	9	2	
—	5	9	3	
June	6	9	4	Omitted cod liver oil. No substitute. Diet the same.
—	7	9	4	
—	8	9	4	
—	9	9	3½	
—	10	9	3	
June	11	9	3½	Taking cocoa nut oleine, half-ounce doses, three times a-day. Diet the same.
—	12	9	4	
—	13	9	5	
—	14	9	6	
—	15	9	6	
—	16	9	7	

This patient left the hospital on the 16th of June, being anxious to do so, feeling pretty well; he however continued the cocoa nut oleine for a few days afterwards, but was obliged to leave it off on account of its producing nausea and disturbance of stomach, which was not the case before; on June 23rd his weight was still nine stone seven pounds; he was then ordered to omit medicine for a time, and afterwards to return to the use of cod liver oil; and on July 7th, when he had again been taking the medicine (oleum morrhuæ) for about ten days, his weight had increased to nine stone ten pounds. This case appears to show that when cod liver oil is acting beneficially, and causing augmentation of the weight of the body, on its omission the increase is stopped, and after a few days a tendency to decrease exhibited; that then cocoa nut oleine produced the increase as rapidly as before; after nine or ten days, however, it produced so much disturbance of the digestive functions as to oblige its discontinuance. I may remark that this patient appeared to be able to take the cod liver oil for an indefinite period, without any such symptoms being induced. In some other of our patients, cocoa nut oleine produced decided benefit, but in the majority the remedy could not be persevered in for a lengthened period; we believe also that Dr. Thompson has not unfrequently found this to be the case; at present, the price of cocoa nut oleine offers no inducement to its use as a substitute.

Although it appears that many of the vegetable oils cannot be advantageously administered as internal remedies, yet, as this probably depends on the difficulty with which they are assimilated by the stomach, it may be that their use externally, as a means of introducing oleine into the system, would be attended with advantage; we require, however, much more clinical experience on this point before we can advance any such opinion with confidence. It is interesting to find, from Dr. Thompson's results, before detailed, that cocoa nut oil increased the solids of the blood in the same way as oleum morrhuæ.

What has been adduced as to the action of certain oily substitutes in producing effects analogous to those which ensue from the administration of cod liver oil, favours very much the opinion that the latter acts in a great measure simply as oleine, and this is strengthened by some observations which we have made during the administration of the margarine, or solid portion, obtained by Messrs. Bell and Co. by the solidification of cod liver oil: few patients were able to continue taking it, and no marked benefit was observed to result. None of the peculiar principles of cod liver oil, such as the biliary matters, iodine, phosphorus, &c., are contained in this portion.

Before concluding the subject of oleaginous substitutes, we may remark that until clinical experience has fully shown the equal efficacy of any of the proposed substitutes, we should not wish to see such an undoubtedly valuable remedy as cod liver oil replaced by any substance, however much it may resemble it, either in physical or chemical properties.

Glycerine.—In 1849 we made trial of the therapeutic value of glycerine, given internally in phthisis, a summary of the results is contained in the last edition of Thomson's 'London Dispensatory,' 1852, where it is stated,

> "The editor, some years since, tried its powers as an internal agent, giving it chiefly to patients in whom the use of cod liver oil was indicated, and in about the same doses—viz., from one to four drachms. The results of his trials were such as to induce him to place but little reliance upon its efficacy. In some cases it appeared to allay the cough of phthisical patients, but no very marked improvement could be found to follow its use."

Its exhibition was not persevered in for any very lengthened period, on account of the substance, as then made, containing traces of lead; but recently, since Mr. Wilson has discovered a new method for preparing it, by the action of high pressure steam upon oil, it has been again spoken of as a remedy. No evidence showing that it possesses any valuable therapeutic properties has been offered. All reason and analogy, and the little clinical observation as yet obtained, is against the idea of its acting similarly to cod liver oil.

A short chapter in De Jongh's work is devoted to the consideration of the best methods of administering cod liver oil. Although many patients at first show great reluctance to commence the remedy, after a short time this repugnance generally ceases, at last they swallow it with a degree of relish; such is more especially the case with children. We have ourselves often observed this in the administration of the pale oil, and so great is the influence of habit upon taste, that we can imagine even the dark coloured oil becoming palatable. As a rule, the oil is best given in its liquid state, and any attempts made to form emulsions or such like preparations have generally failed.* In this country it is frequently taken floating on a little wine, as orange wine, or wine and water, or ale or porter, sometimes with strong coffee or milk; some patients prefer water alone. In the case of the pale oil, which leaves no unpleasant flavour, the medium upon which it is given is of little importance; but with the dark oil, the taste of which is nauseous, and leaves a burning sensation in the throat, a strongly flavoured substance is desirable; some prefer chewing a piece of orange-peel afterwards.

The power of the stomach in digesting the oil, which differs much in different patients, may be occasionally improved by the simultaneous administration of bitter infusions, as of quassia, calumba, hops, &c., with a little dilute nitric or sulphuric acid. The use of salt before and after taking the oil, frequently effects the same object: this is noticed by Dr. G. O. Rees, in the last edition of Pereira's 'Elements.' When patients are really unable to take cod liver oil by the stomach, occasional benefit has resulted from its administration in the form of enemata; it may also, in like cases, be employed externally. In some persons whom we have found intolerant of this remedy, the intestinal canal has been proved afterwards to be the seat of tubercular ulceration.

For adults, the dose of cod liver oil may vary from two teaspoonfuls to a tablespoonful, twice or thrice a-day. A larger amount, as a rule, is not well borne by the stomach. The time of its administration is of little importance; most patients prefer it at, or shortly after a meal, and it is probable that at such periods the stomach possesses the greatest power of assimilation.

We have endeavoured in this article to give our readers a fair epitome of the contents of some of the publications referred to. Our opinion of Dr. de Jongh's work may be thus briefly summed up. We consider it a very complete monograph on the subject, and also that the views of the various authors referred to have been fairly stated. We regard his analyses of the different varieties of the oil as valuable, inasmuch as they put us in possession of many important data in reference to its chemical constitution; but at the same time we unhesitatingly affirm, that some of the conclusions which he has drawn are not warranted by the analyses themselves; and the same applies with still greater force to the deductions from the therapeutic data. As we have placed this work so prominently before our readers, and have devoted so much space to the consideration of its contents, we feel it our duty to state, that our remarks have been strictly confined to that publication the title of which will be found at the head of our article, and have no possible reference to a small pamphlet on the subject which appears to be very busily circulated, not only among the members of the medical profession, but among the laity also. This pamphlet assumes, whether rightly or not we are unable to say, to be written by Dr. de Jongh, and we should imagine that it has been published for other objects than those given for his journey to Norway, namely, "for the sake of humanity." In this small production gross errors and misstatements are put forth. It also gives cases related by De Jongh and others, many selected from the book we have reviewed, with medical details which are quite unfit for, and unintelligible to, the general reader.

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* A chocolate, containing cod liver oil, has been used in Germany, and is now prepared in this country by Mr. H. Lebaigne, 10, Little Titchfield-street. Some physicians think highly of it.

We trust, for the scientific and professional reputation of De Jongh, that he is not the author of this work.

A. B. Garrod.

Review II.

A Practical Treatise on Foreign Bodies in the Air Passages. By S. D. Gross, M.D., Professor of Surgery in the University of Louisville, &c. &c.—*Philadelphia,* 1854. pp. 458.

In the modern practice of medicine and surgery, a principle has been developed which bids fair to become of very general application, and to be of essential practical importance in all diseases in which spasm plays an important part. In these affections, a healthy and natural function becomes morbidly developed, and any force or resistance which may be directly opposed to it tends only to increase the diseased tendency. The true and philosophical method of treatment under such circumstances consists not in endeavouring to overcome the spasm by force, but by removing in some other way that to which the spasmodic action is opposed. In some forms of stricture, it is well known in how great a degree the spasm may be relieved by preventing the pressure of the urine against the back part of the urethra. This may be accomplished temporarily, by emptying the bladder, or by the administration of opium or chloroform; permanently, by an opening into the urethra, behind the seat of stricture. Perhaps the most scientific application of this indirect method of relaxing spasm is to be found in the management of cases of foreign bodies lodged in the air passages; and as involving a great principle, the modern method of treating these affections naturally finds a prominent place in the present review. In a well-known case, published by Sir B. Brodie in the twenty-sixth volume of the 'Medico-Chirurgical Transactions,' this principle of relaxing spasm by artificially preventing the sense of obstruction, is well illustrated.* In this case, after various ineffectual attempts had been made to dislodge a half-sovereign from the right bronchus, the patient was placed in a prone position, on a platform made to be movable on a hinge in the centre, so that one end of it being elevated, the other was equally depressed. The shoulders and body having been fixed by means of a broad strap, the head was lowered until the platform was brought to an angle of about eighty degrees with the horizon. The back being now struck with the hand, a violent cough was produced, but the half-sovereign did not make its appearance. This process was twice repeated, with no better result; and on the last occasion the cough was so distressing. and the appearance of choking was so alarming, that it became evident that it would be imprudent to proceed further in this experiment. An artificial opening was now made in the trachea, between the thyroid gland and the sternum. Sixteen days afterwards (the wound having been kept open), the patient was again placed in the same position as formerly. Two or three efforts to cough followed, and presently he felt the coin quit the bronchus, strike almost immediately against the incisor teeth of the upper jaw, and then drop out of the mouth. "No spasm took place in the muscles of the glottis, nor was there any of that inconvenience and distress which had caused no small degree of alarm on the former occasion."

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* The mechanical ingenuity of Mr. Brunel suggested to himself the plan of inverting his body so as to allow the coin to run out of, as it had run into, his patulous trachea, and he accordingly made the attempt; but there was an element, a principle here to be encountered, such as is never taken into calculation in estimating the facilities and difficulties involved in the construction of locomotive or atmospheric engines, and which the medical man alone is competent to deal with—namely, a self-acting spasm, the effect and attribute of vitality resisting the passage of any foreign irritating substance. Mr. Brunel's plan in itself, therefore, failed, and the practical cause of its failure lay in the violence of the spasm which the coin produced

It is to be remarked in this case, that the foreign body did not come, as might have been expected, through the artificial opening in the trachea, but that it passed without difficulty through the glottis, which was effectually closed against it on former occasions. The essential difference between the unsuccessful and the successful trial was, that in the latter the sense of obstruction, the feeling of suffocation, and consequent spasmodic action, were prevented by the artificial opening in the trachea; whereas in the former they mutually excited and produced each other. These observations have a direct reference to a very important practical point, and one which we enter upon at some length, as it is almost the only one to which we should have liked to have seen more attention given in Dr. Gross' excellent work. If, as we have endeavoured to show, an opening which will admit air in sufficient quantity into the lungs is all that is required in order to prevent a continued spasmodic closure of the glottis, then, in the great majority of instances which occur in practice, any part of the air passages below the seat of spasm may be selected for this operation. But the object of this operation, as almost universally hitherto practised, has been either to allow the foreign body to escape through the artificial opening in the trachea, or else to allow room for the introduction of a pair of forceps through the artificial aperture. An operation undertaken with these views rendered it necessary that the open-

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in the muscles of the rima glottidis at the moment of its contact with that opening. And here it was that the knowledge of the surgeon, superadded to that of the mechanist, completed the triumph of genius and art, and saved the man. Sir Benjamin Brodie made an opening into the windpipe close behind the point at which the spasmodic action was known to occur, partly with a view of quieting the spasm (an effect, by the way, first pointed out by my friend, Professor Porter, as one of the results of tracheotomy), but chiefly to give air to the lungs by a new and artificial route, and thereby afford time and opportunity for the foreign body to get past the obstruction; Mr. Brunel's body was then, as before, inverted, and, as was anticipated, the coin ran, without further obstruction, from the lungs into the mouth. Never did artistic and scientific skill combined produce a more marked, a more happy effect. The whole world rang with applause and congratulation.—Houston on the Modern Improvements in Surgery: Lancet, p. 393. 1844.

ing should be in the trachea. Now, as Dr. Gross observes, laryngotomy is a very simple and easy operation, but—

> "it is far different with tracheotomy. This is particularly true with regard to tracheotomy in children with short, thick necks, to say nothing of the cries and struggles which they are sure to make if they are not under the influence of chloroform, or nearly choked by the foreign body. I know hardly an operation in all surgery that I would not rather undertake than this, under such circumstances." (p. 231.)

It becomes, then, an object of very great interest to be enabled to substitute the simple and easy operation of laryngotomy for the difficult and hazardous one of tracheotomy; and this, upon the principle which we are advocating, we believe may be done in all cases where the size, form, or situation of the foreign body, do not present a mechanical obstacle to its being expelled through the glottis during inversion of the patient's body.

It is remarkable, in connexion with this subject, that in a table of 60 cases of tracheotomy, followed by the expulsion of the foreign body and recovery of the patient, that inversion of the body is mentioned as having been had recourse to in four instances only; and in a similar table of 13 cases in which laryngotomy was performed, it is not mentioned that the body was inverted in a single instance. In 6 of these 13 cases, the foreign body was expelled spontaneously, or by coughing, after the artificial opening had been made in the larynx.

Inversion of the body, which we have so much reason to believe to be such a valuable means of treatment in conjunction with an artificial opening into the larynx or trachea, cannot, however, be relied upon without such an opening. This principle, illustrated by the case above related, is confirmed by the following instance, which fell under the observation of the author of this review, seventeen or eighteen years ago, and is now published for the first time:

A lad was playing with a fourpenny piece, when it suddenly disappeared from his mouth. He applied to the late Mr. Goolden, of Maidenhead, in conjunction with whom the writer of this article saw him several times. The symptoms were such as to leave no doubt that the coin had passed into the trachea. Upon consultation, it was resolved to allow the upper part of the boy's body to fall over the side of a counter, while his legs lay across it. This was accordingly done, but it produced such a violent fit of coughing, accompanied by so much lividity of the countenance, that the experiment was given up. It was, however, repeated on different subsequent occasions, but always with the same result. The parents of the lad ultimately determined to send him to St. Thomas's Hospital, where he one day began to cough and became sick, and running to the water-closet, he distinctly heard the coin chink against the pan. The fourpenny piece was not recovered.

We now return to an analysis of Dr. Gross's work. The facts which form the basis of this volume are, as we are informed, derived from personal observation, from the experience of professional friends, but mainly from the various journals of the United States, Great Britain, and the continent of Europe. The first chapter treats of the foreign bodies which may enter the air passages: their nature; the alterations which they may undergo; their situation; their mode of entrance and expulsion. The most important subject noticed in this chapter is the almost universal tendency that foreign bodies have to pass into the right bronchus. A remarkable exception to this rule is recorded by Dr. Hughes in the 'Dublin Quarterly Journal' for May last. This, Dr. Hughes remarks, is the first recorded case in which a foreign body was discovered in the left lung (p. 321). The tendency of foreign bodies to pass into the right bronchus, Dr. Gross observes—

> "Was supposed by almost every one, until recently, to be owing to the difference in the character, length, and direction of the two tubes. As the right is shorter, wider, and more horizontal than the left, it was perhaps natural enough to conclude that it was particularly favourable to the entanglement of foreign substances. But there was unfortunately one great defect in this theory, the fact, namely, that it omitted to take cognizance of the circumstance that a foreign body descending the trachea, by virtue of the laws of gravity would be more likely to seek an oblique than a horizontal passage. The left and not the right bronchial tube is the one into which the offending substance ought generally to fall. This however, as has been already seen, is not the case. . . . The true cause undoubtedly is the peculiar position and arrangement of the *septum* at the root of the trachea. This septum is not in the median plane, but decidedly to the left of it. Hence a body will be very likely, by striking this septum, to be pushed over towards the right side, its entrance into the corresponding tube being still farther favoured by the greater diameter of this tube. . . . . Mr. Goodall, of Dublin, appears to be entitled to the credit of having first called the attention of the profession to the part played by this septum in directing the passage of foreign bodies." (pp. 46, 47.)

The number of cases analysed by Dr. Gross, in which death occurred without operation and without expulsion of the foreign body, is 21.

> "Of these the substance was situated, in 11, in the right bronchial tube; in 4, in the larynx; in 3, in the trachea; in 1, partly in the trachea and partly in the larynx; in 1, in the lung; and in 1, in the right thoracic cavity." (p. 49.)
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> "In the great majority of instances, the presence of extraneous substances leads to serious structural changes in the pulmonary tissues, followed sooner or later by the death of the patient. . . . Death is produced very much in the same manner as in ordinary phthisis. . . . It not unfrequently happens that tubercular matter is deposited in considerable quantities around the extraneous substance. . . . When the foreign body is retained for a long time, it is generally encysted, and so becomes partially, or it may be completely, harmless. Such a termination, however, is extremely rare." (p. 53.)
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> "Foreign bodies introduced into the air passages by the glottis are sometimes expelled through an abscess, ulcer, or fistula in the walls of the chest." (p. 54.)

The immediate effects of foreign bodies, when introduced into the windpipe, which forms the subject of the second chapter, are too well known to require any lengthened remark. The following extract, however, is given, as having a direct reference to the observations made at the commencement of this analysis:

> "All fluids, however simple, are capable, when introduced into this tube, of exciting the most violent, spasmodic, and suffocating cough; but the impression is evanescent, for the reason that liquids can produce no mechanical obstruction to respiration. The moment the spasm subsides, the breathing is re-established. All solid articles, on the contrary, whatever may be their character, will, by entering the windpipe, or resting against the mouth of the larynx, endanger life by suffocation." (p. 60.)

The pathological effects of foreign bodies when introduced into the air passages form the subject of the third chapter. The mucous membrane in contact with the foreign body is liable to become inflamed, and in chronic cases to become "thickened, more or less indurated, and deeply congested." It may secrete coagulable lymph, mucus in increased quantity, or muco-purulent matter; or the parts immediately in contact with the foreign substance may ulcerate. But the most important change is inflammation of the lungs, which may involve an entire lobe, or the whole of one lung, or even extend to both lungs simultaneously. As the disease advances, the affected organ "undergoes hepatization, and, finally, if the action is perpetuated, it becomes infiltrated with purulent matter." (p. 66.) Œdema of the larynx and pulmonary emphysema are also occasional consequences of this accident. The bronchial glands and pleura also become sometimes implicated in the disease.

> "It is a singular fact that the pathological changes now enumerated may all occur, to a greater or less extent, in cases where the obstruction is seated, not in the lungs or bronchial tubes, but in the larynx or upper portion of the trachea." (p. 69.)

The symptoms produced by foreign bodies in the air passages are divided by Dr. Gross, in his fourth chapter, into those which are immediately induced, and into those which are subsequently produced by the inflammation caused by the continued presence of the extraneous matter. The latter are called *secondary affections.*

Immediately upon the entrance of the foreign substance, the patient gasps for breath, looks wildly round him, coughs violently, and almost loses his consciousness. His countenance becomes livid, the eyes protrude from their sockets, and froth, and sometimes even blood, issues from the mouth and nose; sometimes a disposition to vomit, or actual vomiting occurs immediately after the accident. The relief occasionally experienced from this source is very great.

The symptoms denoting the secondary affections are very various. They are sometimes absent altogether. When cough is present—and this is the most prominent and important symptom—it is usually spasmodic, sudden, short, and uncontrollable; sometimes it is of a croupy character. After having existed for some time, it may disappear and never recur. It may be influenced by the patient's posture. He may be perfectly free while sitting up or lying down, but the moment he rises or turns his body, he may be seized with a violent paroxysm.

The voice is usually natural, but it may be croupy, or hoarse and low, or sharp and sibilant; sometimes it sounds cracked, or it may be reduced to a mere whisper; or again, there may be loss of voice, either wholly or in part. Sometimes the power of speech is temporarily lost, and then returns, either suddenly or gradually, without any assignable cause.

The expectoration is generally a thin sero-mucus, and varies in quantity from a few drachms to several ounces in the day. Not unfrequently it is thick and ropy, more or less opaque, and remarkably abundant. In protracted cases it is generally more purulent; and sometimes it appears to consist entirely of pure pus. Occasionally it is tinged with blood, or the quantity of blood thrown up may vary from a few drachms to several ounces.

The abscess formed by the foreign body may become gangrenous. If the secretion be profuse and puriform, the symptoms are usually those which attend pulmonary phthisis.

The pain, when experienced, may be of a sharp pricking character, or it may be dull, heavy, and aching. It may be limited to the seat of the foreign body, or it may pervade the trachea, larynx, bronchial tubes, and even the lungs. Generally speaking, however, it is very slight. It is liable to be aggravated whenever the patient coughs, and to be accompanied by a sense of constriction, tightness, or suffocation. It occasionally remains fixed for a time at one spot, and then suddenly shifts to another. As might naturally be expected, the pain is more likely to be fixed when the foreign body is immovable; but it may remain in its original position long after the foreign body has been expelled.

In the fifth chapter of his work, Dr. Gross remarks:

"Although the symptoms which denote the intromission and presence of a foreign body in the air tubes are in general sufficiently well marked, yet occasionally the most thorough examination of the patient, and the most minute inquiry into the history of the case, fail to afford the requisite light for the formation of a correct opinion." (p. 89.)

The difficulty of the diagnosis is much greater in children than in adults.

"If a child," observes Dr. Gross, "has been playing with a grain of corn, bean, pebble, or a similar body, and has been suddenly seized with the symptoms of suffocation, violent spasmodic cough, lividity of the face, pain in the upper part of the windpipe, and partial insensibility, the presumption will be strong that the substance has slipped into the air passages. This presumption will be converted into positive certainty if the person was just previously in the enjoyment of good health." (p. 90.)

Dr. Gross nevertheless gives an instance in the following page, in which a child was *suddenly* attacked, while playing with some ears of corn, with severe cough, croupy state of the voice, and difficulty of respiration.

"A less timid surgeon might have been induced to perform what would certainly have been in this instance a useless and improper operation.

"It has been pretended that the presence of a foreign body may be detected by the peculiar smell of the expectorated matter; the late Mr. Liston affected this faculty. On one occasion, in a gentleman who had a necrosed piece of cricoid cartilage in the trachea, he thought the fluid coughed up by the patient resembled that ejected when there is an extraneous body in the windpipe. The diagnosis, singularly enough, was verified by what actually occurred; for about a week before the man died he coughed up a piece of cartilage, and a very small fragment of the same substance was found in the left bronchial tube at the post-mortem examination. Not even Mr. Liston himself had suspected that the patient had inhaled a portion of his own larynx." (pp. 94, 95.)

A more extraordinary case still is quoted at page 101, from a former number of the 'British and Foreign Medico-Chirurgical Review.' The case was that of a boy, aged twelve years, who inhaled the larynx of a recently-killed goose, which became entangled in his own. Eighteen hours after the accident he had lividity of the countenance, spasmodic contraction of the muscles of the neck, and a clear whistling sound in breathing, followed at each expiration by a hoarse noise, not unlike that of the voice of a goose. The trachea was opened, and the substance removed with the forceps.

Whenever a foreign body is impacted in the lower part of the trachea, or in the bronchus, or when it is of such a size or shape that it cannot move freely in the trachea, tracheotomy will be necessary. This operation has the advantage, under the circumstances, of allowing the surgeon to reach the offending substance with the forceps, or other instruments, and thus to dislodge it. This point is admirably illustrated by a case under the care of the late Mr. Liston. In this instance, six months and a half had elapsed before tracheotomy was performed. A piece of bone was situated in the right bronchial tube, and was removed by a pair of forceps introduced through the wound in the trachea.

In performing tracheotomy,

"The surgeon steadies the trachea with the left index-finger, or, what is more effective and more satisfactory, with a tenaculum, and divides at least three rings. In executing this step of the operation, the knife is entered at a right angle to the surface of the tube, with its back towards the sternum. The incision in the trachea must strictly correspond with the centre of the external wound, and should be at least from nine lines to an inch in length. If shorter than this, it will scarcely suffice for the proper play of the forceps." (pp. 234, 235.)

" Tracheotomy" is to be preferred to laryngotomy, in Dr. Gross's opinion, " when the foreign body moves up and down in the windpipe." (p. 236.) In this opinion we cannot coincide, for the reasons already stated. We believe that under these circumstances the foreign body may be expelled through the natural opening by inversion of the body, if only care be taken to prevent spasm of the glottis. For this purpose we should select the easy, simple, and comparatively safe operation of laryngotomy.*

The chapters in Dr. Gross' work which have reference to the treatment of cases in which foreign substances have entered the air passages, are, of course, the most interesting. They are well illustrated by woodcuts, and every point touched upon is elucidated by one or more well recorded cases. The following is Dr. Gross' own summary of these chapters, those points being omitted in reference to laryngotomy and tracheotomy to which we have already alluded.

Although foreign bodies have occasionally been ejected from the windpipe under the influence of emetics, errhines, and other means, the number of such cases is too small to justify the practitioner, under any circumstances, in confiding in these classes of remedies.

These remarks are equally applicable to all spontaneous efforts at expulsion.

Inversion and succussion of the body, with or without beating the chest, are generally hazardous proceedings, unless preceded by an opening in the windpipe; for the reason that the offending substance, if it be forced out of its lurking-place into the larynx, or even against this portion of the tube, is inevitably followed by violent coughing and suffocative symptoms; thus greatly endangering the safety of the patient. The only case in which they ought to be practised is when the foreign body is that of a bullet, or some similar substance.

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* In a pamphlet lately published, Dr. Patrick Black has shown that the prolonged inhalation of chloroform tends, in a remarkable manner, to prevent spasm of the glottis, and that a state of concentration of the vapour of chloroform, which at first would suffocate a patient, may, after insensibility has been partially induced, be continued without spasm or irritation. Now, if under this partial insensibility, the reflex actions of the muscles of the glottis offer less resistance to the entrance of the pungent vapour of chloroform, it is highly probable that they would likewise oppose less resistance to the escape of any foreign body from the larynx. It remains, therefore, to be seen how far the relaxation of the muscles of the glottis, by the administration of chloroform, will supersede the necessity of opening the windpipe in cases of movable bodies in the trachea; and, *à priori*, it would appear highly probable that the proper use of this means, together with the inversion of the patient's body, would be sufficient to produce the expulsion of the foreign substance in a certain number of cases. Whenever such an attempt is made, the surgeon should, for obvious reasons, be prepared at once to perform bronchotomy, should it become necessary.

Inasmuch, then, as no confidence is to be placed in the use of emetics, errhines, and other similar means, inversion and succussion of the body, and not even in nature's own efforts; and inasmuch, moreover, as no patient can be considered safe so long as the extraneous substances remain in the air passages, it follows as a necessary corollary, that bronchotomy affords the best chance of relief, and that, consequently, it should always be resorted to as early as possible, unless there is some special contra-indication; as, for example, serious organic disease of the respiratory organs. The great danger of this accident is spasm of the glottis, which nearly always promptly disappears the moment the artificial opening has been effected. In children, and in young and timid persons, the operation should always be preceded and accompanied by the administration of chloroform, which, while it perfectly calms the patient, greatly facilitates the extraction of the foreign body, by rendering the respiratory organs tranquil and passive.

The windpipe, as a general rule, should never be opened before there is a cessation of the hæmorrhage, lest the blood, by falling into the tube, should embarrass the operator, if not seriously compromise the safety of the patient.

Under no circumstances should bronchotomy be performed without a thorough exploration of the chest and œsophagus. It should be remembered that mere spasm of the glottis, caused by the lodgment of a foreign body in the fauces or gullet, or by derangement of the digestive, respiratory, and nervous functions, may induce a train of phenomena closely resembling those occasioned by the presence of a foreign body in the air tubes. Bronchotomy is generally inadmissible when there is serious organic disease of the lungs, attended with marasmus and all the ordinary symptoms of pulmonary phthisis.

A much better plan than searching for the foreign substance with an instrument, is to invert the patient's body, and to strike the chest with the hand. This procedure should be tried in all cases of balls, shot, peas, beans, water-melon seeds, plum-stones, cherry-stones, button moulds, and other similar articles. Inversion of the body, with previous opening of the tube, is comparatively a safe operation. Succussion and percussion are important auxiliaries in such a case.

In closing our review of Dr. Gross' work, we must express our conviction that the thanks of the profession are eminently due to the author for having collected within the compass of one volume all that had before been done upon the subject of foreign substances in the air passages, and for having brought into a systematic form the information which was scattered through a very large number of periodicals. We would particularly recommend to every practical surgeon a perusal of the very large number of cases which are quoted in this excellent work.

Henry Lee.

REVIEW III.

1. *Lehrbuch der Geburtshülfe, mit Einschluss der Geburtshülflichen Operationen und der Gerichtlichen Geburtshülfe.* Von Dr. ANTON FRIEDRICH HOHL, Professor und Director der Geburtshülflichen Klinik in Halle, &c. &c.—*Leipzig*, 1855. pp. 1139.

Manual of Obstetrics, including Obstetric Operations and Medico-Legal Obstetrics. By Dr. A. F. HOHL, Director of the Obstetric Klinik at Halle.

2. *Clinical Lectures on the Diseases of Women and Children.* By GUNNING BEDFORD, A.M., M.D., Professor of Obstetrics, &c., in the University of New York.—*New York*, 1855. pp. 563.

DR. A. F. HOHL is favourably known to the profession by his works, 'Die Geburtshülfliche Exploration' (Halle, 1833), and 'Die Geburten Misgestalter, Kranker, und Todter Kinder,' published in 1850; Dr. Bedford occupies a very prominent position in the city and University of New York—and the two are well calculated to afford our readers a pretty correct view of the opinions and experience of very distant countries. It has given us great pleasure to find less difference than we expected from the views held by the most experienced obstetricians in Great Britian, which doubtless is attributable to the greater intercourse which, both personally and through the press, has taken place of late years.

The large size of these volumes, and the multiplicity of subjects they embrace, render it impossible for us to take them in much detail; we shall be contented if, by our cursory review, we induce our readers to have recourse to the volumes themselves.

Dr. Hohl's work is of a much more ambitious character than his former productions, embracing not merely the entire range of obstetrical science, but a large portion of the diseases of women, which, we think, would have been better left for a separate work. There is, moreover, a feature which is quite novel in a manual of this kind—viz., the various legal questions involved in, or resulting from, the subjects discussed, and which are appended to each chapter. As we do not intend further to notice them, we may at once state that they appear to us to be fairly stated and carefully considered, and no doubt will be found of great value, at least by the junior practitioner.

But although we may think the range of subjects too extensive for a single volume, and that the separation of midwifery from the collateral subjects would be an improvement, we cannot complain that the subjects are slurred over; some might be better for a little more detail, but in general they are discussed with great minuteness; and, as we should expect from a German, the industry displayed in research is very great. The author seems perfectly familiar with the whole series of German and French writers, and with the current periodical literature of all countries: if in any point he is deficient, it is in his knowledge of British systematic midwifery. The reader will find his references copious, and, we believe, accurate.

The volume consists of four parts: the first embracing the psychical and physical peculiarities of women, as bearing upon midwifery; the second, pregnancy, with its consequences, symptoms, and diseases; the third, parturition and its varieties; and the fourth, childbed and its diseases. Each part has many subdivisions, and is treated with sufficient minuteness; the whole being illustrated by seventy-six engravings, accurate enough for the purpose, but certainly inferior, artistically speaking, to the engravings published in this country.

The anatomical section is very good: we have comparative measurements of males and females, of living women and of the female skeleton; then a description of the organs of generation, and certain diseases to which they are liable; after which, the subject of the female pelvis, its development and abnormal conditions, is treated very fully, but without any special addition to our knowledge. The division of pelvic distortions is much the same as in books by British authors, with, perhaps, more of detail and a more careful reference to authorities. Dr. Hohl has entered into the consideration of the causes of these deformities, and their mechanical effects upon labour, with considerable care. The section on the different modes of exploration, including internal and external measurements, the touch, the sound, pelvimeters, &c., is very full—in some parts, indeed, rather tediously minute. The various theories of menstruation are noticed, down to those put forth quite recently, and the relation of this function to pregnancy, with the various medico-legal questions connected with puberty and virginity, &c.

Conception, utero-gestation, and their effects, local and general; the diagnosis of pregnancy, and of the life and death of the fœtus, with certain diseases and deviations from the ordinary course, occupy a hundred pages, including almost all the observations that have been made on the subject. We were disappointed, however, with the amount of information afforded us on the subject of the action of the fœtal heart as revealed by the stethoscope, inasmuch as nothing is said of the character, rhythm, number of beats, nor of the extent over which they are audible. After enumerating the different opinions which have placed the seat of the placental souffle in the uterus, the placenta, the aorta, &c., the author observes that—

> "Guided by our subsequent experience, and the investigations we have since continued and very lately repeatedly made, we must adhere to the opinion we expressed so early as the year 1833, that the pulsation, with the murmur connecting the strokes, belongs to the arteries and veins of the uterus in the situation of the placenta, and may therefore be called the murmur of the placental region of the uterus." (p. 238.)

As to the *general* signs which are mentioned as indicative of the death of the fœtus, we agree with Dr. Hohl that they are of doubtful value. Sudden and marked changes in the fulness and elasticity of the breasts, or the firmness and tension of the abdomen, are undoubtedly very suspicious, and if persistent would indicate the probability of a dead child; but although these changes, according to our author's experience, never occur with a living child, they may when the uterus is distended by other causes, as in hydrometra. We have, however, seen one case at least, in which the breast became perfectly flabby, and the uterus lost its elasticity and sank down, and in which, moreover, there was a sanguineous discharge from the vagina, yet the infant lived, and was born at the full time. It is very suspicious, also, though not always conclusive, when violent and sudden convulsive movements of the fœtus are followed by stillness and a sensation of dead weight in the lower part of the abdomen. A certain amount of confirmation of our opinion either way may be derived from the fact of the woman having previously borne healthy, strong, and mature children, or weakly, premature, or still-born ones. But, in truth, there is no reliable guide but auscultation, and that not by one examination merely, except when its evidence is positive.

Multiple pregnancy is difficult of diagnosis at all times—impossible at an early period. Some value, but not much, may be attached to the mother's peculiar sensations if she have had children before; something also to the inequality of the abdomen. Dr. Hohl mentions percussion as yielding information of some importance: in single pregnancy, the sound is different on the right and left half of the uterus, because one half is occupied chiefly by the fœtus and the other by the liquor amnii; in double pregnancy, the resonance will be pretty much the same on both sides. The best evidence, however, as Dr. Hohl truly observes, is hearing the fœtal heart equally loud and distinct in two distant parts of the uterus.

But from these and other interesting subjects we pass on to our main object in this review—viz., labour, its varieties and its treatment. After a short section on the "idea of birth," and a comparative view of birth in animals and man, we arrive at the classification, which is based upon—1. The period of pregnancy—i.e., premature, or at the full term; 2. The number of children; 3. The regularity or irregularity of labour; 4. Upon its normal or abnormal course and termination—i.e., its terminating naturally and safely and without assistance, or the contrary.

As to the cause of the commencement of labour, the reason why it takes place at the end of nine months, the primary impulse, &c., we cannot say that our author has thrown much light upon the subject. He enumerates the different causes to which it has been attributed—such as maturity of the placenta, certain conditions of the uterus or its neck, combined action of the uterus and fœtus, reflex action from the spinal centre, congestion caused by the recurrence of a menstrual period, &c. &c.: each of which, as he observes, may be a wheel in the machine, but is not the machine itself. In conclusion, he observes:

> "We now return to the first question we proposed—What are the conditions necessary that the uterus shall contract regularly when the fœtus has arrived at complete development? It must have attained maturity in all its parts—that is, its function as pregnant uterus and receptacle of the ovum must have terminated. It arrives at this limit simultaneously with the maturity of the fœtus. In the termination of this function necessarily lies the commencement of retrogression, by which the waters, fœtus, and after-birth, are expelled. The agents of the retrogression are the muscular fibres which, simultaneously with the parts, have attained their full power. Their shortening lessens the size of the uterus, and, as a necessary consequence, causes a diminution of its cavity and expulsion of its contents: to effect this they need a point of support, which they find in the circular fibres of the inner os uteri, by which they, at the same time, open the uterus for the evacuation of its contents, so that, in a sense, they may be regarded as antagonistic. In pregnancy, it is first the body, then the fundus, and lastly the inferior portion of the uterus which expands; the greatest extension of the circular fibres of the os uteri coinciding with the termination of the functional activity of the pregnant uterus and the development of its motor power, the medium of its retro-

gression, so that it needs but a slight impulse to put the organ into action." (p. 499.)

This impulse the author considers (p. 501) to be the congestion of the tenth menstrual period, occurring at a time when the development of the fœtus is complete, the function of the uterus has terminated, and the increased quantity of blood become unnecessary and accumulating in the uterine vessels. We think the reason given by some of the older writers —that labour occurred at the end of nine months, "*ex naturâ rerum*," or more reverentially, "*ex voluntate Dei*"—quite as satisfactory, and much shorter.

Various opinions, as all know, have been maintained as to the part of the uterus in which the contractions originate: some believing that they commence at the fundus; others, at the cervix; and a third party, that they are simultaneous over the whole uterus. Dr. Hohl considers that they commence at the fundus, because—1. While the pains continue weak, the cervix and os are unchanged; 2. The bag of the waters becomes tense before the cervix is affected; 3. The pulsation of a prolapsed funis is not affected at the beginning of a pain, nor until it has reached its maximum; 4. In abortion, the canal of the cervix is not dilated until the pains are very strong; 5. In turning, the hand in the cavity feels the contraction before the arm embraced by the cervix, and is afterwards expelled by the contractions of the body, without any impediment from the cervix; 6. In a bicorned uterus, both bodies become hard; and in a prolapsed uterus, the author saw the contractions commence at the fundus and extend downward to the cervix—as was observed by Wimmer:

> "What we have hitherto stated forces on us the inference that the contractions, on the whole, and indeed chiefly, begin in the muscular fibres around the openings of the Fallopian tubes, and extend from them to the longitudinal and oblique fibres, which at first find in the former a *point d'appui*, until they act upon the circular fibres of the dilated os uteri internum, which afford them a new support, but then are relaxed and stretched on all sides, until through them the external os uteri is dilated. The more gradually the muscular fibres contract, the more powerful the pains become, so much the more rapid is the extension of the contraction from above downwards; and when, finally, the liquor amnii has been discharged, and the cervix has been retracted to the margin of the body of the uterus, and the circular fibres of the inner os uteri have become a sort of sphincter, we think we shall not err in assuming that now the increased irritation of the latter is a cause of increased effort, as shown by the patient 'bearing down.' Lastly, it would appear to us that a continuous contraction does not exist, but that each pain is composed of lesser contractions and expansions rapidly following each other." (p. 505.)

The usual distinction is made between regular and irregular pains, and the first and second stage of labour, and we agree with the author's limitation of Dr. Simpson's proposition—that the danger is in proportion to the length of the labour—to delay in the second stage. Dr. Hohl also doubts, so far as his experience goes, the correctness of Dr. Simpson's conclusions as to the effect of the sex of the child upon the duration of labour.

There is nothing to detain us in the description of the different stages of labour; the phenomena are enumerated pretty much as in other systematic works; but we cannot agree with the author, that the separation of the placenta is effected by the pains which return after the birth of the child—the separation being, in our opinion, in almost all cases, the result of the pains which expel the child.

The mechanism of labour is minutely and accurately described, based upon the work of M. Naegelé, but as there is little of novelty, we shall not dwell upon it, but rather proceed to the practical parts of the volume. Natural labour is divided into two classes and two orders:—Class I. Head presentations: Ord. 1. Vertex presentations; Ord. 2. Face presentations. Class II. Breech presentations: Ord. 1. Breech alone; Ord. 2. Breech complicated with foot or knee. So far as the practical management of natural labour is concerned, the advice given is very judicious, nor does it differ from our own, except on one or two points which we shall notice. Dr. Hohl prefers a separate couch for delivery, such as we see in some of our lying-in hospitals, but which we think would be very objectionable in private, on account of the increased chance of cold; and it is surely unnecessary if proper precautions are observed. Again, whilst admitting that the patient may lie on her side for delivery, Dr. Hohl expresses his own preference for the position on the back, as is practised most commonly on the Continent. He very properly objects to the membranes being ruptured before the os uteri is fully dilated and the presenting part descending; but he advises that, if the presentation be not within reach when the os uteri is fully dilated, the hand should be introduced at the time of puncturing the membranes, so that if the presentation should be preternatural we may more easily and at once proceed to deliver. Lastly, the usual directions for supporting the perineum during the passage of the head and shoulders are laid down, but followed by a statement that Dr. Hohl has for some time directed his efforts to the head rather than to the perineum:

> "We are fully convinced," he says, "that in supporting the perineum with the hand, in consequence of the attenuation and enormous distension of the former, the hand, in the moment of greatest danger—that is, when the perineum is retracted over the greater circumference of the head just as the head is pressing through—is no longer applied to the perineum, but to the head of the child, the perineum having escaped backward. This we daily see, even when the greatest care is used. For this and other reasons we have, for our own part, long given up supporting the perineum, and only support the head, endeavouring to bring its smallest diameter into and through the vulva. In this we aim only at assisting the mechanism we observe in the natural course of events. During the passage of the head through the vulva, we accelerate the rotations if a greater danger than ordinary threatens the perineum, at the same time we remove the pressure of the head from this part." (p. 582.)

We must say that we neither agree with Dr. Hohl's theory nor practice; we think the left hand, placed obliquely across the perineum, will afford a true support to that part, and it is the most convenient, as it leaves the right hand free; and we doubt the utility of our author's plan.

The reader will be glad to hear the opinion of so distinguished a man upon the subject of chloroform:

> "If we compare," he remarks, "the great number of cases which have been communicated, and which, according to the reports, have passed over without any injurious results, with the smaller number which have had an unfavourable termination in consequence of the employment of chloroform, some might be inclined to record an unqualified verdict in favour of this anæsthetic. But we are not of this opinion, for the fatal termination of a *single* case, in which the remedy was used

> neither in excessive quantity nor incautiously, is sufficient to deter us from its use if it can be dispensed with, and if it is not required for the attainment of a definite object. In surgery such an object exists, but not in midwifery, in regular natural labour. Here chloroformisation is an invasion of a normal function, which is attended with pain, which latter cannot be considered as pathological. While, therefore, for our own part, we repudiate the employment of this means in healthy parturition, we do not mean to exclude its use in some obstetrical operations. To the beginner we would give the following advice, if he determines to employ chloroform in natural labour:—Let him not make use of the vapour when the patient has eaten shortly before; when the labour is an easy one, and the woman has on all former occasions had easy and favourable labours; or if she be prejudiced against its employment, or suffer from an organic affection of the heart or lungs. Let him avoid its use in all cases where the undisturbed action of the uterus, and the uniform power of the organ, are necessary for the completion of labour. It will be wrong, therefore, to employ chloroform when the pelvis is narrow or when the head is impacted. If the patient be much agitated in the first or second stage of labour, in consequence of severe and rapid pains; and if it be desirable to procure her some rest; or if it be wished to moderate the expulsive force when the vulva is narrow and the perineum unyielding, chloroform may come into operation. Let the practitioner, however, take care that atmospheric air be inhaled at the same time; let him not continue the inhalation uninterruptedly, in order that the patient may not be permanently in a state of total unconsciousness. We have also made use of chloroform with some unruly patients who were not amenable to entreaty, who unnecessarily flung themselves about, or were unmanageable during an operation: for such, chloroform is an efficacious means of quietness." (p. 578.)

We have already stated that our author includes under the head of natural labour, not merely head presentations, but those of the breech and inferior extremities. It is not of much consequence, but we should prefer to place the latter among the deviations from natural labour, not only on account of their less frequency, but because they involve necessarily a considerable amount of danger to the infant; and the type of a natural labour should have no necessary risk for mother or child.

Dr. Hohl refers his two classes of dystocia to three causes:—1. Abnormal state of the expulsive force; 2. Abnormal conditions of the passages; and 3. Abnormal conditions of the child; and the sections upon these subjects are, we think, the most carefully elaborated in the book. They embrace a wider range of subjects than most English works of the same class, but on all essential points there is but little difference. Under the first division we find described "too weak and too strong" pains, with a notice of abnormal pains through spasmodic or tetanic action of the uterus. The effects of excessive pains are thus described:

> "The consequences of too strong pains are, therefore, a too rapid transition from the first to the second stage of labour; a too speedy separation of the child from the mother, in consequence of violent and continuous compression of the placenta; a disturbance in the circulation of the blood through the vessels of the funis, which, in consequence of the early discharge of the waters, is pressed against the fœtus; injuries of the fœtus, from its being violently pressed against the bones of the pelvis, particularly when the fœtus is large, or the pelvis, without being too narrow, belongs to the smaller class; more violent injuries of the same, and death, when the pelvis is narrow, which also may occur if the pelvis is too large, or when regular, if the fœtus is small, because the latter is too rapidly separated from the mother, and exposed to external impressions. In this case, also, the fœtus may suffer a fall on the ground. A sudden exhaustion, and even paralysis of the uterus, may follow too strong pains. Various other dangers, moreover, threaten the

> mother—as rupture of the uterus, when the passages or the child are too resisting; laceration of the vagina and perineum, and separation of the bones of the pelvis; evil consequences from congestions and nervous attacks; great exhaustion; hæmorrhage during and after delivery; displacement or paralysis of the uterus when the resistance is too great, and which may become general, and cause the death of the mother." (p. 607.)

It is not easy, nor always possible, to control the excessive action of the uterus, and in many cases it is unnecessary. Dr. Hohl seems to rely chiefly upon tartar emetic, laurel water, and chloroform.

The usual remedies against too weak pains are enumerated. Castoreum, borax, ergot of rye, Indian hemp (?), electro-magnetism, cold to the uterus, and irritation of the breasts; but we gather little information, either new or the result of the personal experience of the author.

The list of impediments to the passage of the child is unusually full, including all of great moment, and some whose influence would be but trifling. Thus we have noticed in sufficient detail, rigidity of the perineum, œdema of the vulva, varicose veins and thrombus of the labia; inflammation and swelling of the vulva, swelling, varicose veins and sanguineous infiltration of the vagina; fibrous, lardaceous, fungous, and scirrhous growths; calculus in the bladder, vaginal cystocele, &c.; and Dr. Hohl mentions a case in which he felt the left kidney pressed down to the pelvis during labour. On the part of the uterus, obstacles may arise from swelling and hypertrophy, from the closure or rigidity of the os uteri, from polypus, from tumours in the uterine walls, and from malposition or mal-direction. Short and practical directions are subjoined for the treatment of these affections, so far as they are under our control.

The difficulties offered by a distorted pelvis are, we think wisely, divided, according to the amount of obstruction, into those which admit of the birth of a living child without artificial assistance, those which may be overcome by the forceps, those rendering perforation necessary, and such as involve the necessity of the Cæsarean section. With regard to the first and most difficult class of cases we shall let our author speak for himself:

> "In the first degree, it is said to be possible for a full-grown child to be expelled by the labour pains, but not without injury to the mother or child, or both. Here the chief stress is laid upon the unassisted action of the natural powers, and on the injury to mother or child. But we know, in the first place, that, even in considerable narrowing of the pelvis, for example, where the conjugate diameter is three and a half and three inches, births take place by the natural powers, without injury to mother or child, as the cases by Solayres, Lachappelle, Martin, and Dubois, demonstrate. Busch speaks of 'frequently-observed instances of limitation of the pelvic space (conjugate three inches and a half), where nature terminated the labour without injury to mother or child;' and he communicates a case where, in a primapara, with a strongly curved vertebral column and a rhachitic pelvis, the conjugate diameter of which measured three inches, the birth of a living child took place without artificial assistance. Credè reports the safe delivery of a strong living child by the efforts of nature, where the conjugate diameter measured scarcely three inches. To these cases we can, from our own experience, add several; for instance, in particular, one in which, with a conjugate diameter of three inches and a half, and a slight distortion of the lumbar vertebræ, a strong child (eight pounds and a half) was born by the natural efforts, without injury to itself or the mother. But we must also, in the second place, object that it is not on the degree of narrowness of the pelvis alone, even if this be moderate, that the

possibility of expulsion by the efforts of nature depends; it also, and very specially in these cases, depends on the strength of the pains, the size, position, and condition of the head; so that it is possible that the labour may run a natural course, and be unattended with injury to the mother or child, and yet require artificial aid. It is not possible, therefore, always to draw a marked line of distinction between this and the *second degree* of constriction, in which the head indeed reaches the brim or cavity of the pelvis, and there remains impacted. We may observe, that in constriction of the outlet the head may pass through the brim, and be arrested in the cavity. Lastly, a *third degree* of narrowness of the pelvis is laid down, where the head cannot at all insinuate itself into the brim. It is true that a constriction to this degree may exist, but it is also true that when the head is large and unyielding, it may, even with a less degree of constriction, remain fixed at the upper aperture; while cases are not wanting in which delivery unexpectedly took place just as the Cæsarian section was about to be performed." (p. 665.)

The remedy for each degree of deformity above the first is, of course, some form of artificial assistance; and these form a series of chapters on operative midwifery, which we shall next cursorily notice. It hardly seems to us necessary to have given distinct sections to such operations as dilatation of the os uteri or puncturing the membranes, especially when in the first are involved recommendations of very doubtful merit, while much that is interesting is omitted. We are glad to find, however, that Dr. Hohl has lent the weight of his authority in favour of the artificial induction of abortion in proper cases, for which it has always appeared to us, that the arguments are as strong as for the artificial induction of premature labour. The cases for which he recommends it are the total retroversion, depression, or prolapse of the uterus; rupture of the uterus in the early months, dangerous hæmorrhage without expulsion of the ovum, fibrous growths in the pelvis which render the passage of a viable child impossible, extreme distortion of the pelvis, cancer of the uterus, &c. He does not think it advisable in ovarian enlargement nor in hernia; but we cannot agree with his rejection of it in the incessant and extreme vomiting of pregnancy, because we know that lives have been saved by it; nor in extreme narrowing of the vagina, if this be the result of cicatrices, as in Dr. Oldham's case.

The object of artificial premature labour is to save the lives of mother and child, and the special case suited for it is distortion or obstruction of the pelvis, not too great to permit the passage of a viable child. But, in addition, there are certain diseases of the mother, and certain affections of the contents of the uterus—as dropsy of the amnion, for instance—in which our author admits its propriety, as well as in those cases mentioned by Denman, where a succession of children are born dead after the seventh month.

The usual means of inducing labour are enumerated: rupture of the membranes, irritation of the cervix uteri by sponges, dilatation, plugging the vagina, the water douche, internal stimulation of the uterus, warm injections into its cavity, external friction, sympathetic irritation of the breasts, or, lastly, the administration of ergot and the application of galvanism. As to their comparative merits, Dr. Hohl observes:

"We would close this sketch with the remark, that puncturing the membranes, the sponge-tent, plugging under certain circumstances, and the uterine douche, can alone be regarded as valuable means of inducing premature labour; that the three latter may either be of themselves sufficient, or may require in addition the puncture of the membranes, as the latter, in turn, may require the former as preparatory; i.e., it is advisable, in cases in which the puncture of the membranes cannot be immediately employed, to commence with that one of the three other methods which may appear most suitable in the particular instance, and that no other procedure can as yet be considered an effective method of inducing premature labour. The accessibility of the uterus may give rise to novel proceedings, and one might perhaps, after the old fashion, excite the womb through the abdominal parietes by a succession of moderate blows, or with the so-called 'Topfsetzen' (literally, Pot-putting), of the Russians, or perhaps instead of the injection of water and aq. Picea, we might blow air into the uterus." (p. 923.)

The chapter on turning is a good one: of course, the usual division into cephalic and podal version is made. The grounds for the latter are, 1. Mal-presentation; 2. Mal-position of the head, which cannot be otherwise rectified; 3. Prolapse of the funis, if the head be high and the soft parts dilatable; 4. An extremity descending along with the head, and re-position being impossible; 5. A certain degree of contraction of the pelvis. In the latter case, Dr. Hohl observes:

> "However, it is an indispensable condition, that the proportion between the head and the pelvis be accurately ascertained. An absolute amount of narrowness of the pelvis cannot be laid down as the condition, for the size of the head may forbid turning where the conjugate diameter is three and a half inches, and may prevent it when it does not exceed three inches. The form of the pelvis is also to be taken into consideration, and especially the relations and direction of the promontory of the sacrum. Thus, it is evident that the head will be more easily brought into and guided through the larger half of an obliquely narrowed pelvis after turning, than when the head comes first and the forceps is used. The inclination of the pelvis also is so far not a matter of indifference, inasmuch as too great an inclination, according to our observations, renders the entrance of the head into the narrowed upper aperture more difficult. We therefore earnestly advise the beginner to be extremely cautious where this inclination exists, as an error will impose on him a difficult operation; for if the head cannot enter, the child will be lost, and the mother be placed in great danger. As a matter of history, we must observe that turning in narrowness of the pelvis found opponents in Stein, sen., Boer, and some others; while it is defended by J. F. Osiander, Ritgen, Trefurt, Siebold, Naegelé, jun., Münch, and Simpson. The latter, as Chailly-Honoré has remarked, goes too far, for he is said to have turned by the feet when the conjugate diameter was only two and a half inches. But Seyfert's view is erroneous, who, in noticing that article, expresses his astonishment that the old idea of turning by the feet in cases of narrow pelvis with head presentations, which he had looked on as given up, should find such warm defenders in Simpson, Scanzoni, Arneth, and others." (p. 929.)

On the other hand, version, according to our author, is counter-indicated, first, by too great narrowness of the pelvis; second, by too small a child; third, by pathological closure of the os uteri; fourth, by the head being tightly jammed against the pelvic brim; and fifth, by great weakness and exhaustion of the mother. The time best suited for the operation, and the mode of operating, do not essentially differ from those laid down in other systematic works, except that the author gives the alternative of a position on the side, back, or knees and elbows, which latter, he says, is more convenient when the pelvis is very oblique.

The next operation in order is perforation, when the pelvis is too small or the head too large, or where the child is hydrocephalic: we cannot but believe that in practice the author must have found other cases, unless he makes a freer use of the forceps than we are accustomed to do in this country.

The amount of distortion rendering this operation necessary is pretty well settled, theoretically, but it is not so easy to decide practically. When the conjugate diameter is under three inches, a living child can rarely pass, with or without assistance. Dr. Hohl fixes upon from two and a quarter to two and a half inches as involving perforation; but aware of the difficulty of the subject practically, he gives some very sensible advice, which we shall quote.

> "Perforation can only be decided upon during labour, for it cannot be previously determined whether it will be necessary or not. The first guide to our conduct must be an accurate unbiassed observation of the process, and particularly of the relations between the head and the pelvis, especially the amount of yielding shown by the former, and the mode of its entering the brim of the pelvis. When the results of examination and observation show that perforation is *really* indicated and *must* be performed, then the employment of the forceps is no longer *harmless.* We have just observed that the results of our examination of the labour afford the basis for our action. Experience has shown that in narrowness of the pelvis the head frequently accommodates itself to the contraction and form of the pelvis, and is either driven by forcible uterine action through the latter, or may be extracted by the forceps. The accoucheur will consequently be so much the more called on to try the forceps before perforation, as it essentially assists the accommodation of the head. We therefore quite agree with the sentiments expressed by Busch, in relating a case of delivery of a rhachitic person, the conjugate diameter of whose pelvis was three inches, and which he terminated successfully with the forceps, 'that in many cases of even considerable limitation of the pelvis, perforation may be avoided by careful and persevering efforts with the forceps.' The cautious obstetrician will soon ascertain whether the forceps will or will not be sufficient, and he will at the same time obtain information as to the hardness and size of the head. But he will arrive at this conclusion, not by measuring the force with which he operates, not by estimating the pulls he makes, but, where he wields the instrument aright, by the result; and this he recognises, we might say, in his hands, in his arms, but especially by examining, after a number of tractile efforts, whether the head seems to be yielding or not, whether it alters or preserves its form, whether it retains its immobility or becomes somewhat more movable, whether it follows, at least during traction, or only recedes when this has ceased." (p. 968.)

We do not, however, agree with the author, that the death of the child is an essential pre-requisite of the operation. Many circumstances may prove that a living child cannot be delivered; and once we are assured of this, a due regard for the safety of the mother requires that perforation should be performed. In all such cases the child is virtually dead, but we *may* save the mother. No one chooses between the life of the mother or child; there *is* no choice. But though we cannot save the child, we may, by anticipating the period of its natural death, rescue the mother. Nor do we consider that the child's being alive is an argument for the Cæsarean section, unless the contraction of the pelvis is so great as to render it questionable whether a mutilated child can be extracted with safety to the mother.

We think that we might advantageously adopt the trephine, which is used in Germany for opening the head, in certain cases where the scissors are inefficient.

We have often been struck, in looking over the statistics of German lying-in hospitals, with the greater frequency of forceps operations, and the rarity of perforation, compared with the statistics of hospitals in these countries; nor have we found any adequate explanation in Dr. Hohl's volume. We can understand the reluctance to perforate; but even if we admit that a certain number delivered by the crotchet might be delivered by the forceps, this would be far from accounting for the great excess in which the latter instrument appears to have been used. On this subject Dr. Hohl affords us no assistance; and we must say, that one of the greatest defects in his volume is the utter absence of numerical details.

The forceps used, or at least figured, by Dr. Hohl, is one with the double curve, and answering to the ordinary long forceps of these countries; and he has laid down very minute directions for its application in ordinary cases, and when the head is in an abnormal position. The grounds of the operation are—(1) to increase the efficiency of weak pains; (2) to supply their absence; (3) to rectify mal-positions; and (4) to hasten the termination of labour, if the condition of the mother or child require it. As the determination of these conditions is a very important point, we shall let Dr. Hohl speak for himself:

"In this respect the forceps is indicated, 1, *for the relief of the mother*—(*a*) when the patient, in consequence of the protraction of the labour or of painful throes, is in a state of considerable excitement; (*b*) when she is phthisical or suffers from hæmoptysis or hæmatemesis, and it appears unadvisable to expose her too long to the fatiguing effects of the expulsive pains; (*c*) when circumstances exist which are aggravated by the efforts made in using the auxiliary powers, or which may bring the life of the patient into danger of a sudden termination—as, for example, goitre, hernia or strangulated hernia, aneurism near the heart, cardiac disease, distension of the bladder which cannot be relieved by the catheter; (*d*) violent hæmorrhage from the genitals, rupture of the uterus or vagina; (*e*) convulsions. We may here observe sometimes, when the head is in the pelvis, the patient's gazing about strangely, or giving a squinting look, or suddenly becoming dumb, and the occurrence of a longer interval between the pains, precede an attack of convulsions. If we then quickly have recourse to the forceps, we may avert the attack, or perhaps a fit may occur during the operation, and none, or but slight ones, after delivery. (*f*) A tumour, especially if ovarian, opening into the vagina; (*g*) when the placenta is situated near the os uteri, and the external hæmorrhage having ceased in consequence of the descent of the head, internal hæmorrhage is to be feared; (*h*) when the anterior lip of the os uteri begins to swell from pressure between the head and the pubis, or when already swollen and incapable of being pushed back, or when pushed up it again falls down. The elevation of the lip may often be accomplished at the commencement of the tumefaction, and even at a still later period, by pushing it upwards with the fingers from either side alternately during the interval of the pains, and by supporting it with the points of the fingers at the moment of their recurrence. Particular caution is necessary in extracting with the forceps, of which we shall speak in treating of the special cases. It is also judicious to complete the delivery with the forceps when the anterior wall of the vagina is pressed forward under the arch of the pubis.

"The forceps is indicated, 2, *for the sake of the child*—(*a*) when auscultation shows that the impulse of the fœtal heart, from some internal cause not always to be recognised, is weak or intermittent, or, in short, when it exhibits any considerable alteration; (*b*) when, the head being situated conveniently for the application of the forceps, a tumour of the head forms, which either rapidly increases or has already attained an unusual size. We cannot avoid considering these indications to be adequate, when such a cause exists as to delay the termination of the labour, even when the delay does not of itself demand the use of the forceps. We know well that many children are born healthy and sound, with considerable tumours of the head; but we have also found many a child still-born, on our arrival, with a large swelling on the head, whose death occurred at the outlet of the pelvis, because the tumour prevented the exit of the head. The tumour oftentimes increases unexpectedly and rapidly, so as to impede the subsequent application of the forceps, as we shall show in considering the special cases. If, therefore, during delivery we are anxious to avert danger from mother and child, we must anticipate that which threatens. (*c*) In prolapse of the funis, which cannot be replaced, and when the funis is drawn over the head and lies close to it, whether in either case it pulsate or not, or when in the latter case it must be divided; under such circumstances it is judicious to apply the forceps before the division; (*d*) when the mother labours under syphilis, even though we may not believe in the possibility of the child's being infected in passing through the parts; (*e*) in prolapse of the placenta, when the head at the same time descends directly into the pelvis; (*f*) when the mother dies suddenly during delivery. (The question, whether after the breech has been born, it is preferable to extract the head with the forceps, we shall consider in chap. viii.) It is, lastly, to be observed in addition, that when the placenta is attached near the os uteri, as well as in hæmorrhage from partial separation of the placenta situated normally, the hastening the delivery with the forceps is undertaken out of consideration for the child.

"The following are the counter-indications to the use of the forceps:—1. When the head is too high up, and not yet prepared. 2. When the head is too large and the pelvis too narrow. Of this we have already partly treated when speaking of perforation, and now merely repeat, that a fixed limit of the pelvis, up to which the forceps may be used, cannot be assigned, as it must essentially depend upon the size and yielding of the head. A conjugate diameter of three inches has usually been stated as the extreme limit; but an adherence to this standard, without reference to the head, would lead to inexcusable mistakes. 3. Hydrocephalus, when the head is large, as the forceps will slip off. 4. Hemicephalus, when the grasp is too small for the forceps.

"Scanzoni, with reference to the position of the fœtal head, considers the forehead turned towards the pubis, or to the side of the pelvis, among the indications for using the forceps, which we cannot consider as alone conclusive." (p. 991.)

We have given this long extract, not because we agree with all the propositions it contains, but as illustrative at once of the views held in Germany, and the Professor's manner of treating the subject. The same fulness, the same subdivision, and the same kind of classification, prevail throughout the book. Although we may not agree in opinion on some points, it cannot be denied that the different points receive a very full investigation. Did our limits permit, we should gladly enter upon the remaining operations and the chapters upon diseases of childbed; as it is, we trust that we have enabled our readers to judge in some degree of the nature of this learned work; and we trust that our brief notice may induce many to read it for themselves.

Let us now turn for a few moments to Dr. Bedford's volume. It consists of Thirty Lectures on Diseases of Women and Children, delivered at what the author terms "the Obstetric Clinique" in New York, established by him in Oct. 1850, since which time 8000 cases have been treated, and instruction given to the pupils upon them, as they presented themselves. Unquestionably, the lectures must have been of essential use to the class of students; nor are they of small value in the volume before us; but we cannot but think that the little bits of oratory and the dialogues between the doctor and patient had better been omitted. The latter is a novelty introduced by Dr. Meigs, and may perhaps give an amusing character to a dry subject; but we do not think that they either elucidate the questions or are quite in keeping with the importance of the subjects, while they certainly add to the bulk and expense of the volume. Another drawback to the value of the *published* lectures is the fact, that we learn little or nothing of the results of treatment. The author prescribes what he thinks best according to his experience, and it may be the very best treatment; but of the effects in the particular cases we rarely are told. Notwithstanding these objections, however, the book is a very valuable record of cases as they occurred, without classification; there are, in addition, some excellent short dissertations on the different diseases under consideration. We are quite sure that the work will be a welcome addition to professional libraries in Great Britain as well as America. The desultory character of the work precludes anything like a systematic review; but we may say generally that the reader will find cases and observations on diseases *of the vulva*, such as warty excrescences, abscess of the labium, pruritus, hypertrophy of the nymphæ, occlusion of the orifice of the urethra, vascular tumour of this part, &c. &c.; on diseases *of the vagina*, including some admirable remarks on leucorrhœa, warty growths, pruritus, occlusion, prolapse, ulceration, &c.; diseases *of the uterus*, both functional and organic, impervious orifice, ulceration, enlargement, induration, fibrous tumours, polypus, displacements, &c.; diseases *of the ovaries*, enlargement, dropsies, &c.; with some judicious observations and instructions on the diseases of children.

We shall endeavour to select a few of the more interesting subjects, and present them to our readers, in order that they may judge how far the foregoing opinion is justified.

The first case is a curious example of neuralgia of the right labium externum in a married woman, from which she had suffered for six months. It was very tender to the touch, and sexual intercourse was unendurable, yet the labium was neither enlarged, nor discolored, nor inflamed; menstruation was regular, digestion good, and no evidence of any disorder of the general health. But there was pain on pressure of the sides of the upper lumbar vertebræ, and the Professor regarded the case as an example of lumbo-abdominal neuralgia, which he treated by an issue. (p. 443.)

A case of stricture of the female urethra is given at page 79. It is a very rare disease, the Professor having only seen this one case. Velpeau mentions three cases. The treatment consisted of dilatation by bougies, and was successful in three months.

We quite agree with Dr. Bedford, that for the vascular tumour of the meatus urinarius—

> "The only remedy is the removal of the tumour; this may be done by ligature, the knife, caustic, or the scissors. I greatly prefer the latter. Take a pair of curved scissors, and remove the tumour completely, then touch the cut surface freely with caustic. This is all that will be necessary." (p. 65.)

There is a case of "inversion of the mucous membrane of the urethra," which might easily be mistaken for vascular tumour, if a little care were not taken. Upon examination, it was found that the cause of difficulty in micturition "consisted in a prolapsion, or perhaps, more properly speaking, an inversion, of the mucous lining of the urethra," and also that "the mucous surface was ulcerated."

> "The distinction between the two affections is so simple, that an error in diagnosis cannot be justified. In the latter disease, as you know, there are usually three characteristic symptoms: 1, excessive sensibility; 2, extreme scarlet redness; 3, bleeding on injury. All these symptoms are absent in the present case."

There are some very judicious remarks upon the disorders of menstruation scattered through the volume; the author very properly lays great stress upon the treatment being varied, according to the causes, and that in many cases of amenorrhœa the use of emmenagogues is most injudicious.

Dr. Bedford records several cases of amenorrhœa from imperforate os uteri, in which he removed the obstruction, and the patients recovered. In one case, the closure of the mouth took place after conception, from injuries inflicted in the attempt to procure abortion. The profession and the public of New York are deeply indebted to the Professor for exposing the iniquitous practices which the history of this case revealed to him. In another case, the os uteri was closed by an inflammatory attack subsequent to miscarriage. Having ascertained the nature of the case, and the operation necessary, the following dialogue is reported:

> "'Madam, are you aware of the difficulty under which you labour?'—'Yes, sir, I have heard you say that I have an obstruction.' 'That is a very proper word, my good woman. Do you wish to have the obstruction removed?'—'Oh, sir, I would, if it is not dangerous.' 'There is no danger, madam, if the operation be rightly performed; and if you will consent, I will perform it without any further delay.'—'You are sure, sir, it wont kill me?' 'Indeed I am, my good woman. We do not kill people—our profession is intended to save, and not to destroy human life.'—'But, sir, people do die in spite of the doctors.' 'Yes, madam, that is true; there is a limit to all human skill, and it sinks into insignificance before the high decrees of Heaven! Will you permit me to relieve you?'—'Anything you say, doctor.' 'Then, madam, I will do what is right for you.'" p. 321.

Now, without questioning that this little flourish of trumpets may have been very consolatory to the patient, we do not think that the insertion of such dialogues in every two or three pages is peculiarly profitable to the reader.

A case of catalepsy, arising from suppressed menstruation, with uterine engorgement, is related at pp. 331—378. The patient had menstruated once at the age of seventeen, but not afterwards, and the first fit occurred three weeks after marriage. The treatment consisted, not in the administration of emmenagogues, but in diminishing the uterine congestion by a dozen leeches applied to the vulva, and by the exhibition of twelve grains of calomel, with one of ipecacuanha, at night, and an ounce of castor oil in the morning—rather a large dose, certainly. The leeches were to be repeated at the next monthly period, the bowels kept free, and only a vegetable diet allowed. The result of this treatment was very successful.

We find four cases of vicarious menstruation recorded; in one, the discharge was from the nose, in another from the stomach, in a third from the umbilicus, and in the fourth from the rectum. The treatment was by brisk purgatives, abstraction of blood at a menstrual period, and iron. In one case only are we told the result. We must not omit to notice an interesting example of menstruation occurring with perfect regularity in a woman five months pregnant. In mentioning the proofs of pregnancy, we were rather surprised at the omission of the most certain—viz., the audibility of the fœtal heart.

There is a short dissertation on sterility at page 535, in which the subject is treated in an able and sensible manner, and cases illustrative of the various causes and the different treatment are scattered through the volume. The one which immediately follows is a good example of sterility caused by stricture of the canal of the cervix, and cured by gradually dilating the passage by bougies, quite as satisfactory in many cases, and much safer in all, than incision.

The author relates an interesting example of physometra, occurring after the birth of a putrid child. We are not told how the os uteri was closed, but the symptoms were those usually observed—viz., defined form of the tumour, which remained the same after the bowels had been well freed; resonance, and suppressed menstruation. The treatment consisted in introducing a silver tube into the uterus, through which an escape of fœtid gas immediately took place; the exhibition of mercury, so as to produce ptyalism, and a prolonged course of sarsaparilla.

Lecture IX. opens with a description of the uterus, its coats, blood-vessels, nerves, &c., from which we quote the following observations, which are not without especial interest at present:

> "The uterus is contained within the pelvic excavation, supported below by the vagina, having in front the bladder, with the bas-fond of which it is connected at its inferior third; posteriorly, the rectum, between which and the posterior surface of the uterus is the triangular fossa; and above, in front and behind, the small intestines. These are the respective relations of the unimpregnated womb; its long axis is slightly oblique from above downward. The question now naturally arises, is the uterus an organ which enjoys a great degree of mobility? You will find, gentlemen, that there are few organs in the system which possess this property to a greater extent. . . . You see, therefore, that the uterus is characterized by great mobility, resulting frequently in displacements, some of which are transitory, whilst others are more permanent, calling for the interposition of science. When you consider the numerous causes of uterine displacements, more or less constantly in operation, together with the peculiar offices of the uterus itself, you cannot regard this mobility of the organ in any other light than as a conservative act of nature." (pp. 141, 142.)

No doubt many of the causes are external to the uterus, and perhaps in some of the displacements they are always so, but we regret that the author has not given due weight to the peculiar conditions of the uterus itself, which act as very influential causes. One case of anteversion is given, and it is interesting, as being the result of excessive constipation occurring within a month after childbirth. The symptoms were a

> "Severe bearing down pain in the back passage, with a frequent desire to pass water, but an inability to void more than a small quantity at a time; she is labouring under obstinate constipation, sometimes passing a week without an evacuation, and then, after excessive straining, she is only able to pass a small piece of hardened fæcal matter."

On examination, Dr. Bedford found "the fundus uteri pressed forward, pressing upon the bladder, whilst the cervix was turned in an opposite direction," and the rectum was "greatly distended with lumps of hard fæcal matter." Immediate relief was afforded by what the profession facetiously termed "a fundamental operation"—viz., digging out the hardened fæces with a spatula, followed by the exhibition of cathartics.

There are two cases of retroversion in which the author, seemingly doubtful of any benefit from the intra-uterine pessary or the rectal tampon of M. Huguier, had recourse to M. Amussat's method, which consists in applying the *potassa cum calce* to the posterior lip of the uterus, and to the corresponding portion of the posterior wall of the vagina, in order to procure adhesion between the two. In one of the cases the result is stated to have been decided success.

The following are stated to be the causes of this displacement:

"A deformed pelvis with an increased capacity; undue pressure of the viscera, particularly the distended bladder; falls, blows, &c.; and I can well imagine how that ridiculous contrivance of fashion—the destructive corset—by its pressure from before, backwards, below the umbilicus, may act as a cause of this displacement."

Surely, we have here an omission of the most important and most frequent cause; as far as our experience goes, we have found some condition of the fundus uteri, rendering it, so to speak, top-heavy, to be far the most general and most influential in its production; and it appears to us that so long as this condition continues, the treatment should be directed to its removal, rather than to the mechanical rectification of the displacement.

Several cases of prolapsus uteri are recorded, from various causes, both external and internal, and here the author very properly makes a clear distinction in the treatment. For example, in one of the cases dependent upon engorgement, he remarks:

"One of the commonest effects of engorgement of the cervix uteri is prolapsion of the organ; and you can very readily, I apprehend, understand why this result should follow. Under the influence of engorgement, the uterus becomes increased in volume, and consequently in weight; this increase of weight necessarily causes the organ to descend more or less into the vagina, and thus the prolapsion is produced. Do you not, therefore, at once see the absurdity of introducing in a case of this kind, the pessary? This instrument can exercise, under the circumstances, no curative effect, but will tend directly to a general aggravation of all the morbid conditions,—it becomes a source of irritation to the engorged surface, thus inviting an increased afflux of fluids to the part, and thereby augmenting the supply of morbid elements. Its tendency also is, by pressure, to produce ulceration. . . . From what has just been said, it is plain that the prolapsion in this case is merely an effect, while the true cause is the engorged condition of the cervix. The indication, therefore, is to let the prolapsion alone, and to direct all our efforts to the removal of the engorgement." (p. 442.)

We had marked other subjects for notice which possess great interest—diseases of the ovaries, some of the accidents and operations of child-bearing, together with some very important cases of infantile disease, with the varied experience of Dr. Bedford thereupon, but for these we must refer our readers to the volume itself. We would respectfully advise the author to classify the subjects in his next edition, or at least to give a classified table of them; to curtail or abolish the dialogues; and to give, if possible, the results of his treatment, and then will the work be worthy of its author, a credit to its country, and a valuable mine of instruction to the profession at large.

Fleetwood Churchill.

Review IV.

Quarante Années de Pratique Chirurgicale. Par Ph. J. Roux. Tome Premier, *Chirurgie Réparatrice.* — *Paris*, 1854. pp. 474. Tome Second, *Maladies des Artères.*—*Paris*, 1855. pp. 476.

Forty Years of Surgical Practice. By Ph. J. Roux. Vol. I. *Reparative Surgery.* Vol. II. *Diseases of the Arteries.*

Vol. I.—Plastic Surgery? Autoplastics? Heteroplastics? Anaplastics? Neoplastics? Morioplastics? Decorative Surgery? Chirurgia Curtorum? Clinical Orthopædics?—We do not know but that Roux's simple phrase, "Reparative Surgery," is more expressive, as it is more euphonious, than all the names by which this great and still somewhat novel branch of surgical science and art has been designated. It is certainly no less comprehensive, since it enables him to group under one title as much as any, and more than some, of those high-sounding denominations include. It describes a domain of surgery which has of late years extended rapidly and far, and with which, whatever further advances may be made, the name of Roux will ever remain honourably associated.

The volume before us is the first of four which the author had the intention of publishing, if his life and health were continued to him. He died, however, when he had revised the proof sheets of about the half of his first volume; and it is due to the estimation in which he was held by the Parisian Society of Surgery, that his work has so far been completed and published. The present volume is complete in itself, and the others are to contain articles on Difficulties and Errors in the Diagnosis of Surgical Diseases, and on the Accidents which may arise during the Performance and in the Sequel of Operations. The author's experience for more than forty years in the Charité and Hôtel-Dieu ensures the value of his writings on such subjects as these.

There is some peculiarity in the manner in which he has performed his task. The work is in the epistolary form, and the friend whom he has selected from amongst illustrious foreign surgeons for the dedication of the first volume, is our own fellow-countryman, Mr. Lawrence. The appeals to "mon cher ami," and "mon cher Lawrence," are sometimes merely quaint, but at other times they serve for the confirmation of facts of which Mr. Lawrence was cognizant.

The author's fame as a surgeon is chiefly connected with the operations for cleft palate and ruptured perineum; and his essays on these subjects form the most valuable in the book. The remainder of the work is occupied with a long preliminary description of the various modes of performing reparative operations, and with notices of his experience in the reconstruction of the various organs of the face. We have neither space nor need to refer to his earlier letters on 'Généralités,' or to the operations required by all the several deformities. He is giving his friend the results of his own experience, without always regarding how much of what he writes has by this time ceased to be new, and we shall be excused from following him into many matters which he pursues with much prolixity. Still, the records of his experience on most of these matters are both profitable and interesting, and will repay perusal.

He furnishes no new information on the restoration of the pinna of the ear; and the extensive subject of the reconstruction of lost or deformed eyelids is equally well handled in our English systematic works on Ophthalmic Surgery as in that of M. Roux. The surgeon who has watched the remarkable manner in which the natural form and usefulness of the eyelids are restored after severe injuries, will not be surprised to find that M. Roux has only two instances to adduce in which accidental wounds of the eyelids called forth any reparative surgery. Though he acknowledges the extreme variety of the conditions of the lids in eversion, he appears to have adopted an almost invariable habit of operating on the plan of Sir William Adams, and he failed thrice in seventeen operations. His cases of restoration of lost lids are few, and not strikingly successful. Rhinoplastic operations, also, appear to have found but little favour with M. Roux; and it is curious to contrast the brief notice which he gives them in his work, with the long and minute essay of Dieffenbach on the same subject. The author appears to have undertaken only five operations of any kind upon the nose, and he acknowledges that he is but partly pleased with the results. The reports of the German operator claim too much reliance for us to think with M. Roux on this subject, and the successes of our own countrymen are sufficiently numerous to relieve the operation from discredit. Mr. Skey, indeed, appears to have been eminently successful in this branch of reparative surgery, since he incurred the responsibility of being made the confidant of a patient whose personal attractions, restored by his art, led to her being perplexed by an offer of marriage.

We will state briefly the author's fifth case:

> "A young man had had his face horribly disfigured by an enormous fibrous polypus, which, commencing in the right nostril, had projected in all directions, and had produced attenuation, and eventually perforation, of the palatine vault, the septum narium, and the nose itself. It was removed by an incision extending up the whole mesial line of the nose, and meeting another which passed along the right eyebrow. Two years afterwards the patient returned to M. Roux, desiring to be relieved of the annoyance which he experienced in speaking from the perforation of the palate, and having also the aperture in the nose still open. The nasal bones having been almost entirely lost, the nose was rather deformed and flattened, and the aperture, though smaller than it had been, measured still a centimetre in breadth, and twice as much in height. It was of oval form, and had a cicatrized border. The parts on the bridge of the nose—i.e., to the left of the opening—were found at the operation to be too thin to furnish the required material; and it became necessary, after having begun to cut the flap from within, to change the plan, and procure one from the thicker, well-nourished, and movable integuments covering the ascending plate of the superior maxillary bone. A nearly square flap was brought over the opening, and secured to its freshly cut surfaces by four points of simple suture. Perfect union took place, and the linear cicatrix which remained could scarcely be noticed as an addition to the deformity already occasioned by the loss of the bones." (p. 90.)

Deformities and Restoration of the Lips and Cheeks.—If the previous subject have been little advanced by the author, his collection of facts on that of deformities of the lips and cheeks is very valuable and interesting.

One day, a subject was brought into the dissecting rooms, whose history could not be traced. A black patch was observed upon her cheek, which proved to be the external plate of a kind of stud, and connected by a shank with another plate, which lay inside the mouth. The whole machine had been fitted to close a circular hole in the cheek, which was large enough to admit the extremity of the middle finger. As the instrument did not so exactly fit the aperture as to prevent all escape of fluids from the mouth, the inner surface of its outer plate was lined by some adhesive substance. (p. 101.)

A man of twenty-four years of age, who had lost nearly the whole of the lower lip from some gangrenous affection in his infancy, presented himself to be relieved of his deformity. Though he habitually lost a considerable quantity of saliva, his general health was perfectly unaffected. The defect in the lip resembled that which is not unfrequently made by the surgeon in the extirpation of the larger cancers of that part, except that it terminated in an arch above the level of the chin, and that the edges of the skin adhered to the bone, and were thin and uneven. The alveolar border of the jaw, where it corresponded to the loss of substance, projected unnaturally, and threw the incisor teeth so far forward that it was evidently impossible to bring the skin over them. This unnatural protrusion appeared to be due simply to the loss of the muscular lip, and illustrated very well the power of the soft parts over the form of the bones.

It might have been possible to have formed a symmetrical pair of flaps from the parts beneath and about the chin, and to have turned them up toward each other so as to form a lower lip, in a manner more or less resembling that of M. Chopart in France, and of Mr. Syme in our own country. M. Roux, however, removed the portion of the lower jaw corresponding to the deformity in its whole depth, and brought together its lateral portions and those of the lower lip respectively. Perfect union followed, and the lip was reconstituted, with no deformity but a mesial cicatrix and a receding chin. (p. 108.)

The next case was that of a girl of twenty-one, who had suffered from gangrene of the face only two years before. A large gap existed in the left side of the face, the lower edge of which was continuous with the free border of the lower lip, and its upper boundary approached within a centimetre of the lower lid. In breadth it extended from the middle of the malar bone very nearly to the mesial line of the nose. The destructive process had removed the left cheek and much of the adjoining part of the nose, with the left half of the upper lip; and, the superior maxillary bone having been in great part involved in the sloughing process, the deeper wall of the antrum, as well as the cavity of the left nostril, was exposed. The tongue, being unsupported on the left side, continually protruded. All but the lower border of the huge gap was adherent to the bones. Such a deformity might well have been given up as impossible of cure, and but for the entreaties of the patient it would have been given up more than once. Seven operations were performed on her altogether, but only three of them contributed to the cure; and twelve months were occupied in effecting what was at length accomplished. The gap was too large to be filled at one operation, and by the transplantation of a single flap. The first procedure, therefore, consisted in completing the orifice of the mouth. This was accomplished by implanting a considerable part of the lower lip into the left side of the upper. An oblique incision, commencing at the edge of the lower lip, a little to the left of its middle, and carried downward toward its left side, set free a flap, which was then fixed by sutures to the left side of the remnant of upper lip. The angle of the wound rose nearly to the level of the mouth, and formed the left commissure, while its right edge became continuous with that of the lower lip, and completed it. The union was perfect; and the aperture in the cheek having thus been separated from that of the mouth, there remained only to fill the former. But its diameter in every direction was still very large, and the prominence of the bones nearly all around, and the absence of any subjacent structure to which a flap could have been laid, precluded any advantage from either bringing the edges together, or seeking to close the orifice with a flap. Twice were the edges pared and united by sutures; but the strain was too great, and the parts gaped as widely as ever. Once a flap was formed from the palm of the hand, but an unhappy slipping of the apparatus which bound the arm to the head, tore through the connexions of the flap with the face, and it was allowed to reunite by suppuration with the hand. The plan of turning out the lining membrane of the adjoining part of the cheek proved equally inefficacious, for the flap was too thin, and sloughed. M. Roux now thought it useless to persist, and he procured for her an artificial cheek; but the girl would not even try it on, and implored him to persevere.

Six months had elapsed since the first operation, and the new half of the upper lip had maintained a perfectly natural condition, and had drawn up the new commissure quite to the level of the opposite. It occurred to M. Roux that the flap thus raised might be transplanted a second time, and to a greater distance than before, and might even fill the gap in both cheek and nostril. If it should unite in that situation, the gap in the upper lip would resemble a widely-parted hare-lip, and might be readily closed by a new operation. Accordingly, an incision was made in the cicatrix which united the right and left portions of the upper lip, and the latter was separated from its new deeper connexions, and attached by eight points of suture to the sides of the lateral opening and the nose, which had been previously prepared to receive it. Complete union took place, and the contractile power of the new flap subsequently drew towards it the soft structures which had formed the borders of the unnatural opening. A seventh operation reunited the right side of the upper lip to the flap, and the continuity of all the soft parts of the face was thus restored. The cheek was indeed flat, and scored with cicatrices, and the mouth was narrow, and placed on the right of the mesial line: there was, however, nothing offensive in her appearance, and she lived twelve years after the operation, in the public occupation of a shopwoman. (p. 118.)

A man of twenty-five had his face bruised, crushed, and otherwise injured by the fall of some heavy stones. He survived, with the loss of nearly all the soft parts, and many pieces of the bones of the face; and presented himself, two or three years after the accident, with the remnant of the bones forming nearly a plane surface, and covered with scars. Besides other deformities, he had no lips, and nothing indicated the usual situation of the mouth behind the one great scar which hid it. A single opening existed in the middle of the face, through which the cavity of the nostrils could be seen, and small morsels of food could be thrust down into the mouth.

Reparative surgery could effect little for him, but the thin tissue which occupied the place of a mouth was opened transversely, and the edges of the mucous lining brought out, so as to give the aperture something of the appearance of a mouth. M. Roux had much fear of the closure of the aperture, and lays some stress upon the union of the inner and outer layers of the new lips by suture. It is, however, probable that the formation of the angles of the new mouth, by means of a small seton or the actual cautery, a week or two before making the transverse incision, would have effectually prevented its subsequent closure. An artificial nose was then supplied to him, and the poor fellow was dismissed in a state which no longer excited the disgust of all who beheld him. (p. 132.)

Congenital fissures of the upper lip present so much uniformity, even in their varieties, and the principles of their treatment are so generally agreed on, that it will be needless to follow the author through the nearly ninety pages in which he addresses Mr. Lawrence on this subject. He gives a case in which he operated on a child two days old, and after the apparent failure of the operation, obtained a cure by careful bandaging and changing the child's nurse. The most interesting observations in the chapter are those on the period which should be selected for the operation, as he thinks it should vary with the character of the deformity. He found operations for hare-lip to succeed best in proportion as the patient was advanced in age, while in those performed during the first few weeks of life he met with as many failures as successes. There is no need to operate early on a child with a simple hare-lip, if it can take the breast, nor any advantage in doing so, if the child have, in addition, a fissured palate. The influence of the reunion of the soft parts on the bones, however, should lead us not to postpone beyond the first year of life the operation for a hare-lip which is complicated with a fissure of the palatine vault. With this exception, however, M. Roux counsels that all operations for hare-lip should be put off to the third or fourth year of life. One of his most successful operations was performed on a subject of thirteen years of age.

Deformities and Restoration of the Palate.—The appearance of the volume before us satisfies a long expectation of surgeons. It was known that Roux had performed many operations on the cleft palate, but no statement of the results of his practice had been published for many years. We have now, however, the means of estimating the value of the operation which he had such concern in originating, and, through the courtesy of Mr. Fergusson, of comparing its results with those of the operation as performed on the principle of the latter surgeon. Operations for the cleft palate have not yet been sufficiently discussed in our English surgical works; and though we hope in some measure to supply the want, yet we look for some publication on the subject from Mr. Fergusson himself, and are happy to hear that he is preparing one. The best foreign essay on the subject is the one before us. It is in great part a reprint of the first essay published by the author in 1825; but enriched and enlarged by the addition of his experience during nearly thirty years.

We may be spared from entering into the history of the contest as to priority, which was waged between the German and French, on behalf of Graefe and Roux respectively. The former practised an operation on fissured palate three years before the latter; but he used metallic wires, and failed. Roux devised independently a similar operation; but used the silken suture, and succeeded. When the two operators afterwards met at Graefe's table in Berlin, the host acquitted his guest of the scientific plagiarism of which he had been accused. It was no unlikely thing that the plan of uniting the fissured palate in the same manner as the hare-lip, should have occurred to two surgeons who were both on the look-out for reparative operations, especially if they both watched the parted halves approach each other in the effort of deglutition, in the manner shown by Mr. Fergusson to be due to the action of the superior constrictor of the pharynx.

The palate is rarely the seat of actual injury: yet it has occurred to M. Roux *three times* to have been under the necessity of slitting the velum palati along its middle, in order to facilitate the extraction of tumours from the pharynx, and twice to have seen cases in which it had been accidentally wounded. A girl, of five years of age, fell with the handle of a racket in her mouth. The handle perforated the soft palate, and tore back a small flap, which remained hanging in the mouth behind the opening. M. Roux passed two double threads through the flap, in such a manner that the nooses projected on its buccal surface and held a twisted roll of oil-silk, while the ends, passing up through the perforation, were brought out through the front of the nostril, and fastened just tightly enough to keep the flap applied in its place. Perfect union took place, and the threads were removed in four days.

The other case was one in which a young woman, of seventeen or eighteen, applied to M. Roux on account of a fissure of the velum, which, though commencing at the palatine spine, and having perfectly even edges, was yet formed by an unequal division of the soft palate, the left portion being rather larger than the right, and the fissure consequently inclining to the right. It is not unusual to find the halves of the soft palate of unequal size in cases of congenital fissure; but in the present case the velum had been rent by the horn of a young bull, when the child was four and a half years old. For some unexplained reason the patient was not operated on.

The defects of the palate admitting of operation, which originate in disease, are not all syphilitic. M. Roux is of opinion that strumous disease occasionally issues in the production of fissures of the velum, as well as in perforations both of the velum and vault. Such defects of the palate, from whichever disease they may proceed, are generally less suited for surgical operations than those which are congenital. For the most part, they are attended with some serious loss of substance; and, in the instance of the velum, the soft parts which remain are left by the disease marked with cicatrices, and bound by unnatural adhesion to the pharynx.

Perforations of the velum may present any form, and occupy any situation; but they are all alike in having thin edges. The form best adapted for operations is the vertically oval; and the mode of proceeding must vary with the case. In some instances, when the perforation is situated far back, it may be wise to convert it into a fissure, by slitting through the back of the velum, and then to operate as for a fissure, in the ordinary way.

Perforations of the vault admit of cure, if the loss of substance or breadth of the aperture be not too great. In the case of congenital fissures of the whole length of the palate, the union of the velum by operation is followed by a gradual approximation of the sides of the remaining palatine fissure, which ceases when the bones become full grown and fixed. It is therefore important to wait for some years after uniting the velum, before proceeding to operate for the cleft in the hard palate. Indeed, it is doubtful if it be right to attempt the closure of such fissures by operation. We believe that no surgeon has been satisfied with such an attempt; and Dr. Warren, who first spoke well of it, has since failed so often that he has abandoned the operation. Roux himself scarcely tried it, or at least records no instance of success from the attempt. When, however, such a fissure reaches but a short distance into the bones, and is angular and not arched, there is no necessity to leave an aperture unclosed in front of a united velum. The structure lining the back of the bones can be dissected off them, and made to form a continuous flap on each side with the halves of the velum; the whole can then be brought to the mesial line, and united there on a level below that of the bony palate. Success has followed such operations in the hands of Dr. Warren, Mr. Avery, Mr. Fergusson, and others. We may notice, however, in passing, that other operators are not at one with M. Roux as to the facility of separating the soft structures from the palatine vault. Large perforations resulting from disease can only be filled by an artificial substitute. The following are examples of the closure of smaller ones by operation:

> "The patient was thirty years of age. It was determined to close the opening by lateral flaps. The fibro-mucous membrane which lines the hard palate admits of being easily separated from the bone with the handle of a scalpel, and, when separated, presents a considerable amount of flexibility. Advantage was taken of this latter fact to give as much breadth as possible to the pedicle of the flaps; and upon the supposition that the chief supply of arterial blood came to the hard palate from behind, it was arranged to leave them attached posteriorly. Two angular flaps were accordingly cut, which met at their posterior and broader parts, like the two halves of the letter M, and included the aperture in the palate between them. They were then separated from the bone, and brought inward into contact beneath the aperture, which had been previously prepared by incisions, to unite with them. Two ligatures were employed; that near the base of the flaps was tied on their lower surface; but the anterior, having been passed in the opposite direction, was first tied on the upper surface of the flaps, and then fastened by its ends, previously brought out through the palatine aperture and nostril, to a plug beneath the nose. Complete union took place." p. 256.

The other case is abridged from the operator's account in the 'American Journal of the Medical Sciences.'

> "It was one of an oval opening in the palatine vault, and the principle of the operation was to cause flaps to glide over the opening, at the same time that they retained their original connexions. The interval between the former and the new position of the flaps was filled, before their transplantation, by granulation. The operation consisted in making an oval incision at some distance around the opening, separating the parts between the incision and the border of the aperture from the bone, and inserting a piece of buckskin to prevent their reunion in their former situation. An oval flap was thus made, encircling, and adherent only at the edge of, the aperture. After a few days, the whole wound had filled with granulations, and the second part of the operation was undertaken. This consisted in detaching

the flaps at their concave border, or from the edge of the opening in the palate; and, while they retained their connexions with the new granulations, bringing them into apposition beneath the opening. It was a tedious proceeding, but was quite successful, and in three weeks the patient was perfectly relieved of every vestige of his deformity. The first oval incision was made at two operations, separated by an interval of six days, as Dr. Mütter feared that the flap might slough if it were deprived of so much of its former connexions at one moment."

Fissures of the velum originating in disease may occasionally call for operation. Of four such cases which were treated by M. Roux, two occurred in public male singers, who sought his aid, partly because of an alteration in the tone of their voices, both in speaking and singing, but chiefly on account of the loss of some of the higher notes of their diatonic scale, which, though they could be uttered, were feeble and, as it were, broken. The loss of substance was in both cases of syphilitic origin, and situated at one side of the velum; and the operation consisted in uniting the loose velum to the posterior pillar of the fauces. Complete union was not obtained in either case, and the defect of the voice, which still remained more marked in singing than in speaking, was in neither case relieved enough for the patients to renew their public avocations in the capital.

Congenital fissures of the velum and vault are always seated in the mesial line. Except in a very few cases, in which the vault alone is cloven, the uvula is always involved, and fissures differ from one another only in their extent forward. The alveolus, however, never splits in the mesial line. There is nothing in the cry of an infant by which the existence of the cleft can be distinguished, or in some sounds of the voice of the adult; but they occasion a nasal tone, a difficulty in the pronunciation of certain consonants, and an alteration of the voice in singing. Some of its consequences may be overcome without an operation; the return of an infant's food through the nostril, for instance, can be prevented by placing the child erect whilst sucking, or by introducing the food into the pharynx with a long tube. The nasal tones are often at once removed by the closure of the fissure; but the articulation can only be perfected by a sufficient practice of the renovated organ. Amongst the many other and curious consequences of the defect, the most important is its influence upon the education of the child and youth; and it is one which calls for the early performance of the operation, for such persons, as they study ill and are easily discouraged, do but rarely complete their education.

The state of the parts presents much variety in different cases. They exhibit all degrees of thickness, and are often unequal in this respect on the two sides. In one case, that of a young lady of twenty-two, the uvula only was fissured, yet the voice had a disagreeable tone, and the pronunciation was imperfect. But on looking just above the angle of the fissure, the velum was seen to be so much reduced in thickness as to appear composed of only its mucous layers, or of a thin single layer of nearly transparent membrane. The thin piece was in the mesial line, of the shape of a lozenge, and about as large as the nail of the middle finger, and it seemed probable that the inequality of the palate arising from this attenuation of one portion, had some effect in producing the fault in the voice and pronunciation. Those whose palates are fissured, usually have the face of large size; and the wide mouth and alveolar arches, which this size of the face implies, facilitate the necessary manœuvres in the operation.

In selecting the period of life at which the operation should be performed, the author states that, while not necessarily serious to the patient in itself, yet it involves a long-continued abstinence from food, which is most detrimental to healing wounds in young subjects; so that though some young persons may be found possessing an amount of fortitude not natural at their years, yet the regimen after the operation almost necessarily insures its failure in young subjects. It has indeed been performed on a child four months old; and although it failed, of course, the surgeon has only half repented of his hardihood. M. Roux has failed thrice at fifteen years; and he thinks it unwise to operate before eighteen.

In his mode of operating, the author made no material change since his first and successful case. He still passed the ligatures first, and sometimes cut them in paring the edges of the cleft; and still completed the operation without lateral incisions. His objection to making such "button-holes," as he terms them, on either side, to relieve the strain upon the ligatures, we think valid; but his neglect of the incisions recommended by Mr. Fergusson, though they were suggested by the dissection of a fissured palate, we cannot agree with. Mr. Fergusson's proposal to divide the levator palati and palato-pharyngeus, he confounds with that of M. Sedillot, who proposed to divide *all* the muscles of the palate, and that by incisions which could not possibly divide them all. In fact, M. Roux appears to have been too satisfied with the results of his own operation to perceive the need of altering it; but a comparison of the results obtained by the two modes of operating, will plainly show that success is best secured by the division of the muscles.

The whole of M. Roux's operations on the palate number a hundred and thirty-nine, and may be divided into the two principal heads, perforations and fissures. Three out of four operations for closing syphilitic perforations of the vault succeeded, and four out of five perforations of the velum. One operation for closing a congenital fissure of the vault, remaining after the successful performance of staphyloraphy, proved unsuccessful from the sloughing of one of the flaps; and two cases of lateral fissure of the velum, produced by syphilis, were but partially improved by operation. The author's incisions along the middle of the velum for surgical purposes, united perfectly in all three cases. The remainder of his cases are those of congenital fissure. The report is confusedly given, but so far as we can succeed in understanding it, the following is the numerical result:

First operations for fissure of the velum	61
Successes	44
Second operations	8
Successes	7

Three of the eight patients who submitted to a second operation had not been operated on at the first occasion by M. Roux. The total number of his patients was, therefore, sixty-four, and his successes amounted to fifty-one in sixty-nine operations.

First operations on the velum, the vault being also fissured . . .	51
Successes	25
Second operations	4
Successes	1

That is to say, twenty-six successful results from fifty-five operations on fifty-one persons.

By "success" in this report, is not to be understood an immediate and entire union of the whole fissure. Various further measures were not unfrequently needful to secure the closure of portions of the cleft which had not united, or of permanent openings which were not unusually made by the sutures. Nor does the word imply anything as to the subsequent perfection or imperfection of the articulation and voice. And in the second division of the cases, though great improvement took place in the size of the opening which remained in the palatine vault after the fissure of the velum had been closed, insomuch that in one case it diminished by more than one-half of its breadth in three months and a half, yet the word "success" implies only the union of the velum. Indeed, in all M. Roux's later and successful cases of this kind, he added to the size of the opening already existing in the bones by a transverse incision separating the whole velum from the vault. Of the eventual condition of this part of the aperture the author gives no account.

Three cases are reported as having terminated fatally. One of these was that of a young lady of fifteen, who was about to quit Paris after a very successful operation; but fifteen days after the operation symptoms presented themselves, which soon assumed the plain appearance of pulmonary phthisis, and destroyed life in less than three months. The second patient was a young woman of twenty-two, in whom everything promised a successful issue to the operation, but no union took place; inflammation of the palate, of the pharynx, of the whole pulmonary mucous membrane took place, and terminated her life on the eighteenth day after the operation. The third instance was that of a young man, whose death M. Roux attributes entirely to his mental state. He appears to have been constantly harassed with the painful consciousness that he was subjecting himself to the operation in opposition to the known wishes of his parent. Without physical symptoms which could explain his death, he sank on the fifth day after the operation. There had been no union. The body was not examined.

The various rumours and partial statements which have been current respecting the results of M. Roux's practice of his operation of staphyloraphy, had prepared most surgeons for a less satisfactory account of them. A success in two-thirds of the cases, however, may be looked on as satisfactory, if we remember that for many years the alternative was to leave these patients unrelieved by art. The deaths were not "numerous," as had been stated; there were three: and out of the remaining one hundred and twelve cases, seventy-seven were successful. But the author acknowledges to have seen, during the London Exhibition in 1851, the preparation upon which Mr. Fergusson's proceeding of dividing the muscles was founded; and it seems to us a matter of regret that he should, since then, have retained so inflexible an attachment to his own mode of operating, and should have, practically, sanctioned no other in his book. For what is the result of the English operation? The chief causes of failure are removed by it. Patients can swallow fluid food, and escape the terrible regimen of starvation and forced quiet of the throat which Roux enjoined. The flaps come together without strain. The ligatures, not being dragged by the spasm of the muscles, do not cut through the tissue, and make perforations which require to be closed by the actual cautery; they may, consequently, be left for a much longer time than that stated by M. Roux, and instead of their removal being indispensable on the fourth day, the last has occasionally been left as late as the seventh. The probability of success is plainly enhanced to a great degree by such delay, and the singular circumstance which occurred in the practice of Mr. Skey may not unfrequently occur again.

> "Mr. Skey, not long since, operated for fissure of the soft palate. The edges of the wounds sloughed and retracted, and the case seemed nearly hopeless; but he kept in the sutures, and granulations sprang up from the edges of the cleft, after the separation of the sloughs; they met in the mid-space of the cleft, and coalesced, and formed a perfect scar."*

The treatment under which this success was obtained is detailed by Mr. Skey in his work 'On Operative Surgery;' it consisted of an abundance of strong fluid nourishment, half an ounce of the compound tincture of bark daily, and the topical use of a solution of the nitrate of silver.

The numerical results of the operation when the muscles are divided, are equally satisfactory. Mr. Fergusson, at the time at which we write, has operated forty times, and has failed but thrice. His thirty-seven cures were performed on thirty-nine patients, one of the three who were disappointed consenting to a repetition of the operation. The state of the parts had varied much in the different cases: in one the edges of the cleft had already been pared three times; in others the parts were so large that they might have been brought together without any division of the muscles. Mr. Avery has published seven successful cases, and others are scattered through our periodical publications. The eventual effects upon the voice and articulation do not appear to be either more or less satisfactory than those of the unmodified operation of the author. Indeed, no instance of success can exceed that which he obtained in his first case; for, only eleven days after the operation, the subject of it read a paper at the French Academy of Sciences, detailing the manner of his cure.

Such great success as that which we are able to record as attending the operation so happily suggested by M. Roux, and improved by the scientific addition of Mr. Fergusson, leaves little to be done in order to secure its perfection. Yet a new era seems to open in the treatment of some of these cases; for there is a prospect of the operation of staphyloraphy, perfect as it is, being in certain cases anticipated by the use of the actual cautery.

The power of contraction possessed by the cicatrices of burnt parts has long led to the employment of caustics in the treatment of diseases. Various unnatural openings in soft parts have been closed by them, and fistulæ contracted or cured. We have ourselves succeeded by the use of the actual cautery in curing the incontinence of urine left in a young woman, two years after she had undergone the operation of lithotomy. The urethra, which permitted incessant incontinence in any posture, and admitted half the forefinger, was in a few weeks so contracted by the treatment, that she was able to walk for two hours without incontinence or inconvenience. Its chief use in the palate has hitherto been to close the smaller perforations of the velum. M. Montain, of Lyons, many years ago, applied it to the adjoining surfaces of the two halves of a fissured palatine vault in an infant, after having brought them into contact in the mesial line by lateral compression; and he is said to have succeeded in procuring their adhesion. M. Jules Cloquet, however, has carried the practice further, and has applied it to the treatment of cases of fissured velum. His assertion is, that the two halves of the velum can be brought into complete union along the mesial line, as an arm and the trunk, or two adjoining fingers, are fixed after a burn by a web of skin drawn towards the angle which unites them; and on this principle he makes repeated applications of cauterizing agents at the angle only of the fissure. He states that some twenty such operations suffice to close a fissured velum by a linear cicatrix in the mesial line, and to restore its functions in deglutition and speaking. Whether its use is restored in singing is not stated. The plan is applicable also to the lateral syphilitic fissures, but the least extension of any fissure forward into the bony vault necessarily precludes this mode of treatment, as there can be no cohesion of the first angle, or, consequently, of all the rest. The plan has not been enough tried to allow a fair comparison of its results with those of the operation by incision and suture. But the proposal is a rational one; it has succeeded; and the proceeding is so simple, especially if the electro-galvanic cautery be employed, that there appears little doubt of its superseding, in suitable cases, the painful and tedious operation of staphyloraphy. Its chief advantage, however, is that it may be applied in infancy, and that the evils which arise from the existence of a cloven palate during the important years of education may be obviated. The proposal is quite a new one, and needs the reports of additional observers as to its uniform results.

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* Paget's Lectures on Surgical Pathology, vol. i. p. 224.

Ruptures and Restoration of the Perineum.—This essay, like the preceding one, is not the first which the author has published on the subject; but, unlike that on the palate, it is, in great part, merely a reprint of his first edition. It contains descriptions of twenty operations on the perineum, which M. Roux practised on eighteen patients. Three of these patients died, one of eighteen years of age, prostrated, and with diarrhœa; a second, aged forty, whose perineum had been destroyed by syphilitic ulcerations, of general long-standing disease; and the third, thirty-two years of age, also a venereal case, of erysipelas. The perineum was restored in thirteen of the eighteen patients, two having submitted to a second operation. In many of the cases a fistulous communication between the rectum and vagina remained, though in a few it was afterwards obliterated by the use of the actual cautery. Some of the patients bore children after the operation, without injury to the new perineum.

In all but five cases the division of the perineum was in some way occasioned by the process of parturition. A rapid birth, a large first child, a long perineum, was generally the cause of the rupture; though once, even in the birth of a fifth child, it was occasioned by the use of instruments. In one instance, the separation occurred gradually, and was only completed several days after parturition; it seemed to arise from ulceration, proceeding outwards from a recto-vaginal fistula, which had existed since a previous labour. There was no instance of a congenital fissure. A previous surgical operation for the cure of fistulæ in perineo was, in one instance, the cause of it; and M. Roux speaks of a case, not his own, in which the jealousy of a husband led him to the brutal revenge of thus mutilating his wife.

Roux's great merit in regard to the operation of perineoraphy was, undoubtedly, his employment of the quill-suture. In carefully paring the disunited halves, and fitting them to each other, he acted as others had done before him; but by his employment of the quill-suture he revived the operation, and led the way to still further improvements. The chief advantages of a suture of that kind are that it retains the surfaces of deep wounds in apposition, and that a great length of time usually elapses before it cuts its way out. The interrupted or the twisted suture may be employed with it; and the combination ensures greater quiet for the whole wound. It is impossible to deny that great success has also attended M. Dieffenbach's employment of the more superficially-acting sutures (though the author fails to acknowledge it), but that success was due in great part to his relieving the strain upon the sutures by long and deep lateral incisions. M. Roux never practised lateral incisions, nor have they been generally adopted by the principal operators of our own country. The incisions of most value appear to be those of the sphincter ani, since by merely separating that muscle and the levator from the coccyx, Mr. Hilton placed the ruptured perineum under the control of the anterior fibres of the levator ani, and procured sufficient relief of the distressing incontinence of fæces to two patients to enable them to resume their ordinary avocations with comfort. Mr. I. B. Brown, after operating on M. Roux's plan, divided the sphincter completely on both sides. This procedure, which appears to have been suggested by his observing the posterior part of the fissured perineum parted by the traction of the two halves of the sphincter, prevented the separation of the wound after its union by the quill-suture, and greatly contributed to the all but universal success which he reports as attending his operations. The plan of dividing the sphincter in these cases is precisely analogous to that of severing the levator palati and palato-pharyngeus from the soft palate in staphyloraphy, and to Mr. De Morgan's plan of dividing the tendo-Achillis in the insuperable spasm attending certain cases of fracture of both bones of the leg. It must undoubtedly facilitate the speedy union of the wound; for M. Roux never removed the ligatures in his operations till the fourteenth day, and in one of his patients the perineum tore asunder on the sixteenth day after the operation; whilst firm union had occurred in most of Mr. Brown's cases before that period, and he removed his ligatures between the third and the sixth days. A further advantage is probably due to the division of the muscle, that the septum unites better, and recto-vaginal fistulæ are less common. Such fistulæ remain, as a rule, in M. Roux's practice; Mr. Brown mentions but one after eighteen of his operations. The actual cautery succeeded with both operators in closing these communications, but some of M. Roux's patients were dismissed with the fistulæ uncured. And lastly, the irksomeness, as well as the danger to the wound, arising from prolonged constipation of the bowels, is also much relieved by the division of the sphincter. Certainly, we may say that if perineoraphy has become what the author fondly calls it, a classical operation, the division of the sphincter must become so too, for it is a scientific and well-nigh essential part of the whole procedure.

Should such results as those reported by Mr. Brown be confirmed by the observation of other practitioners, there will be little need to resort to another plan of uniting the ruptured perineum, suggested by M. Jules Cloquet. He has proposed to apply to these cases the practice which he has successfully adopted in fissures of the soft palate. We have already spoken of this procedure. He asserts that the repeated application of caustics or the actual cautery to the angle of the division will permanently and firmly close both the septum and the perineum. In this region, as in the palate, he has met with success; but it has been obtained only after a long continuance of the treatment, and we fear the tediousness of the cure will often outlast the determination of the patient to obtain it. The result will probably in most hands be more satisfactory than that of the operation by the suture, for such success as that of Mr. Brown can hardly be looked for universally. He records no instance of failure, and only one of death; but it is to be recollected that the results of his public practice were obtained chiefly in a new hospital, in which, as in private practice, there was probably little liability to erysipelas. The actual cautery is of course inapplicable to recent ruptures, which are often successfully treated by the early employment of the suture. It will probably find its application to patients who are unsuited for operations.

Vol. II.—Since the preceding remarks were written, the second volume of M. Roux's intended work has been published. It is addressed to M. Chelius, and has for its title, 'The Diseases of Arteries,' though the only subjects treated in it are aneurisms, wounds of arteries, and aneurismal tumours of bone. The detailed cases are full of interest, most from their nature, and all from the manner in which they are described: they are arranged so as to illustrate, in successive letters, the pathology of spontaneous aneurisms, the successful and the unsuccessful issues of their treatment by the Hunterian operation, "false consecutive," and "arterioso-venous" aneurisms, recent wounds of arteries, secondary hæmorrhage, and aneurismal tumours of bone. One chief object of the work is to commend in France the English operation of John Hunter, which was introduced into French surgery by M. Roux in 1814; but our English readers will hardly care to learn the foreign adventures of an operation with which they are so familiar. Another object of Roux's, in the work, is to advocate the use of the ligature recommended by Scarpa, who included the artery with a roll of oiled silk within two ligatures. Curiously enough, however, Roux miscalculated the numbers of his cases; he enumerated the deaths correctly as 8, but supposed the total number of his cases to be 33, instead of 23, and thus estimated highly a mode of operating which, by the showing of his own correct numbers, is one of the most fatal. The error affects other conclusions also, and has afforded the

Commission much perplexity; for they did not feel at liberty to correct the text of the deceased author, nor yet to sanction an injury to science which he would doubtless have avoided inflicting had he lived to revise the book. They have accordingly issued this second volume with accompanying notes; as, however, the remaining MSS. would require as much, if not more, revision, they have, and perhaps wisely, determined not to publish them. The present volume, therefore, completes the posthumous works of M. Roux.

With the same tenacity which characterized his adherence in other matters to what he had once approved, we find him clinging to the Hunterian operation; and his second volume, though nearly five hundred pages long, furnishes us with no information on various modern proposals for the treatment of aneurisms. His last case of mediate compression occurred in 1816; galvano-puncture, and the injection of coagulating materials, he never tried; nor did he ever tie a trunk on the distal side of the aneurism. It does not appear, indeed, that he ever met with a case requiring this last measure, or with one demanding the ligature of the innominata, the common, or the internal, iliac; whilst, of his 84 operations on arteries, only one was performed upon the external iliac. It would seem that the liability to certain aneurisms varies in different countries, as we recollect to have been told by Rokitansky eleven years ago that he had then never seen one on the subclavian artery. The value of Roux's work lies chiefly in the cases which he details, and from which our space permits us only to select the following:

"CASE VI.—In the years 1805 and 1806, a labouring man was in the Charité Hospital, with a popliteal aneurism. Deschamps and Boyer applied compression to the femoral artery in the middle third of the thigh. Various accidents interrupted the treatment, but it was continued for twenty months. At the end of that time he was dismissed, cured; and in the year 1841, coming under M. Roux's care in the Hôtel-Dieu, he was found to have no vestige of the previous disease in the ham." (p. 71.)

"CASE VII.—A man, of thirty-three years of age, having two aneurisms in the same limb, one in the ham, the other in the groin, was compelled to lose the limb by amputation between the two diseases, on account of the mortification of the leg. Instead of tying the external iliac artery, as Roux suggested, Boyer applied pressure on the artery at Poupart's ligament, and a tin apparatus made to fit the remaining tumour, and to maintain a cold temperature on its surface by being filled with cold water. The patient lived five years, and the tumour rather diminished than increased in size. He died of an aneurism of the arch of the aorta." (p. 72.)

"CASE VIII. is one of popliteal aneurism, treated in 1811 by compression of the femoral artery. But a slough forming under the pad, the artery was opened, and though a ligature was applied to it, the patient died of secondary hæmorrhage." (p. 76.)

"CASE LXII.—An agricultural labourer, aged twenty-five, of healthy aspect, and unaffected by any syphilitic or other constitutional disease, was admitted into the Hôtel-Dieu in 1844, with a great enlargement of the upper part of his left tibia. He had been first conscious of the disease eighteen months before, when a severe pain was brought on in the part by a great exertion in lifting a heavy stone. The pain became permanent, but no enlargement of the limb took place until two or three months after the appearance of the first symptom. In twelve months from the commencement of the disease the swelling began to pulsate. At the time of his admission into the hospital, the whole of the head of the tibia was considerably enlarged; but one portion, of the size of the thumb, approached the surface just in front of the head of the fibula, and extended forward beyond the level of the tibial crest. It was covered with varicose veins, and with rose-coloured but not inflamed integument; its surface was smoothly rounded, not nodulated, and of bony, cartilaginous, or soft consistence, in different parts. Its pulsations, or rather its expansive movements, were both visible and palpable, and were a little in arrear of those of the radial artery; when compression was made on the femoral artery they stopped, and the tumour sunk a little, and became more supple; but its tension and pulsations instantly returned on releasing the arterial trunk. There was no bruit." (p. 456.)

Concluding that the case was one of aneurismal dilatation of the vessels of the bone, and uncomplicated with any malignant growth, M. Roux determined to treat it as aneurisms by anastomosis are treated elsewhere, and tied the femoral artery. The result was a complete cure; the tension of the tumour instantly disappeared, and the mass itself slowly shrunk and became consolidated. In five months the tibia was still a little enlarged, and was rugged, but it presented no soft spots; and in twelve months no trace of the disease could be discovered.

There is one circumstance which will not fail to affect many readers of this work in England with extreme displeasure—viz., the sacrilege of invoking the name of the Most High as a mere form for the expression of an opinion. Science pays little enough of avowed homage to the God whose works are its subject. Would that it did far more! We may, however, be grateful that our English works are at least unstained by such a blot as that to which we have alluded.

Charles H. Moore.

REVIEW V.

1. *Maximen der Kriegsheilkunst.* Von Dr. LOUIS STROMEYER. Kœnigl. Hannöverschem Generalstabsarzt, &c. &c., früherem Generalstabsarzt der Schleswig-Holsteinischen Armee. 2 Abtheilungen.—*Hannover,* 1855. 8vo, pp. 773.

Principles of Military Surgery. By Dr. LOUIS STROMEYER.

2. *Die Schusswunden.* Nach auf dem Schlachtfelde wie in dem Lazareth während den Jahren 1848 und 1849 gesammelten Erfahrungen dargestellt von Dr. BERNHARD BECK, Grossherzogl. Badischem Militäroberarzte.—*Heidelberg,* 1850. 8vo, pp. 343 und Tabelle.

Gun-shot Wounds. Represented from Experience collected on the Battle-field and in the Hospital during 1848 and 1849, by Dr. BERNHARD BECK.

3. *Ueber Resectionen nach Schusswunden.* Beobachtungen und Erfahrungen aus den Schleswig-Holsteinischen Feldzügen von 1848 bis 1851, von Dr. FRIEDRICH ESMARCH, Privatdozenten an der Universität Kiel, früherem Oberarzte in der Schleswig-Holsteinischen Armee.—*Kiel,* 1851. 8vo. pp. 136 und Tabelle.

On Resections after Gun-shot Wounds. Practical Observations made during the Schleswig-Holstein Campaigns of 1848 to 1851, by Dr. FREDERIC ESMARCH.

1. *Ueber Schusswunden, verbunden mit einem Berichte über die im Grossh. Militärlazareth zu Darmstadt behandelten Verwundeten vom Sommer*

1849. Mit zwei lithographirten Tafeln. Von Dr. GUSTAV SIMON, Grossherzogl. Hess. Militärarzt.—*Giessen*, 1851. 8vo. pp. 160.

On Gun-shot Wounds, with a Report on the Wounded of the Summer of 1849, *treated in the Military Hospital at Darmstadt.* With two lithographed plates. By Dr. GUSTAVUS SIMON.

5. *Beiträge zur Lehre von den Schusswunden Gesammelt in den Feldzügen der Jahre* 1848, 1849, *und* 1850, von HARALD SCHWARTZ, Dr. Med. und Chir., früherem Oberarzte.—*Schleswig*, 1854. 8vo. pp. 230.

Contributions on Gun-shot Wounds. Collected during the Campaigns of 1848, 1849, and 1850, by Dr. HARALD SCHWARTZ.

6. *Namentliches Verzeichniss der Todten und Invaliden der Schleswig-Holsteinischen Armee aus den Jahren* 1848, 1849, *und* 1850-51, *mit mehreren numerischen Uebersichten.* Von Dr. HEINRICH CHRISTOPH NIESE, Generalarzt der früheren Schleswig-Holstein Armee.—*Kiel*, 1852. 8vo.

Nominal Return of the Killed and Wounded of the Schleswig-Holstein Army, from the years 1848, 1849, and 1850-51, with several Statistical Tables, by Dr. H. C. NIESE.

IT has always been acknowledged by eminent authorities, that military surgery requires its cultivator to be possessed of certain concise general principles, which, though identical with the rules laid down for the surgery of civil life, should be expressed in so distinct a manner as to protect the individual from an unnecessary repetition of experiments, and to insure to the sick or wounded soldier the full benefit of a definite course of treatment. All great military surgeons have striven to establish these principles in theory and practice; the success which has attended the efforts of men whose names will ever be connected with the history of that branch of the healing art which forms the subject of this article, has been great and uniform, and has rendered this department an integral part of medical science. But being well aware that the establishment, through their own experience and writings, of these principles was not sufficient to insure their general application, these distinguished men have adopted various ways and means to diffuse that knowledge, which, from the mode of organization of the service (we allude more especially to the system pursued in continental armies), it was impossible for the bulk of the army surgeons to derive from their own theoretical acquirements or practical experience. One of these means consisted in issuing instructions, written by the experienced surgeon, or by those who were in a position to sum up the experience of the many; but though the use of these instructions increased daily in frequency, yet they were generally considered of doubtful utility; and so far as they were obligatory, they rather fettered and impeded the progress of the surgeon.

If we inquire into the obstacles which may prevent military surgeons from gaining a sufficient amount of practical experience to insure their efficiency under ordinary circumstances, many points present themselves for consideration. Of the surgeons who accompany an army into the field, only those acquire real surgical experience who are detached in charge of the hospitals. Of the surgeons who move with the troops, those only who are attached to the ambulances become familiar with the performance of operations. The rest acquire no experience whatever, beyond applying the first dressing to a wound, or making an examination, and ordering a dose of medicine, preliminary to sending the patient to the hospital. As, however, it is necessary that every army surgeon should be as universally experienced as possible, it is obvious that he ought to be employed successively in each department of medical service,—in the field, at the ambulances, in the trenches, in field and stationary hospitals. This system would, on the one hand, obviate the necessity of a formal code of instructions, by affording the surgeon an opportunity of obtaining personal experience, while, on the other hand, it would be the surest means of finding out for what particular service every man is most qualified, so that he may afterwards be permanently employed in it, and be able to instruct those who shall subsequently enter it in their turn. It was upon these principles that Dr. Stromeyer conducted the medical department of the Schleswig-Holstein army, after he had, in 1848, been appointed its chief; and he had the satisfaction of seeing his system thankfully acknowledged as a boon by the whole of the army surgeons. Under this system a great many surgeons were trained and fitted for every kind of medical service; the right men came into the right places, while, at the same time, the whole department was sifted of its inefficient elements. The reasons why rotation upon Dr. Stromeyer's principle should be adopted in every army are very numerous, and so strong, that the enumeration of a few will, we trust, suffice to prove our position. As a man necessarily takes more interest in the result of his own acts than in the results obtained by others, we ought to employ this tendency for the benefit of the wounded, and afford the surgeon an opportunity of curing a man upon whom he has been required by circumstances to operate. After an important action, the surgeons who have attended in the field or at the ambulances should be sent to the hospital along with the wounded who have been under their care during or immediately after the battle. The interest in the welfare of their patients would be thereby increased, and the wounded would receive greater attention than can be otherwise insured to them. A regular system of rotation would not only afford an opportunity to all surgeons of an army of gaining uniform experience, but would bring to light their several talents and abilities, so that those in command would be able readily to ascertain for what department each individual was best adapted. Good operators would be principally attached to the ambulances; physical qualities, such as a strong constitution, would qualify for field service; those who have peculiar facility in treating the sick or dressing the wounded would be sent to the hospitals; military qualities would enable the possessor to command, inspect, and superintend; and the mind fitted for organization would find employment in keeping accounts and registers, and supplying the medical wants of either hospital or army. In recommending the adoption of Dr. Stromeyer's practice, and the perusal of his remarks thereon, and on the organization of the medical service of the Schleswig-Holstein army contained in the introduction to his work, we do so from a deep conviction of the soundness of the reasoning which led to the introduction of that practice, and from the opportunities we ourselves had of witnessing the success attending it, and the satisfaction it gave to the body of army surgeons. We may, at the same time, be allowed to express our opinion, that an army in which the system of rotation is not carried out,—but where, on the contrary, by the establishment of hospitals not attended by army surgeons, the latter are still more confined to the less instructive duties of the field,—will never enjoy the advantage of a thoroughly experienced and efficient medical staff. The military surgeon should have encouragements in times of war by no means inferior to those held out to the officers. Though the position of military surgeons has been very much improved of late, yet these improvements do not amount to justice towards that body. With satisfaction we refer to the example of the Austrian general commanding in Italy, who, as Dr. Beck records, in the introduction to his treatise, addressed his troops as follows: "The difference between officers as combatants and surgeons as non-combatants must cease. I see everywhere military officers and surgeons equally exposed to the fire, and therefore the surgeons shall enjoy advantages and distinctions in every respect equal to those of the officers." The Austrian general kept his promise, and Dr. Beck himself had the honour of being one of those who returned with the well deserved Order of Merit.

The introduction to Dr. Stromeyer's work contains important observations on the management of hospitals, on the ambulances, and on instructions for the sanitary service. A complete analysis of the two volumes of the 'Maximen' would be an impossibility in our pages; a mere enumeration of the chapters would scarcely leave us any space for their contents. We therefore have deemed it more expedient, and more in accordance with the consideration we have for the taste of our readers, to select a few subjects, which, from their practical importance, from their novelty, or from the circumstance that they involve and illustrate vital principles of surgery, are worthy of more especial attention. In doing so, we would at once convey to the reader our impression of the general style in which all subjects are treated by the author. In the present instance, we find the surgery of war, to a larger extent than has hitherto been usual, illustrated by the experience of civil practice. All leading questions are treated upon a sound physiological basis. And yet the book reads like a novel rather than a systematic treatise, because science is illustrated by interesting and curious cases,—because anecdote and story have their place. A lively and humorous expression often goes further than lengthy arguments, and carries the reader on, imparting knowledge insensibly to his mind. Literary science is well represented. We are introduced to many a conversation which the author had with those who were or now are leading men in medical science. His social relations, his professional friendships and associations with men of equally high standing with himself,—the experiences of his life, practice, travels, and studies,—the productions of his thought,—all these features impart to the work a degree of interest and value in which, we venture to say, it has never been surpassed by any publication on the same subject. With that simplicity and clearness of style which is distinctive of high qualities of mind and power of thought, principles are developed which command the attention and recognition of the present and the future in no less a degree than that great principle which was promulgated to the world only two decenniums since by the same author, and which has for ever placed his name on a level with the highest in our profession.

In the section, On Wounds produced by Arms used in Warfare, the different descriptions and modifications of wounds, more particularly of gun-shot wounds, and their immediate consequences, are described. Admirable as are the chapters On Neuritis and the Therapeutics of Traumatic Hyperæsthesia, the description of these is surpassed by that of the Lesions of Bloodvessels. The practical rules and operative cautelæ concerning hæmostatic operations will be most instructive, even to the experienced. The chapter On Lesions of the Bones (a subject on which Dr. Stromeyer published a separate treatise in 1850), will convince the reader of the correctness of the author's opinion, which is further substantiated in the chapter On the Treatment of Gun-shot Fractures of the Extremities in general, that there is no operation more objectionable, if generally applied, than resection of bones in the continuity. The section On the Course which Gun-shot Wounds take in general, contains a regular treatise on the pathology of pyæmia, which considerably advances our knowledge of that formidable disease. The section On the Treatment of the Wounded on the Field of Battle, is one particularly rich in original experience. The precision with which the indications for amputation or exarticulation on the field of battle are given, induces us to lay the following extract of them before our readers.

General Indications.—1. When a large limb has been carried away by any projectile, and it is desirable to replace the contused and lacerated wound, with the bone generally projecting, by a clean one. Fingers and toes scarcely ever require amputation. Of seventy-three gun-shot fractures of the hand and fingers, seven only were amputated. 2. When the lesion of a limb is such as to preclude the possibility of its further existence, dependent upon nerves and bloodvessels; or of its usefulness, dependent upon bones and muscles. 3. When a grazing shot of heavy calibre has left the skin uninjured, but has destroyed the bony and soft parts. Dr. Beck performed amputation in such a case.*

Special Indications.—I. Upper extremities. The indications differ from those presented in injury of the lower extremities, because, according to the statistics of Dr. Esmarch, all wounds of the upper limbs heal more readily than wounds of the lower, and wounds of the right arm with more readiness than those of the left. Amputation of the upper extremities is indicated only when vessels and nerves are injured at the same time, or when fracture exists along with rupture of the great bloodvessels, or after considerable loss of substance of the soft parts. The special indications then are: 1. When the arm has been carried away by a large projectile, or has been lacerated and broken to such an extent as to preclude the possibility of its preservation. In the diagnosis of these cases, great care is required to ascertain whether the organs of the chest or abdomen have sustained injuries which necessarily must be followed by fatal results.† In these cases the surgeon is frequently induced to perform amputation by mere pity with the wounded, whose sufferings are alleviated by the measure. Dr. Stromeyer caused several amputations of this kind to be performed on the field of battle; and he relates a case of railway accident, which occurred in 1853, in which he amputated the lacerated arm of a man whose lower extremities were paralysed by a co-existing lesion of the spinal marrow. The patient did not die until after the wound of amputation was healed up, in the fourth week. Brilliant statistics are here out of the question. 2. When the brachial plexus has been divided by a shot in the upper arm, so as to deprive the whole hand of motility and sensibility. Under these circumstances the brachial artery must have been torn, and the radial pulse have ceased. 3. When the humerus is fractured and the brachial artery is torn. Even if there is no hæmorrhage at the time, the cessation of the radial pulse is sufficient proof of the division of the artery. 4. When the elbow-joint is shattered and the brachial artery divided. 5. When the wrist-joint or wrist has been perforated by a ball in one of its larger diameters. A shot piercing the wrist in the direction from the dorsal to the palmar surface, without injuring either radial or ulnar arteries, does not involve the necessity of amputation. No lesion of bones of the upper extremities by either rifle or cartridge-ball is, in itself, an indication for amputation. Wounds of the brachial artery alone do not require amputation. The author has, in two cases, tied both ends of the brachial artery in the wound itself, with perfect success. Rupture of the median, radial, or ulnar nerves, alone or in combination with gun-shot fracture, does not involve the necessity of amputation.

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* P. 300, Case 4.

† See a case of Sir G. Ballingall's, third edition of his Outlines, p. 63.

II. Lower extremities.—1. When a part of the leg has been carried away by heavy shot, or has sustained an irreparable loss of soft parts, or has suffered a subcutaneous comminuted fracture, with crushing of the soft parts. 2. When the crural or popliteal artery and vein are ruptured, and the circulation in the lower part of the limb has ceased, even if there is no hæmorrhage from the wound for the moment. 3. When the femur is shattered and its fragments have been carried in the direction of the large bloodvessels. 4. When the femur is shattered to a great extent upwards and downwards, as is frequently the effect of a grape-shot. 5. When the femur is broken and the sciatic nerve ruptured. Division of the sciatic nerve alone does not indicate amputation: Dr. Stromeyer saw the injury three times followed by a favourable result. In one case trismus supervened. In the favourable cases, paralysis continued in the parts supplied by the nerve, but the limb was still more useful than a wooden leg. 6. When the knee-joint has been perforated by a ball, and its articulating ends are shattered to such an extent as to give rise to immediate inflammatory swelling, should the patient require to be removed. Even in those cases where, after perforation of the synovial membrane, the lesion of the bone consists only of a simple impression or contusion, amputation is, according to universal experience, the only means of saving the patient's life. But in this case the operation may be delayed till the patient has arrived in a hospital, conveyance not having the same injurious influence upon the state of the wound as it has when extensive shattering is present. 7. When the tibia has been extensively shattered directly below the knee-joint, so that fissures enter the joint, which can be ascertained without difficulty. In this case, too, it is advisable to delay amputation till the arrival of the patient in the hospital, if it be near. 8. When the tibia and fibula have been fractured and extensively shattered by a ball. Under various favourable circumstances, however, the leg may be preserved in this case. 9. When the tibia alone has been shattered to a large extent. In this case the leg may possibly be saved, but this is not very probable. 10. When the ankle-joint has been perforated by a ball in one of its longer diameters, so that extensive shattering or splintering of the tibia, or astragalus, or of both, may be inferred to have been produced. Simple grazing shots, with opening of the synovial membrane of the ankle-joint, permit of conservative treatment, with the exception of those cases in which a great portion of the malleolus externus has been lost; an injury which causes the foot to take the shape of a valgus, and makes it useless. 11. When the anterior part of the foot has been crushed by heavy shot. The only lesion of the foot by rifle-balls requiring amputation is shattering of the astragalus as a component part of the ankle-joint.

Period for primary amputation.—The majority of modern surgeons adhere to the principle which was introduced into practice by English and French military surgeons during the Napoleonic wars, of amputating as soon as possible. Though Hutchison says the amputating knife should follow the shot as speedily as possible, yet most surgeons wait until the wounded man has recovered from the first shock. The three Schleswig-Holstein campaigns gave new proofs of the correctness of these principles. Amputations within the first twenty-four hours give the best chances for the preservation of life. On the second day the prognosis is somewhat more unfavourable, if the wounded have been conveyed to a distance and the limbs are infiltrated. On the third and fourth days the prognosis is worst, but improves gradually from the sixth day.

Period for secondary amputation.—This operation should be performed: 1. In cases of gangrene, where it is not likely that a stump suitable for healing will be formed. 2. In cases of arterial hæmorrhage complicated with gun-shot fracture, where the hæmorrhage cannot be arrested by the ligature at a distance from the wound. 3. In cases of profuse suppuration. 4. For deformity or uselessness of a limb. 5. In all cases in which the diagnosis has been established too late to admit of primary amputation. From his experience, Dr. Stromeyer gives a series of practical rules regarding secondary amputation. It should not be performed upon limbs which are infiltrated as far as the trunk. The inflammatory symptoms should be allowed to subside. The author thinks it of the highest importance to follow the advice of Hennen, and to remove patients upon whom secondary amputation has been performed from a crowded hospital, and to place them where they can have a free supply of pure air. He goes even farther than Hennen, and is of opinion that the operation should only be performed after the patient has been removed into a better atmosphere, where the most favourable period for operating may be watched.

We shall next bring before our readers the subject of the resection of joints on the field of battle. These operations have been introduced into military surgery through the Schleswig-Holstein war. The number of resections from chronic disease of the joints will daily become more limited in the ratio of the progress made in the knowledge of their pathology and treatment. Gun-shot fractures of joints act, however, at once, and decisively, in producing danger to limb or life, while in chronic diseases of joints a similar amount of injury only appears after months or years, and is often due to unskilful management on the part of the surgeon.

Up to a recent period, two causes tended to retard the introduction of resections of joints into military surgery: the first was, that these operations were placed on a level with resections in the continuity of bones; the second, that chloroform was unknown. Bilguer, whose example was followed by most surgeons, saw no difference between resections of joints and resections in the continuity, notwithstanding his admission, on several occasions, that lesions of joints particularly required the operation. But the difference between the anatomical conditions of both cases is so considerable, that they do not at all present the same therapeutical indications. Dr. Stromeyer proceeds to discuss these anatomical differences, and shows why simple incisions into the joints, as practised by Ambroise Paré, are not more humane than the proceedings of that good citizen of the town of Gotham, who, in order to save his dog the pain of amputation of the ears, removed them by small pieces, which he cut off daily. Even if the result of that conservative treatment be not altogether what is desired, ankylosis may be obtained, except, perhaps, where the loss of bone happens to be so considerable as to preclude subsequent osseous union. But the few casual mishaps attending excision are counterbalanced by the circumstance that it at once renders all operations unnecessary, prevents the patient from suffering severely, and permits us to hope for the formation of a movable joint.

In 1839, Dr. Stromeyer saw a young man at Würzburg upon whom Professor Textor had performed resection of the elbow-joint for complicated fracture and dislocation. The mobility of the joint was so perfectly restored and so powerful, that Dr. Stromeyer already, at that time, inferred that resections of joints would find their most successful and useful applications on the field of battle. The advantages of resections of joints did not escape the great practical genius of Larrey, although he was unfavourable to resections in the continuity of bones, as Roux stated at the inauguration of his statue. But Larrey had so many amputations and exarticulations to perform, that no time remained for him to perform resections during the great battles at which he was present. Moreover, it is only since the introduction of anæsthetic agents that resections have become generally applicable. The operation is too prolonged in comparison with amputation, so that, without the assistance of chloroform, it would have still to yield to the shorter operation. Resections should be performed on the field of battle, if time and circumstances permit. If the wounded are in a state of great excitement, they will not easily be brought under the influence of chloroform; this happened frequently during the battle of Idstedt. If time be limited, and attempts to produce anæsthesia fail, the wounded may be removed. At all events, we may hold it to be true of resections equally with amputations, that the sooner they are performed the better. The wound heals in scarcely less time than after amputation, and if the operation be performed early, there is more chance of the formation of a flexible joint. It is a known fact that cicatrices are large and unyielding in proportion to the time occupied in the process of repair. With regard to the cause of bony union after resections, Dr. Stromeyer differs from Dr. Esmarch, who assumes it to be brought on by the want of passive movements at the proper time. He regards it as demonstrated that the period at which the operation was performed exerted the most important influence upon the production of osseous union. The practical applicability of resection is as yet limited to the shoulder and elbow-joints. The lower extremities oppose to the application of this proceeding many difficulties, which the author analyses without attempting their removal. But his ardent wish is that some means might be devised to supersede amputation of the thigh for gun-shot fracture of the knee. It is indeed very repugnant to the feelings to be obliged to remove an otherwise perfectly sound leg, on account of a small hole in the knee-joint, and, at the same time, to have but an imperfect assurance that we are thereby preserving life.

The subject of resection of joints after gun-shot wounds has been fully treated in Dr. Esmarch's work, which contains the details of all the cases observed during the Schleswig-Holstein war. It is only the second part of this author's work which treats of gun-shot wounds of joints, the first part being devoted to the consideration of wounds of the diaphyses of bones by rifle-balls, and their treatment. Although every part of Dr. Esmarch's work is deserving of our attention, yet the chapter On Resection of the Elbow-Joint is, perhaps, peculiarly adapted to illustrate the great and perfect success which followed the introduction of this operation into military surgery. Of forty patients upon whom excision of the elbow-joint was performed, six died. In one case the fore-arm became gangrenous, and had to be removed afterwards. One case was not yet cured, and remained in hospital as late as 1852, as we see from Dr. Niese's statistics. This case was for some time under our own care in 1850, and presented peculiar difficulties, through the circumstance of the ball having been divided into several parts, of which we withdrew one flat piece from the neighbourhood of the necrosed middle of the humerus through one of the fistulous canals that had formed. The remaining thirty-two patients were all perfectly cured, and retained a more or less useful limb. In eight of these cases the flexibility and general mobility of the arm at the elbow-joint was very extensive; in nine cases the mobility was more or less perfect, in thirteen cases ankylosis took place, and of two the ultimate issue is not known. Resection of the elbow-joint involves much less danger to life than amputation of the upper arm. The latter operation had a fatal result in nineteen out of fifty-four cases, the former in only six out of forty. We hope that the attention of military surgeons has, before this, been drawn to the contents of Dr. Esmarch's work, and that the doctrines and principles put forth therein have already been tested, which, we believe, if done fairly, will certainly be successful.

The second division of Dr. Stromeyer's work treats of the injuries of the separate parts of the body. The first section of this part describes the injuries of the head as they present themselves to the surgeon on the field of battle. The second section is an analysis of the symptoms to which injuries of the head make the wounded liable. To this subject a space of 134 pages is devoted, a proof in itself of the importance the author attaches to it. We shall give a few extracts from this section, and first bring before our readers the subject of concussion of the brain.

The disorders in the functions of the brain must be limited by the physiological properties of the organ, and by those of its covering. These limits are by no means narrow, as the consequences of concussion without lesion of the skull may range from slight faintness to sudden death. None of these conditions have been sufficiently cleared up by pathological anatomy; and it is not too much to say, that even the physiological conditions which come under consideration in these cases, have not been sufficiently attended to in practice. With regard to its compressibility or elasticity, the brain may be analogous to water. And as water is nearly incompressible, every passing impression received by the elastic skull would by necessity be transferred to the whole mass of the brain, if ever the channels of the blood and cerebro-spinal fluid were entirely closed up. But as these passages are open, every impression received by the skull must be combined with a displacement of fluids, which circumstance makes it very difficult, if not impossible, to determine accurately what occurs within the skull at the moment of concussion. Professor Bruns has made some experiments to determine the elasticity of the skull. A head was fixed in a vice between two small boards, so that, on screwing the branches of the vice together, the head could not escape, but was compressed in the direction of its transverse or its longitudinal diameter. The measurements were taken with the aid of compasses on four points of the skull laid bare for that purpose, and this was repeated after every second or third turning of the screw. It was found that the skull could undergo considerable compression without being fractured, and could return to its normal shape after cessation of the external force; and that the skull enlarged as much in one direction as it was compressed in another. But, says Dr. Stromeyer, though the skull may be compressed fifteen millimetres in its transverse diameter, and may recover its former shape, yet this skull on being subjected to a second experiment will sometimes break on application of a much slighter compression than that applied at first; showing that on the first compression some interstitial fractures must have taken place, which yet permitted the skull to maintain its original elasticity. And moreover, the slow action and effect of a screw cannot be compared with the results of a force rapidly applied. These experiments, however, show, at all events, that the skull can undergo considerable changes of shape, and yet by the aid of its elasticity may return to its normal conformation. This external integrity of the skull is the starting point for examining concussions of the brain. Lesions of the diploë of the skull do not come under consideration at present, since all injuries of the brain occurring in a skull whose shape was not altered, passed as concussions. From the experiments of Professor Bruns, it is now evident to what considerable injury, within the range of its elasticity, an organ must be exposed which, like the brain, consists of an easily lacerable substance.

Dupuytren was the first to draw attention to the circumstance, that contusion of the brain might be produced without injury to the bony covering, and attributed the possibility of its occurrence to the elasticity of the skull. He was borne out by Von Walther, who said that every concussion of the brain was combined with some contusion of that organ. Dr. Stromeyer goes a step farther, and thinks that concussion of the brain is in fact nothing else but contusion of that organ, which in its momentary compression may have suffered many and different injuries in its substance.

The author adopts the ordinary division of concussion of the brain into three forms:—1. That which is directly fatal; 2. That in which the consequences last some length of time; 3. That in which the effects pass off quickly. The effusion of blood and serum, found after concussion that has proved directly fatal, is explained by the vacuum which the compressed skull forms on re-expanding. The symptoms of the second form of concussion are described, and a physiological explanation is given of the two most constant features of concussion of the brain—weakness of the heart's action and vomiting. The discovery of E. H. Weber, of the influence of irritation of the pneumogastric nerves upon the heart's action in making it irregular, and when increased, of stopping the action altogether, has been brought to bear on the question of concussion; and the author discusses at some length an opinion of Dr. Stilling, the well-known neurologist, according to which the consequences of concussion of the brain concentrate in the region of the roots or "nuclei" of the pneumogastric nerves, below the fourth ventricle, because these are the softest parts of the brain, and therefore must suffer prominently from contusion of the skull, or compression, which we generally call concussion. This opinion is brought in parallel with the results of dissections performed by Rokitansky, Nélaton, and Sanson. The therapeutical indications are concise and simple. The horizontal posture must be maintained till the circulation has recovered itself; when the face gets a little more colour, the head is gradually raised. On the pulse becoming quicker, cold applications to the head are recommended as the best means to prevent capillary apoplexy. Ice must be used with care. Venesection is indicated by a strong, hard pulse, and by the colour of the face. Enemata and purgatives should not be forgotten, nor is calomel to be omitted on inflammation setting in. Arnica, Dr. Stromeyer does not apply; he states that he occasionally threw it out of the window, when he found it in the room of a patient with lesion of the head. The weakness of the heart's action prevents congestion and inflammation of the brain, and should therefore not be interfered with by the exhibition of stimulants. Not without anxiety, but without being led away to false measures, Dr. Stromeyer, in 1851, treated a girl of twenty-five years of age for concussion of the brain, who had fallen head foremost from a height of twenty-five feet, and lay for a fortnight in a state of torpor. Under the application of cold, with one venesection, the state of the pulse not admitting of a second one, complete consciousness returned after a fortnight, and the girl recovered.

Pressure on the Brain.—Pressure on the brain is the consequence of a diminution of the cavity of the skull, caused by foreign bodies, depressed fragments of bone, extravasated blood, serous and purulent exudations, and congestion of the bloodvessels. A review of the different physiological conditions of the brain in relation to the varying proportions of venous and arterial blood, to the interstitial serum and the cerebro-spinal fluid, and to the influence of expiration and inspiration, forms a particularly interesting chapter, which constitutes the foundation of the successful method of treatment of many depressions of the skull.

The principle, which was discovered empirically, is, by antiphlogistic treatment to prevent the brain from swelling so much as to be pressed against the depressed bone. If the depressed piece keeps its situation, the continuance of pressure produces atrophy of that part of the brain which is more directly pressed upon. Extravasations within the skull, which are so important in civil practice, are of little importance in gun-shot wounds of the skull. Those who know the symptoms of gun-shot wounds with depression, will never be liable to confound their immediate symptoms, or their symptoms at the period of congestion, with those of traumatic extravasation. The diagnosis of these two conditions is well established by Dr. Stromeyer, as well as the differential diagnosis of concussion from pressure on the brain. In the former, the symptoms are paleness of the face, a thin, weak pulse, quiet respiration, contracted or not materially altered pupils, and gradual decrease of the symptoms, which showed the highest intensity directly after the infliction of the injury. Pressure on the brain is accompanied by a flushed face, a full, slow pulse, snoring respiration, enlarged pupils, gradual increase of symptoms and paralysis of the body on the opposite side to that on which the head has been injured. Dr. Stromeyer has not observed a single case of gun-shot wound of the head in which he had been induced to attribute the symptoms to internal hæmorrhage. Dr. Beck relates an interesting case of an officer, who died on the fifth day after having received the wound. He felt well enough to dine with his friends, and to write letters the night before the morning on which he suddenly died. Besides the gun-shot fracture of the occiput, the post-mortem examination revealed the presence of a large effusion of blood into the cavity of the skull

The chapter On Congestive and Inflammatory Reaction of the Head contains censures on the present state of therapeutics and the do-nothing-ism of our days. *Osteitis cranii traumatica* is anatomically well illustrated. The double direction which, under some circumstances, the motion of the blood takes in the veins of the diploë, is exemplified by the remarkable encysted hæmorrhagic effusions of the skull described by Hecker, Dufour, and Stromeyer, which are produced by mechanical separation of the pericranium from the skull; the pouch being filled with blood from one of the large sinuses by means of one or more of the emissaria Santorini. These hæmorrhagic cysts (*Varix traumaticus*, Bruns) become distended by blood on any congestion of the brain taking place, and empty their contents into the sinuses as soon as the cause of congestion subsides. They represent on a large scale the physiological process by which blood is drained from the external part of the head by the sinuses of the dura mater.

A case, where a girl of sixteen lost her whole scalp in a mill, and escaped with her life, is related as a proof that the skull may be deprived of most of its bloodvessels without permanent damage. Hæmorrhages from the diploë during trephining, as observed by Dupuytren, Hecker, and others, are explained by the compression which the congested and expanded brain exerts upon its venous sinuses. The question is put to pathologists, What relation does this arrangement of the circulation of the head bear to pyæmia, and what share has it in the production of that disease? The following case has a direct bearing on the question of pyæmia from wounds and diseases of the head, and shows moreover how much caution must be exercised before believing a pathological fact to be complete:—

> "In 1850, at Rendsburgh, Dr. Stromeyer was present at the post-mortem examination of a man who was said to have died from intropulsion of erysipelas of the head. He had suffered from erythema of the right side of the face, which had disappeared after twenty-four hours. The man had fallen into a soporous state, and had died on the fourth day. Nothing worth noticing was found inside the skull. On opening the chest numerous pyæmic lobular abscesses were discovered in the lungs. Dr. Stromeyer now inquired whether the man had not had some small wound, and was informed, that before he entered the hospital he had a small boil just above the right eyebrow, which had been squeezed open by the surgeon of the regiment. Upon that the erythema set in, which caused him to be sent to the hospital. Though the seat of the small furuncle was not any longer clearly perceptible, yet Dr. Stromeyer caused the vein in the neighbourhood of the spot to be laid free, and found it filled with pus up to where it entered the facial vein. From this insignificant place, therefore, the blood had been impregnated with pus. The short distance which the pus had to traverse in this case, in order to make its way into the right side of the heart, explains to some extent the rapid progress of the case."

Dr. Stromeyer afterwards saw at Kiel two similar cases of fatal boils of the upper lip. Professor Weber has collected seven cases of this kind, inclusive of Dr. Stromeyer's cases, which occurred at Kiel or in its neighbourhood.

From all this, and from the fact that death always ensues under cerebral symptoms, it is likely that pyæmia occurring in patients with gun-shot wounds of the head, is the result of the entrance of pus through the large venous canals in the interior of the head.

Reaction of the Brain and its Membranes.—The direct consequence of any lesion of the brain or skull is a tendency to hyperæmia, which easily passes into inflammation and its terminations. The inflammation following wounds of the head may be of two kinds,—acute (primary) or chronic (secondary); the former developing itself out of the hyperæmia, which follows the lesions directly; the latter showing itself at a later period, even after weeks or months of apparently perfect good health. Primary encephalitis, with its congestive, erethic, and paralytic states, the author describes in a practical manner. The post-mortem appearances are given; and of these we mention, as a remarkable feature, the fibrinous concretions in the sinuses and in the veins of the cerebral membranes, into which they may be traced and followed for several inches. In secondary inflammation of the brain, the concretions are most frequently met with in the longitudinal sinus. They are, it seems, a natural sequel of acute encephalitis, and are formed of necessity whenever swelling of the brain compresses the superficial veins, if at the same time hyperinosis renders the blood more liable to coagulation. Coma and paralytic symptoms denote the formation of these concretions.

The modifications of inflammation of the brain by complications—such as concussion, internal hæmorrhage, or depression—are made subjects of serious inquiry. On correct views regarding these points will depend the solution of the question, whether trephining should be resorted to or condemned.

The author gives a description of the transformations which the injured substance of the brain may undergo. The following is a brief summary of the intra-cranial lesions adverted to:

1. If the dura mater be injured, no further symptoms may arise. In this case the compressed part of the brain fills again with blood, after removal of the pressure, as Guthrie observed after extraction of depressed fragments or removal of extravasated blood.

2. The patient dies, and the substance of the brain is found, underneath the injured spot, in a state of red softening.

3. The dura mater is injured, and the contused brain comes into contact with the air. This case generally has a fatal issue, as was observed by Schmucker. Dr. Stromeyer relates an illustrative case. The patient died of gangrene of the contused part of the brain, because the line of demarcation failed to be formed. This demarcation must be a double one; firstly, in the brain itself, and secondly, on the surface of the brain, so that a cohesion of this organ with the dura mater may take place, in order to protect the so-called sac of the dura mater against the admission of pus and ichor. Most patients die from the difficulties which stand in the way of the accomplishment of this process.

4. The dura mater has been disintegrated by the original injury, or has been cut into intentionally after trephining, or has been destroyed in the circumference of the wound by gangrene or ulceration. The case of a soldier is related, who was shot in the left part of the frontal bone. A fortnight after the injury had been inflicted, the brain protruded from the two openings in the frontal bone, in consequence, as was found, of an abscess, three inches long, in the left anterior lobe, which had caused this enormous expansion of the brain (fungus cerebri). This case illustrates, also, the next-mentioned transformation of the contused part of the brain.

5. An abscess is formed, with or without expansion.* Von Walther says, that these abscesses are sometimes of a size beyond all conception.

6. The contused part of the brain remains constantly under the compression of the depressed part of the skull. It was known centuries since, that depressions of the skull might be healed without producing severe symptoms, and without leaving paralysis or impairment of mental powers. These observations did not fail to make an impression upon unprejudiced observers; and accordingly we find at all times some surgeons, at least, opposing the use of the trepan, and relying upon the efforts of nature. Three hundred years ago, Lafranchi, of Milan, said, that in fractures of the skull everything depended upon the assistance of the Holy Ghost, which the surgeon should implore above all things; trephining he found rarely necessary. The healing by nature of depressions of the skull failed to make a lasting impression, because such cases were believed to be rare exceptions. Surgeons, therefore, went on trephining till they found by experience that in large hospitals recovery after this operation is an exception. It is only within the last twenty or thirty years that a more rational system has been followed, and that the operation of trephining is daily being confined within narrower limits.

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* Th. Fr. von Walther, System der Chirurgie, vol. ii. 1847, p. 56, § 93.

The question which now presents itself is, what advantages are offered in a case of compression of the brain by a treatment which leaves the depressed pieces of skull undisturbed in their place? The following may be enumerated: 1. The wounded part is not irritated by a renewed injury. 2. The early congestion of the contused part of the brain, and the formation of extravasations, are avoided. 3. The air does not obtain free entrance to the dura mater, to the sac of the arachnoid, or to the brain. The advantages of subcutaneous wounds are sufficiently known, and will be appreciated with regard to the skull and brain, as well as to other parts.

The author devotes several pages to the description of pyæmic inflammation following wounds of the head. Secondary encephalitis and ostitis engage the author to some length. Typhus and delirium tremens wind up the pathology.

Treatment.—Three hundred years ago, Lafranchi established the same limitation of the indications for trephining, which a surgical genius of our days—Dieffenbach—derived a second time from his practice. These principles Dr. Stromeyer found to be correct from his own experience, regarding the fatality of the operation of trephining, and the recovery after considerable depression of the skull without that operation.

Dr. Stromeyer relates a series of cases, from his earliest surgical experience, in the year of the great comet (1811), to the present time. He records his experience during the seven years he devoted to the study of his profession. During three years out of these seven he attended the hospitals at Berlin, Vienna, London, and Paris, and yet he did not meet a single case in which the operation of trephining had been successfully resorted to, while many severe wounds of the skull came under his observation which recovered without any operation. Notwithstanding this, he adhered to the views taught by Dease and Astley Cooper, and supported by Brodie by the aid of statistics from the London hospitals, that in complicated fractures of the skull trephining must be resorted to, because of the threatening formation of pus. Further experience led the author to doubt the correctness of these views, principally because it became apparent to him that the air must exercise a deleterious influence upon a contused part of the brain, no matter whether it be admitted by trephining or by the simple removal of loose fragments of the skull. The latter operation had a very unfavourable issue in a case at Freiburg, in 1848, and in another on the field, in 1849. Cotemporaneously, Professor Langenbeck performed two similar operations at Flensburg, which shortly after terminated fatally.

After the battle of Kolding, in Schleswig (April 23rd, 1849), there were eight gun-shot fractures of the skull, with depression, and more or less considerable brain symptoms, in the hospitals at Kolding, Christiansfelde, and Hadersleben. In all these cases, with only one exception, the detachment of the fragments was left to nature. The whole eight patients recovered perfectly. One patient, from whom some fragments were removed on the seventh day, was placed in considerable danger by this treatment. Dr. Stromeyer therefore resolved never to adopt it again. The treatment which brought these eight cases to a favourable issue was neither expectant nor operative, but simply antiphlogistic. Dr.

Stromeyer recommended it to the younger surgeons, and had the pleasure of seeing that all depressed gun-shot fractures of the skull from small-arms, no matter whether they injure the brain and dura mater or not, may be cured after this plan, without paralysis or interference with the other functions of the brain remaining, even when the state of sopor had lasted for weeks together. In 1850, after the storming of Friedrichstadt, in Schleswig, two young surgeons came under Dr. Stromeyer's care with gun-shot wounds of the head, accompanied by deep depression; they were both subjected to the non-operative treatment. In the first case, no venesection was required; in the second case life was only preserved by a venesection. Both recovered perfectly. A third case, that of a fusileer, terminated fatally from encephalitis, the surgeon in attendance upon him shunning bloodletting.

From the two campaigns of 1849 and 1850, Dr. Stromeyer possesses the notes of forty-one gun-shot fractures of the skull, with depression, in which there was no doubt about the existence of fracture of the skull, because it was denuded. It is, however, doubtful whether the brain or the dura mater were injured, because this can only be ascertained by the escape of cerebral matter from the wound, or by extracting fragments at an early period. Of these forty-one cases, seven terminated fatally, one from abscess of the liver, one from typhus, two from primary encephalitis through neglect of antiphlogistic treatment, two from phlebitis encephalica, one from secondary encephalitis, in consequence of the patient visiting a public-house. Thirty-four were cured, of whom one had been trephined by Dr. Ross, who afterwards described the case in the 'Deutsche Klinik.' This is the only case of trephining which gave a favourable result in all three campaigns. A soldier was wounded in the head at the storming of Friedrichstadt, and was transported by rail to Altona. He had been doing well for several days, when the symptoms of acute encephalitis caused Dr. Ross to trephine him. A detached piece of the internal lamina was found under a slightly depressed part of the frontal bone. Notwithstanding the operation, seven venesections and sixty leeches were necessary to combat the continuing inflammation of the brain. It is to this energetic antiphlogistic treatment, and not to trephining, that Dr. Stromeyer attributes the successful termination of the case. The highest number of venesections ever employed by Dr. Stromeyer in cases of gun-shot fracture, when trephining was not performed, was five; this occurred in a single case only, in 1849, when there was no ice to be had. Dr. Ross's case may serve to illustrate the disadvantages which trephining inflicts, even after healing of the wound. His patient, after being cured, was accidentally struck at the trephined place with the end of a whip-lash, or cord, which caused the reappearance of dangerous cerebral symptoms. Two cases in which the operation of trephining was resorted to unnecessarily by Dr. Beck and Dr. Ross, are severely criticised by Dr. Stromeyer, in which, we have no doubt, our readers will fully coincide.

Gun-shot wounds of the head should, in their recent state, be examined with great care by the aid of the finger or the probe alone. They must not at first be dilated, under any pretext whatever, whether for the sake of diagnosis or of prophylactic treatment.

The exclusion of air, the presence of which favours the decomposition of the secretions of the wound, is indicated in all cases. This is best accomplished by a piece of fine linen, which, when damp, adheres perfectly to the wound, and is to be removed at long intervals only. It is covered by some dry charpie, above which is placed a wet compress; a net made for the purpose fixes the whole dressing. Cold applications are made over the net. When these are discontinued the piece of linen next to the wound is moistened with oil. Every patient with a wound of the head must be well watched. The best advice Sir Astley Cooper gives in his Lectures on Wounds of the Head, is to visit a patient suffering from concussion of the brain at least three times a-day. In all cases of wounds of the head great care should be taken not to let the right period for venesection pass by. With regard to local treatment, the cautious extraction of perfectly loose fragments and foreign bodies stands foremost. The removal of impacted balls should not be attempted. No fault must be avoided more carefully than that of attempting the extraction of necrosed pieces of bone at too early a period, since it produces no harm whatever to leave them longer than is actually necessary. Small incisions of the wound for the purpose of extraction should not be resorted to before the third week.

These principles are by no means new, nor is it necessary to turn to the declared adversaries of trephining in order to meet with them. Hennen, who in considerable depressions of the skull trephined only when there were brain symptoms which did not at once yield to depletion of the vessels, says that it was not absolutely necessary to trephine for depressed fragments, though nobody would be hazardous enough to leave fragments which could be easily removed. He mentions the case of a man who, with a funnel-shaped depression one inch and a half deep, lived for thirteen years, and enjoyed a comfortable existence, provided he did not drink too much. Hennen had several specimens of that kind in his possession.

Space does not permit us to repeat the whole of the author's arguments. They necessarily lead to the conclusion that, if depressions of the skull do not in themselves indicate trephining, neither do they so even when, under the influence of reaction, unconsciousness and paralytic symptoms make their appearance. They only indicate those remedies which keep down the reaction, and trephining certainly cannot in the least be said to do this.

Dr. Stromeyer has, on principle, not trephined in two campaigns, and we have seen the results. After the exposition of his reasons, nobody, he hopes, will regard his disuse of the trephine merely as an experiment, but will accept it as the necessary result of observation and of correction of preconceived opinions. The strongest corroboration of the author's views regarding the treatment of compound gun-shot fractures of the skull is, perhaps, the statement of Mr. Rose, of the Coldstream Guards, recorded by Sir George Ballingall:—

> "On the 3rd of August, 1809, six days after the battle of Talavera, an order was given for all the wounded in hospital at the latter town, who could march, to leave it. Among those who undertook the march there were twelve or fourteen with wounds in the head, accompanied with injuries of the bone. At least four or five of these had both tables of the skull fractured, and two of them, along with

fracture of the os frontis, had each the globe of one eye totally destroyed. In none of them had the trephine been applied, nor had any attempt been made to remove splinters of bone. After leaving Talavera they were exposed to very severe fatigue. Every evening, after the day's march, Mr. Rose collected the wounded around him, and examined and washed their wounds, dressing with care those that particularly required it. Cold water was the principal application employed. The retreat occupied sixteen days, in spite of which, and with no other treatment than that which has been described, every one of those who were wounded in the head recovered."

Sir George Ballingall quotes this in proof of the good effects of cold applications; we lay equal stress on the non-interference. A future generation may perhaps base upon the above record a perambulatory treatment of gun-shot fractures of the head, the state of the brain permitting.

Dr. Stromeyer compares the results of his treatment with those obtained by the Nassau surgeons as described in the 'Nassauer Jahrbücher.' The indications for trephining given by the most recent German author, Professor Bruns, are reviewed, and the slight with which, by omission, he treats one of the greatest authors on the subject (Dieffenbach), is justly animadverted upon. Practical remarks on the general treatment of patients with injuries to the head, conclude this very elaborate chapter.

We cannot part with Dr. Stromeyer's work without giving expression to our sincere feeling of admiration for his numerous and great achievements in the surgery of war, and we are confident that all readers will derive as much pleasure and instruction from the perusal of this treatise, as we have derived in writing this review.

The work of Dr. Simon excited some interest at the time of its publication, from the fact that it contained some novel views on the nature of gun-shot wounds, the majority of which he opined to be closely related to tubular incised wounds, with loss of substance. Upon this theory he based his treatment of gun-shot wounds, the object of which was a cure *per primam intentionem.* It consists in removal of the contused border of the wound in the skin by the knife, suture, and compression of the canal, with the application of cold.

The chapters On the Treatment of Complicated Gun-shot Wounds, On Amputation, and On the Conservative Treatment of Gun-shot Fractures, principally those of the upper third of the femur, are greatly deserving of attention. The large amount of carefully observed and well related original matter imparts to the work a lasting value. We regret that we are compelled to content ourselves with briefly alluding to it.

Dr. Beck's volume contains one great impediment to the recognition of the author's merits; we mean the style of language in which it is written. But, though this style be heavy, unusual, and in some places of so confused a construction as to obscure the meaning of whole sentences or paragraphs, it is yet pervaded by that freshness which vigour and originality of mind impart to a pen; the great amount of very valuable original experience affords ample proof that the author's hand is more practised in the use of the knife than in that of the pen. Dr. Beck started for his five campaigns with a good anatomical foundation, and with the invaluable advantage of having been the assistant of Dr. Stromeyer. His principal experience he gained in the Italian campaigns, where he was employed as operator at the ambulances of the Austrian second Army-corps, and as surgeon to several Austrian military hospitals in succession. During eighteen months of the years 1848 and 1849 he was present at four battles, seven skirmishes, and a siege, and saw more than four thousand wounds, one-fourth of which he treated himself.

His work begins with an introduction, containing a short sketch of the position of the military surgeon in general, and of that of the Austrian surgeons in particular, to which we have already had occasion to refer. A sketch of the Austrian army medical department follows, and some improvements are suggested, of which one of the most important is the recommended institution of ambulances for every brigade, like those introduced in 1849 in the Schleswig-Holstein army by Dr. Stromeyer. Observations on the instrumentarium, and on the means and modes of transport of the wounded, conclude the introductory part of the work.

The general part treats of projectiles and the wounds they effect, of the physiological process in the wound and its neighbourhood, and of the treatment of gun-shot wounds in general. These chapters are written with due regard to the literature of the subject, but do not profess to contain any principle different from what has hitherto been taught. It is the same with the following chapters, which treat of the diseased action in wounds and their neighbourhood. Pyæmia engages the author for twenty pages. In this chapter we meet with the statement, that of ten pyæmic patients, seven have purulent deposits in the lungs. We pass by the concise statement of the extent to which pyæmia affected the patients under the author's care, and of the positive effect of the remedies he proposes at the end of this chapter. To phlegmonous inflammation only half a page is devoted—very little in a treatise which, from its title, professes to treat of gun-shot wounds, with all their relations and consequences. Of gangrene we hear little, and nothing of its prevention. Hospital gangrene has not come under the author's observation.

In the chapter On Hæmorrhage from Gun-shot Wounds, the author founds his remarks on the basis of his practical experience. Ligature in the wound itself he only advocates after grazing shots, and lacerated wounds from fragments of shells. In wounds of larger vessels, the trunk should always be ligatured, as, for example, the femoral, below Poupart's ligament, when the crural artery is injured in its lower third. With regard to this point Dr. Beck is at variance with Dr. Stromeyer, who recommends ligature in the wound wherever it is practicable, but insists on the ligature being applied to both ends of the severed artery, an operation the evident advantages of which escaped Dr. Beck. He performed ligature of the femoral below Poupart's ligament in two cases, of which the first, hopeless from the extent of the wound, ended fatally. The second case had a favourable result, notwithstanding several severe hæmorrhages before and after the operation. In another case the brachial artery was ligatured successfully. Dr. Beck corroborates Guthrie's statement, that a wound of the femoral artery requiring an operation, complicated with a simple fracture of the femur, is a case for immediate amputation.

In two cases Dr. Beck observed diffuse aneurysm after wounds of arteries, and had to perform secondary amputation in consequence. The first case (thigh) had an unfavourable result, the second one (upper arm) was cured.

Of tetanus nine cases are recorded, which all terminated fatally.

We received the work of Dr. Schwartz so late, that we were unable to allot to its review a space at all proportionate to its merits. Under the modest title of 'Contributions,' the author gives a most elaborate treatise on gun-shot wounds, exclusively, as he states in the preface, upon the basis of his own experience. He had charge of the first line of hospitals, which received the severely wounded soldiers, particularly those with gun-shot fractures.

From the number of cases which the author relates, we conclude that his experience in the treatment of the latter has been considerable, and entitles his opinion, even where it is at variance with the opinion of high authorities, to great consideration. We particularly noticed that the author does not advocate venesection in the treatment of complicated gun-shot fractures, that he regards secondary amputation admissible, at any time after the lesion, in all cases where it is possible to perform the operation in perfectly sound parts. In some chapters the absence of a careful comparison of the author's experience with what was known before him, a fault of which German authors are not generally guilty, is very perceptible; but on the whole the author deserves the highest credit for the assiduity with which so great an amount of material has been collected, the clearness with which it has been related, and the modesty which characterizes his language, wherever his opinion differs from that of others.

We have completed our task. Our object has been to place before our readers some of the peculiar features of military surgery as now taught and practised in Germany. It will, we hope, be felt that the great reputation of Dr. Stromeyer as a civil surgeon has been enhanced by his achievements on the field of battle. We trust that our readers may be induced to turn to the pages of the original.

The importance of the subject, which now attracts more than ordinary attention, can scarcely be exaggerated; the functions of the medical man in relation to navies and armies are even yet not sufficiently understood by those to whom the supreme control of our armaments is entrusted; but the wounded look to him as their solace and their aid, as is well expressed in the following quaint words of an old author:[*]—"Patent certantibus campi; jam corpora procumbunt humi truncata; membra late dispersa sternuntur; manat undique cruor; salus una restat moribundis; vocant hominis amicum; Ecce chirurgus!"

J. L. W. Thudichum.

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[*] Thes. in Par. Chirurg. Sch., 1763.

REVIEW VI.

1. *Ueber die Wirkung des Nordsee-Bades. Eine Physiologisch-Chemische Untersuchung.* Von Dr. F. W. BENEKE.—*Göttingen*, 1855. 4to. pp. 129.

On the Effects of Residence at the Watering-Places of the North Sea. A Physiological and Chemical Investigation. By Dr. F. W. BENEKE.

2. *Der Stoffwechsel. Eine Physiologisch-Chemische Untersuchung.* Von Dr. F. BIDDER und Dr. C. SCHMIDT, Professoren in Dorpat.—*Mitau und Leipzig*, 1852. 8vo.

The Metamorphosis of Tissue. A Physiological and Chemical Research. By Dr. F. BIDDER and Dr. C. SCHMIDT, &c. &c.

CHANGE of residence from London to the sea-shore exercises on almost every one a wonderfully invigorating influence. The man advanced in life, already perhaps painfully conscious of the approach of old age, often finds that the buoyancy and vigour of youth are suddenly restored; while the young man experiences an increased aptitude for muscular exertion, and a sharpened appetite for all the pleasures of life. In certain diseased conditions the influence of sea air as a curative agent is even more astonishing. In all the forms of that protean disease, struma, sea air is as much *the* remedy *par excellence*, as quina in ague and periodic neuralgia, or as iodide of potassium in chronic rheumatism. In children especially, whenever we can be sure that we have to do with pure struma without any intermixture of a syphilitic taint, we may predict that sea air alone will exercise a curative influence incomparably greater than that of any other remedy, or succession of remedies, however judiciously combined.

A visit to the Sea-bathing Infirmary at Margate will afford to any one who may be inclined to imagine that we are over-stating our point, an accumulation of evidence to the contrary. There he will see carious tarsi, which, according to the ordinary principles of surgery, suggest no other alternative as regards treatment than that between Syme and Chopart; joints already condemned to excision; diseased cervical vertebræ, in which a little further progress must be sudden and certain death; hideous cases of lupus, or of so-called glandular swellings,—in all these and many other forms, the immediate result of removal to the sea air is the manifestation of a tendency to a favourable termination which did not before exist. Ill-conditioned sores, the sure indices of dead bone beneath, assume a healthy aspect; sequestra, which under other circumstances would require the knife for their removal, here become imprisoned in newly-formed bone, and disappear; and a disease to the duration of which, according to a very high authority, "there is no natural limit, except the life of the patient,"* terminates spontaneously in cure. Even in those cases in which a result quite so favourable is not obtained, and in which the knife cannot be wholly dispensed with, the surgeon operates under circumstances most conducive to a favourable result.

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* Syme's Surgery, p. 196.

Considerations similar to the above seem to have led Dr. Beneke to undertake the admirable series of researches, the title of which we have placed at the head of this review. During his long residence in this country as medical officer to the German Hospital, he visited the Margate Infirmary, and was as much struck with what he saw, as we have been. The facts were obvious and incontrovertible; but when he applied himself to the question, How are such results brought about? he found himself wholly in the dark, and discovered that scarcely an attempt had been made by any previous inquirer to assign to them a physiological explanation.

The desired opportunity at length presented itself, in a summer's residence on the island of Wangeroge, off the coast of Hanover. A series of researches were planned, which were to serve as a groundwork for a more complete study of the subject than had hitherto been attempted. The objects in view, and the means by which it was proposed to accomplish them, are explained as follows:

> "In all pharmaco-dynamical researches it is necessary to determine, in the first place, whether the therapeutical agent in question sensibly affects the organism, or, what is the same thing, modifies the exchange of its materials; secondly, what are the limits of this action, if it exist; and, lastly, in what way it is induced. The limits of the exchange of material under the normal conditions, and for a period immediately preceding the observation, must be ascertained with exactitude. During the application of the agent, the observations must be continued under vital conditions, which are in all other respects as nearly as possible normal; and further, after the agent has been discontinued, attention must be directed to its subsequent action." (p. 3.)

It must be obvious to the reader that a research successfully carried out on the principles here laid down, would yield a rich harvest of results, which, without considering their importance in relation to the therapeutical question, must possess the greatest value as a contribution to our as yet very scanty knowledge of the limits of the vital exchange of material which constitutes the vegetative life of man. Although the researches of Valentin, Boussingault, and above all, of Bidder and Schmidt, have taught us, with something like mathematical precision, the laws of the balance of nutrition in some of the lower animals, it must be admitted that as regards man this precision is entirely wanting.

Dr. Beneke is well aware of the extent and the difficulties of the task before him:

> "What is required is to keep a daily exact reckoning of the total income and outgoings of the body; the solid and fluid ingesta must be quantitatively and qualitatively determined. The employment, subjective state, distribution of time, and condition of the atmosphere, as regards density and temperature, must be observed. . . . The outgoings of the body by the kidneys, intestinal canal, and lungs, must be measured and analysed with the utmost attainable accuracy." (pp. 3, 4.)

In order that we may fully appreciate the difficulties of Dr. Beneke's problem—the solution of which, as regards the human subject, we may premise that neither he nor any other physiologist has yet accomplished—we shall lay before the reader the general method by which every research that has for its object the estimation of the factors and resultants of the vital exchange of materials, must be conducted. The general results which have been arrived at in the lower animals, will serve as a point of departure in the critical examination of Dr. Beneke's deductions.

In the investigation of the *normal* condition of the functions of organic life, that, namely, in which income and expenditure are equal, there are two problems which present themselves. The first is, to determine the quantity of incoming and outgoing material in relation to the weight of the whole body; the second, to ascertain the mutual relations of the individual co-efficients of which income or expenditure is made up. These last are divisible into two classes. Those belonging to the first class, in which it is absolutely essential that the greater number should be included, must all be determined directly by analysis or measurement. These being known, the values of the others, many of which are incapable of direct determination, may be arrived at indirectly. Such is the problem. The procedures required for its solution are alike difficult and laborious,—so much so, indeed, that until very recently, among the many who have attempted, none have completed the task. Within the last few years, however, Professors Bidder and Schmidt of Dorpat,—men no less remarkable for their penetrating insight into the mysteries of life, than for their prodigious industry—have succeeded, as regards the carnivorous animal, in tracing a continuous outline of the great process of nutrition, which in all its prominent points is an authentic rendering of nature. The cat is the example selected. The analyses on which the results in question are founded are extremely numerous, and must have involved an expense of time and labour which, to our English notions, seems almost incredible. From these the following table has been deduced, which it is hoped will serve as well to exhibit the form which the problem must in every case assume, whether in man or in the lower animals, as the solution which Bidder and Schmidt have afforded:

TABLE I.

Table exhibiting the Normal Balance of Material in the Cat,[*] estimated in terms of the thousandth part of the weight of the Body as unity.

In-coming.					Out-going.			
SOLID AND LIQUID.								
Aliment	Water.		60·4	*a.*	Water		50·9	*a.*
	Raw beef	Carbon	6·2	*b.*	Urine and fæces.	Carbon	0·65	*β.*
		Hydrogen	0·8	*c.*		Hydrogen	0·2	*γ.*
		Nitrogen	1·4	*d.*		Nitrogen	1·4	*δ.*
		Oxygen	2·2	*e.*		Oxygen	0·9	*ε.*
		Inorg. const.	0·55	*f.*		Inorganic constit.	0·55	*ζ.*
GASEOUS.								
Inspired oxygen			18·45	*z.*	Expired carbonic acid	Carbon.	5·55	*η.*
						Oxygen	14·85	*θ.*
					Pre-existing water		9·5	*φ.*
					Water formed by oxidation.	Hydrogen	0·6	*χ.*
						Oxygen	4·9	*ψ.*
					Nitrogen		0·0	*ω.*
Total			90·00		Total		90·00	

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[*] The above table is constructed on the basis of the results of the first period of research in the second series, pp. 333—336.

We have indicated the determinable factors or resultants by the beginning letters of the alphabet, the quantities of each of them entering or leaving the organism being estimated by direct analysis or measurement. Those, on the other hand, which lie out of the reach of analytical processes, are successively estimated as follows:—$\phi = a — \alpha$; $\chi = c — \gamma$; ψ being the quantity of oxygen required to combine with χ, so as to form water $= 8\chi$; $\omega = d — \delta = 0$. It was found that, of the nitrogen entering the organism in the form of albuminous compounds in the food, almost the whole was eliminated as a constituent of urea, the fæces containing not more than 0·1 per cent.[*] The expired oxygen $x = \theta + \psi — (e — \epsilon)$. The oxygen contained in the food, e, is more than double of that which passes out in the form of urea and fæces. This excess $(e — \epsilon)$ is expired in the form of carbonic acid or water. After deducting it from the whole quantity of oxygen so eliminated, $(\theta + \psi)$, we have a remainder, x, which, as it can have arisen from no other source, must have been derived from the atmosphere.

One of the most remarkable results to be deduced from Bidder and Schmidt's determinations, is the discovery of a relation between the resultants of the two great processes by which, on the one hand carbon, on the other nitrogen, are eliminated from the system; a relation of the greatest importance in its bearing on the general doctrine of nutrition. This relation corresponds in a most remarkable manner to that which exists between the two elements in question, in the composition of albumen. From this composition it follows that the quantity of nitrogen, 1·4, indicated in the table as entering normally into the exchange of material, would correspond to 9·8 of that substance; this would contain 5·35 carbon and ·66 hydrogen. The quantities of either element existing in the secretions, viz., β and γ, being deducted, the remainders, 4·46 hydrogen, and 4·7 carbon, imply the amount of each resulting from the disintegration of the albuminous compounds, and available for the respiratory process. The comparison of these with the values of ϵ and χ in the table, will show only an inconsiderable excess in favour of the latter. In other series of researches,[†] in which there was, instead of equilibrium, a daily gain of weight of 1·9 per cent., the quantities ϵ and χ were completely covered by the residues in question; so that it appears that in vigorous health respiration was carried on entirely at the expense of the albuminous compounds.[‡] During inanition,[§] on the other hand, this relation was reversed. The activity of the respiratory process was somewhat greater than in the normal condition, so that the mean values of η for four successive periods of four weeks each, were 5·68, 5·74, 6·21, 6·28; while, on the other hand, the successive values of δ for the same period were 0·95, 0·98, 0·87, 0·89; exhibiting an immediate and very considerable diminution in the quantity of nitrogen eliminated, which, owever, was not progressive—so that the amount of plastic material

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[*] In the cat-tribe the organic constituents of the urine are made up entirely of urea. Uric acid is wanting, or exists as a mere trace. Hence the facility with which the former substance can be separated and estimated in these animals.

[†] Bidder and Schmidt, p. 336—339.

[‡] Les substances plastiques ne prennent qu'une part fort restreinte à la production de la chaleur animale. Liebig, Nouvelles Lettres, p. 137.

[§] Bidder and Schmidt in op. cit., p. 308—338.

(albumen) which entered into the organic exchange, must have been at least 33 per cent. less than in the normal condition. The increased proportion of carbon and hydrogen must have been derived from a source abounding in these elements, but free from nitrogen; so that we may conclude that, in the starving animal, which lives upon the components of its own body, a much larger proportion of the fatty as compared with the albuminous constituents is employed in the maintenance of life, than in the animal under the normal condition. Or, to express the same deduction in other words, the relation which exists between the quantity of nitrogen eliminated by the excretions and that of carbon expired ($\delta:\eta$), is not a constant one, but varies directly as the sum of the incoming and outgoing material. It was found that in the cat this quantity is capable of very great variation consistently with the maintenance of health; and that even when provided with as much animal food as it would eat, viz., from four to five times as much as was required to maintain life without loss of weight, the outgoing material, while it was correspondingly increased, still exhibited in the mutual proportions of its constituents the relations which we have pointed out.

The above are examples of the valuable results at which Bidder and Schmidt have arrived. In man, unfortunately, the determination of some of the most important resultants of the exchange of material—those, namely, by means of which the elimination of nitrogen, and of carbon in the form of carbonic acid, must be measured—is attended with much greater difficulty. The carbonic acid which passes into the atmosphere by cutaneous transpiration, cannot be measured by any direct process; and even the estimation of the carbonic acid expired by the lungs can only be effected at such an expense of time and labour as to make it almost impossible to repeat the analysis sufficiently often to arrive at safe results. As regards the elimination of nitrogen, it has been shown by Bischoff* that in man, under some circumstances, urea is by no means to be considered as an index of the exchange of material; and that, on the contrary, sometimes a larger, sometimes a smaller proportion of the nitrogenous constituents of the body is eliminated in this form, the rest passing out either in the bile by the intestinal canal, or as carbonate of ammonia by the skin and lungs. Whatever value we attach to these conclusions, it is clear that, in the present state of science, we have no reliable means of estimating the amount of nitrogen extricated from the human organism in any given time.

We cannot better represent the aspect of the inquiry in the present stage of its progress, than by comparing it to the chart of an imperfectly discovered country, on which lines of coast, mouths of rivers, and positions of lakes and mountains, are here and there accurately laid down, while between them there are large spaces, corresponding to tracts of unexplored territory. Just as the geographer, guided by considerations of greater or less weight, marks out in dotted lines the contour of islands or continents, so the physiologist completes the outline of the great process of nutrition in man, aided by determinations derived from the lower animals. Still, the whole remains a mere provisional delineation, in which those parts only have value or permanence which are the transcripts of nature; while the rest, however like nature, are but the traces of that which has only an ideal existence. Let us, above all, be cautious, lest we give to our dotted lines the authority of facts, and by long accustoming ourselves to an imaginary physiology of nutrition, forget that the permanent solution of the problem is yet far off.

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* Der Harnstoff als Maas des Stoffwechsels, p. 142 et seq. Giessen, 1853.

Dr. Beneke does not pretend to have solved it. "How many questions," says he, "still remain open; how inadequate must my labours appear to serve as the groundwork for general conclusions." He describes as follows the method and order of his observations:

> "My investigations were especially directed to the following questions:
>
> "1. What influence does mere residence at the sea-side exercise on the exchange of material?
>
> "2. How is this influence modified by daily bathing?
>
> "3. What influence does the bath exercise, immediately after its employment, on the exchange of material, as compared with the effect produced in the whole twenty-four hours?
>
> "4. Is it true that residence at the sea and sea-bathing induce subsequent emaciation?
>
> "5. What further objective or subjective phenomena affecting the health are to be considered as constant effects of these agents?
>
> "For the solution of these questions, as complete an acquaintance as possible with my own exchange of material was, in the first place, necessary. A research of five days in January, and another of fifteen days in February, were therefore undertaken; the urine being daily analysed, and the evacuations and solid ingesta estimated as accurately as possible. . . . From the 5th to the 8th of July, special preliminary researches were undertaken at Oldenburg (Dr. Beneke's residence), the urine being analysed twice daily; and on the 15th of July the researches in Wangeroge began, the 10th and 11th being occupied in the journey. Four days were devoted to the study of the effects of residence on the island without bathing; and the following seven days to the additional effects produced by a daily bath." (pp. 6, 7.)

From the 24th of July to the 4th of August no observations were made, Dr. Beneke judging it advisable, on hygienic grounds, not to continue his laborious researches too long; but on the latter day they were again commenced, and persevered in till the 12th, a bath being taken daily, with the exception of the 6th and 7th. Lastly, after Dr. Beneke's return to Oldenburg, similar observations were made, which extended over two periods of three days each. In all these researches the weight of the body was accurately determined each morning. Of the processes employed in the analysis of the urine we are told nothing, except that the urea was estimated after Liebig's method—viz., by precipitation with a solution of protonitrate of mercury of fixed strength.

In order to avoid the necessity of repetition, we have arranged the mean results of the seven series of researches in the following Table. The numbers which express the proportions of the constituents of the urine we have given in terms of the mean weight of the body at the period of observation. The absolute values, as given by Dr. Beneke, are apt, if we overlook the influence of the fluctuations in this standard, to lead to fallacious conclusions:

TABLE II.

Time and Place of Observation.	Quantity of Fluid Ingesta.	Mean Weight of Dr. B. at period of Observation.	Total Quantity of Urine.	Constituents of the Urine, estimated in terms of a thousandth part of the mean weight as unity.				
				Urea.	Uric Acid.	Sulphuric Acid.	Phosphoric Acid.	Chlorine.
1st Period: Oldenburg, January, 1854.	Litres 1·353	Kilogrammes 64·0	Litres 1·226	0·4201	0·00464	0·032	0·0367	0·1643
2nd Period: Oldenburg, February, 1854.	1·404	64·0	1·408	0·384	Not determined.	0·02637	0·03756	0·187
3rd Period: Oldenburg, July 5—9, 1854.	1·921	60·62	1·317	0·403	0·0069	0·02316	0·04607	0·1668
4th Period: Wangeroge, July 13—17.	1·894	61·11	1·469	0·45	0·0035	0·0275	0·039	0·1734
5th Period: Wangeroge, July 17—24.	2·096	61·25	1·290	0·4622	0·005	0·0309	0·0427	0·1523
6th Period: Wangeroge, August 4—12.	1·920	62·63	1·440	0·456	0·00517	0·0296	0·0426	0·208
7th Period: Oldenburg, August and Sept.	1·947	63·20	1·467	0·39	0·003	0·022	0·0315	0·2015

During the second period of observation, Dr. Beneke lost daily ·05 per cent., his original weight being sixty-four kilogrammes. This loss cannot be accounted for by deficiency of food, but should rather be attributed to the impairment of the powers of assimilation by prolonged sedentary employments. From the mean quantities of aliments of different kinds employed during the first ten days of the first period, we have endeavoured to estimate approximatively the quantity of nitrogen which must have entered the digestive apparatus. According to our deductions, which are founded on the analyses of Horsford and Schlossberger, the daily aliment of all kinds must have contained 22·17 of nitrogen—a quantity corresponding to 47·6 grammes of urea; whereas only 24·6 grammes were actually excreted daily. If we take into account the animal food alone, we have 12·8 grammes of nitrogen, corresponding to 27·4 of urea. Now although, as above noticed, there is reason to believe that a certain proportion of nitrogen finds its way out of the system by other means, this cannot be estimated at more than ten grammes daily, and probably falls very far short of that quantity. We are therefore led to the conclusion that a quantity of nitrogenous material, corresponding to at least ten grammes daily of nitrogen, must have passed undigested through the alimentary canal.

A series of researches undertaken by Dr. Böcker, of Bonn, with a similar object,—namely, to ascertain the limits of his own exchange of material—afforded to him the following mean values:—Urea, 0·4781; uric acid, 0·0048; sulphuric acid, 0·0386; phosphoric acid, 0·039; chlorine, 0·156;[*] which, although considerably above those obtained by Dr. Beneke, correspond closely with them in their mutual quantitative relations. Bischoff gives ·434 as the corresponding value for urea in a healthy man weighing sixty-seven kilogrammes.

During the third period, extending from the 5th to the 9th of July, the urine passed in the morning hours (viz., before 1 P.M.) was analysed separately from that passed during the rest of the day. Solid food was used *ad libitum*, it having been found that all attempts to estimate it accurately were unsuccessful. The fluids, however, were still measured. In correspondence with other observations, it was found that while in winter the total amount of urine passed daily exceeded that of the fluid ingesta by about 0·9 per cent., in summer it was only 68 per cent. of the latter. This, of course, corresponds to a considerable increase of perspiration. From the Table it appears the urea was considerably increased; the absolute daily quantity being, however, in the second series, 24·59, in the third, 24·43, the mean weight of the body having diminished from sixty-four kilogrammes to 60·62. The proportion of sulphuric acid was somewhat diminished, that of phosphoric increased; the uric acid was increased by one-half.

During the fourth period of observation, in Wangeroge, Dr. Beneke gained in weight by about 1 per cent. daily; the amounts of urea and sulphuric acid excreted were again increased about 20 per cent.; the quantity of sulphuric acid being, in the third period, $\frac{1}{17}$th, in the fourth, $\frac{1}{18}$th of that of the urea. The uric acid was diminished, and fell somewhat below its original limit. The phosphoric acid was likewise diminished, but not at all in the same proportion. Lastly, with the diminished supply of fluid, the total quantity of urine was visibly increased. From these facts Dr. Beneke draws the following conclusions: 1. That sea air induces a considerable diminution of the "cutaneous evaporation." This remarkable and important result, if confirmed, is in complete accordance with what we know of the physical condition of the atmosphere on the sea coast, which possesses an evaporating power about twenty per cent. less than inland.* 2. That while the increased secretion of urea and sulphuric acid points to the increased metamorphosis of the nitrogenous compounds, the inverse relation which presented itself as regards the uric and phosphoric acids must be accounted for in another way:

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[*] During the same period Dr. Böcker expired daily, according to his own estimate, 73,512 cubic centimetres of carbonic acid, which is equal to 1096·3 grammes. This is a much higher number than that obtained by other observers.

> "These undoubted facts necessitate the conclusion, that the uric and phosphoric acids are subject to different laws of metamorphosis from urea and sulphuric acid. Both the latter increase and diminish according to the greater or less quantity of the albuminous constituents of the food. The former, on the contrary, obviously vary inversely as the intensity of the process of oxidation. An elevated process of oxidation, such as unquestionably existed in this instance, increases the amount of urea and sulphuric acid excreted, but diminishes the proportion of uric and phosphoric acids. If the amount of phosphoric acid contained in the urine, like that of sulphuric acid, simply expressed the greater or less quantity of phosphates in the food, it would have been greater in Wangeroge than in Oldenburg, inasmuch as in the former place the same quantity of brown bread, rich in phosphates, was used, as in Oldenburg. The diminution, however, was not only relative, but absolute, which could have depended on nothing except on modifications of the metamorphosis of material, which induced diminished excretion of phosphoric acid, and resulted in its retention in the organism. If we inquire into the nature of these modifications, we must rest contented without a completely satisfactory answer." (p. 47.)

Dr. Beneke, however, offers an explanation by which he thinks the difficulty may be solved:

> "A retarded metamorphosis of the nitrogenous compounds finds its expression in deficient oxidation of the uric acid and of the products of its disintegration, particularly the oxalic acid; a deficient oxidation of the last into carbonic acid induces a diminished solution and extrication from the organic nexus of the earthy phosphates belonging to it. Such an increased separation of earthy phosphates is, however, always connected with emaciation of the body, inasmuch as the phosphate of lime is a necessary requisite for cell formation. This condition is strikingly prominent in scrofulous, that is to say, atrophic, children; and daily observation teaches us that these children excrete with the urine a large quantity of uric and oxalic acids, and earthy phosphates." (p. 47.)

In another place he remarks, in relation to this doctrine:

> "If we remember the experiments of Wöhler and Frerichs, which show that after feeding with uric acid the proportion of urea is increased, while oxalic acid appears in the urine . . . . we gain an insight, I imagine, into the difficult relations of the process of nutrition, which is no less of value for pathology than for therapeutics. Excepting in conditions of actual inanition, the nitrogenous materials and fats are seldom deficient. The third requisite, however, for cell formation, phosphoric acid (and particularly the phosphate of lime), on account of its dependence on easily modified conditions of the metamorphosis of material,—viz., the formation of uric acid and oxalic acid—undergoes constant variations,

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* See page 100.

and, as I am convinced, is the sole cause, in the greater number of cases, of the manifold fluctuations in the weight of the body which present themselves to us in practical life." (pp. 96, 97.)

The doctrine which connects the origin of oxalic acid in the organism with the disintegration of uric acid was, in the first instance, derived from the fact that oxalic acid is one of the results obtained in the artificial oxidation of uric acid. The experiments of Frerichs, however, afford satisfactory ground for concluding that it is not mere playing with chemical formulæ to attribute the disappearance of uric acid in the organism to a process of oxidation. So that in so far as we are justified in forming any fixed conclusions in relation to so difficult a question as the physiological relations of uric acid, we think that Dr. Beneke has given the right explanation of the remarkable diminution of that constituent which presented itself to his observation. As regards the phosphoric acid, however, it seems possible to take a simpler view of the question. The principal resultants of the exchange of material may be divided into three classes: 1. The neutral azotized substances are necessarily thrown off by the kidneys. 2. The sulphuric and phosphoric acids leave the organism partly by the same channel, partly by the intestinal canal. 3. The organic acids, including grape sugar, are bodies capable of further oxidation, and their elements are, for the most part, eliminated by the lungs and skin as carbonic acid and water. Their appearance in more than the normal proportion in the urine—that, namely, which is sufficient to insure the slightly acid reaction of that secretion—must ever be considered as an indication of *diminished* oxidation, and a proof that they, not being converted with sufficient rapidity into their ultimate products, have accumulated in the blood to such an extent as to necessitate their expulsion in larger quantities by the kidneys.

If, now, we turn to the question of the earthy phosphates, we find that the very condition upon which their proportion in the urine depends (the efficient cause of that solubility which is the necessary condition of their existence in that secretion), is to be found in the presence of an excess of organic acids in the blood. From these considerations we infer that the fact on which Dr. Beneke lays so much stress, confirmed, as he asserts it to be, by former observations—viz., that the uric and phosphoric acids diminish in the urine with every increase of the intensity of the process of respiration—is precisely what we should expect from what we know of the general nature of that process. Such increased activity will induce a relative diminution in the amount of organic acids in the blood, a condition which is obviously favourable to the formation of basic phosphates. From Bidder and Schmidt's investigations, it appears that, under very varied conditions of nutrition, the quantitative relation of the phosphoric acid eliminated both by kidneys and intestines, to the amount of nitrogen excreted in the form of urea, and consequently to the activity of the vital exchange of material, is very constant. Thus, in a cat supplied with as much animal food as it could eat, the phosphoric acid held to the urea the relation 1: 19·6; while during inanition, when the mean secretion of urea was diminished to not more than 30 per cent. of its original value, the relation was, during the first four days, 1 : 18·24, during the second four, 1 : 17·67, and during the third, 1 : 16·80 : a difference which might depend rather on purely physical conditions—viz., the concentration of all the secretions, which increased as inanition advanced.

As Dr. Beneke has made no determinations of the quantities of phosphates of lime and magnesia contained in the fæces, it is impossible to answer the question in a positive manner. We may, however, suppose it probable that the fluctuations in question are not so much indicative of variation in the whole amount of earthy phosphates "extricated from the organic nexus," as of corresponding fluctuations in the balance existing between the proportions of earthy phosphates eliminated by the two channels which have been mentioned. So long as the other resultants of the exchange of material, and particularly the expired carbonic acid, remain undetermined, every theory involving an altered activity of the respiratory process must remain mere matter of inference.

During the fifth period, from July 17th to July 24th, a bath was taken daily at half-past six A.M. There was a mean gain of weight during the periods of 0·1 per cent. daily. The function of the intestinal canal was much increased, there being usually two abundant evacuations daily. The proportion of urea was but slightly increased, about 1¼ per cent., while that of sulphuric acid rose about 11 per cent. There was an increase in the uric acid of more than 40 per cent.; the phosphates, however, did not maintain the parallelism which had been before observed, their proportion being little above the previous one: 150 centimetres more of fluids were used than during the previous period; the quantity of urine excreted, however, was 180 less. This last result is the opposite of what Lehmann* of Rolandseck found to be the effect of the sitz bath on the amount of urine secreted. According to him, both the cold and hot sitz bath are followed by an immediate increase of the renal secretion, both as regards water and solid constituents, this diuretic effect being most marked an hour after the bath. In the present instance the diminution observed affected principally the morning hours, being, for the hours preceding one P.M., 40 per cent. of the original quantity, and for the others only 6 per cent.

As to the action of the bath on the constituents of the urine, Dr. Beneke observes:

"If in this case we were to consider the urea as the index of the exchange of material, we should, on the one hand, arrive at the conclusion that the bath only increases the action of the air by about one-fifth; while, on the other, we should have great difficulty in explaining the remaining results. Considering the fact that the sulphuric acid and urea have been hitherto observed to vary in the same relation to each other, it would be difficult to account for the great relative increase of the former in the present instance; so much the more as the number in question only expresses the quantity excreted by the urine, without taking into account that a larger quantity of sulphur was certainly eliminated as bile in the abundant stools than during the former period." (p. 60.)

From these considerations, Dr. Beneke infers that here the activity of the process of nutrition was rather indicated by the sulphuric acid than by the urea, grounding his assumption on the observations of Bischoff, already alluded to. (p. 89.)

"Is it not quite conceivable that during the period of bathing, in consequence of the increased functional activity of the skin and intestinal canal, the quantity of urea appearing in the urine is no longer to be considered as the expression of the quantity of metamorphosed nitrogenous constituents, but that a larger proportion of these than in former periods is lost in other forms? . . . . Is not the glutinous character of the perspiration which is observed in almost all bathers dependent on the increased excretion of carbonate of ammonia by the skin, which would form, with the fatty constituents of its secretion, a soapy compound?" (pp. 61, 62.)

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* Die Wirksamkeit der Sitzbäder auf die Vermehrung des Stoffwechsels. Arch. für w. Heilkunde, Band I. Heft 4. and Band ii. Heft 1.

The above explanations carry with them much of apparent probability, but unfortunately the close examination of Dr. Beneke's own facts will show that they are not well founded. If we compare the amounts of urea and sulphuric acid secreted immediately after the bath—viz., before one A.M.—with that secreted during the rest of the day, we find, in the first place, that the slight increase of urea, about 1·5 per cent. for the whole day, affected entirely the afternoon hours; in fact, the morning quantity was about 11 per cent. less than in the third series of observations, while the amount secreted for the rest of the day was greater in the same proportion. In precisely the same manner, the increase of the sulphuric acid, which for the whole day was 11 per cent., *belonged almost entirely to the afternoon hours.* It will be remembered that, in the former series, its quantity stood to that of the urea in the relation 1 : 18·8; during the morning hours the increase was so inconsiderable as only to alter this relation to 1 : 17·3, while for the rest of the day it was 1 : 14·2. We think these numbers are very far from favouring the conclusion that the bath exercised any immediate influence in increasing the disintegration of the nitrogenous constituents. Certainly the very trifling increase of sulphuric acid during the morning hours, even when estimated in relation to the urea, does not make it necessary to call in the aid of carbonate of ammonia to account for it consistently. Unfortunately, we cannot substitute a better explanation for Dr. Beneke's, but must leave the resolution of this question to further and more extensive observations.

The increased secretion of uric acid Dr. Beneke considers can only be looked upon as "an isolated fact, dependent on an absolute increase in the amount of uric acid formed;" this increase being so great, "that, in spite of the elevation of the process of oxidation, a larger quantity makes its appearance in the urine." As to the mode in which this is brought about our author says nothing, considering it attributable to a "special action of the bath;" and he thinks that in this mode of viewing it we have an explanation of the circumstance that "gouty persons, in whom uric acid seems to form a *materia peccans*, do not well bear sea-bathing." The increased amount of phosphoric acid was accounted for in the same manner as before—viz., on the ground that, in consequence of the increase of the uric acid, a larger quantity of oxalic acid was formed, and that more phosphates being by this means set free, more appeared in the urine. Dr. Beneke's reasoning seems to be more loose and speculative in this than in any other part of the work. Determined to find an illustration for his favourite theory, he has forced his facts into a construction which they do not naturally bear. Is there any reason for admitting or even supposing that the process by which uric acid is formed in the blood, is so far separable from the general process of the metamorphosis of the nitrogenous tissues, that we may treat it as a separate physiological function? Can it be imagined that, in a process of which each stage depends on all the preceding in the closest manner, we can have an "absolute increase" of one of its factors without affecting the rest? We know very little indeed about uric acid, but all that we do know tends to teach us that it is a transition product, and, consequently, that it must affect and be affected by its antecedents and successors in the most direct manner. It is at least probable that all the nitrogen which leaves the organism as a constituent of urea must have previously existed in the form of uric acid: if so, it is plainly unreasonable to suppose that the latter is capable of varying independently of the former.

From the 24th of July, on which day the fifth series of researches were concluded, to the 4th of August, there was a gain of weight in eleven days, during which no observation was made, of nearly 0·2 per cent. daily, which was the more remarkable, as the action of the alimentary canal continued to increase until there were two or three abundant motions daily. The quantity of urine passed during the morning hours continued less than when no bath was taken. The sixth series of researches commenced on the 4th of August, extending to the 12th. The action of the bath was now much less obvious than before. The proportions of urea secreted during the morning, as compared with the rest of the day, approaching more nearly to that observed during the fourth series. The quantity of fluid excreted, compared with that of the fluid ingesta, was considerably greater than during the preceding period, and it is especially worthy of remark, that for the third and fourth days of the present research, on which the bath was omitted, the results obtained differed very little from the averages. The relation of the sulphuric acid to the urea was 1 : 15· 4. An interesting observation was made on the 8th of August. Late in the evening Dr. Beneke took a walk of three hours. The following day all the constituents of the urine, except the phosphoric acid, were in much larger quantity than on any other occasion. The urea amounted to 0·524, the sulphuric acid being in the relation to it 1 : 15·9, while the increase of uric acid was even more marked, its numbers being 0·007. During the three previous days there had been a mean loss of weight of 226 grammes, but on the day in question there was an increase of 0·7 per cent. It is of course impossible to attach much importance to results so isolated; they are of interest, however, in relation to a practical fact, of which we have yearly examples—that Autumn pedestrians, after exchanging a sedentary life for one involving no inconsiderable bodily exertion, almost always return home with increased *embonpoint*.

The results of this series seem to us to lose much of their value from the state of Dr. Beneke's health at the time. During the first few days he suffered much from fatigue and exhaustion. This condition, which is the well-known result of too frequent sea-bathing, must necessarily be inconsistent with an active vegetative life. In the present instance it was attended with a considerable daily loss of weight. In relation to this fact our author remarks:

> "The wonderful cures of scrofulous, atrophic, rhachitic, and other children which I have seen on the English coast at Margate, have been explained as the result of the tranquil passing of the day in the invaluable sea air. Fatigue is always loss of strength, and rest gain, where the air has in itself the power of increasing the exchange of material and of elevating the process of oxidation." (p. 86.)

Dr. Beneke returned to Oldenburg on the 17th of August, but did not resume his observations till the 29th, on which day he commenced his seventh series. The most remarkable result obtained was that while the urea and sulphuric acid fell to their old levels at Oldenburg, the uric and phosphoric acids did *not* exhibit the same return, their numbers being considerably below those which presented themselves at any previous period. The whole series of observations extended over six days, which were divided, by an interval of eleven days, into two periods of three days each. During the last three—viz., about four weeks after the cessation of the daily bath—the diminution in question was much more marked than during the first, the means being, for uric acid, 0·0015, and for phosphoric acid, 0·0265. At the end of the period of observation, Dr. Beneke's weight was about the same as when the baths were discontinued. In stating that in the course of it he gained flesh considerably, and founding on that assumption a theory of the after-action of sea-bathing, he seems to have committed an oversight in the interpretation of his own figures—the actual mean weight during the whole of the seventh series being 63 kilog. 700 grammes, while on the last day of bathing it was, at the same hour, 63 kilog. 162 grammes. Could this difference be considered for a moment of sufficient importance to afford ground for a theory on the after operation of sea-bathing, when we find that during the stay at Wangeroge the variation amounted in one instance to 624 grammes? Still more surprising is the way in which the remarkable diminution of the uric acid is accounted for. Dr. Beneke thinks that, during sea-bathing, the material from which uric acid is produced is so rapidly transformed, that after the discontinuance of the agent "the store of it is for a while exhausted!" To such an explanation as this it is difficult to make a direct objection. As to the general conclusion—viz., that the subsequent action of the sea-bath consists in its promoting the formation of new material, it is very certain that the facts before us do not afford any sufficient ground for its adoption. Not only was there no increase of weight worth speaking of, but the proportion of nitrogen eliminated by the urine was small. As regards the diminution of the uric acid and phosphates, it does not seem possible in the present state of our knowledge to advance further in explanation than by connecting it with an increased relative activity of the respiratory process. Whether such increased activity ought to be attributed to a diminution or acceleration of the metamorphosis of material must remain undetermined, although, in accordance with the deductions of Bidder and Schmidt, we should be more inclined to the former than to the latter alternative.

We have now arrived with Dr. Beneke at the conclusion of his task. If we compare what he has effected with what was required in order that the various physiological questions which he has proposed may receive satisfactory answers, we must admit that it appears wholly inadequate. If, on the other hand, we contrast it with all that has been accomplished before, in the elucidation of the subject, Dr. Beneke stands distinguished, not only because he has pointed out the right path of investigation, but because he has himself followed it with pre-eminent industry and success.

It is the opinion of one of the highest authorities* that pure, and particularly physiological, chemistry is too little advanced to make it possible to hope for any success in its application to pathological questions. The observation applies with the same force to investigations on the actions of remedies. Our knowledge of the normal is so inexact, we have so few points of departure which are ascertained with anything like mathematical certainty, that, after all that has been done, it is only here and there that we feel ourselves on firm ground, either in pathology or therapeutics.

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* Lehmann, Physiol. Chemie, Band iii. s. 493.

The conclusions of Dr. Beneke may be stated categorically as follows:

I. Residence at the sea, without bathing, increases the excretions of the organism about 12 per cent.*

II. Sea-bathing induces, 1, an additional "acceleration of the exchange of material," which nearly equals that produced by the air itself; 2, an "absolutely increased production of uric acid;" 3, "an increased production of oxalic acid and excretion of phosphates."

III. The above effects of sea-bathing are most recognisable in the hours immediately succeeding the bath.

IV. The subsequent effect of residence at the sea is increased assimilation, as indicated by increase of weight of the body, and marked diminution of uric acid and phosphates.

Of these, the first only carries with it sufficient grounds for its acceptance.

The concluding pages of Dr. Beneke's work are devoted to the difficult question of the *modus operandi* of sea air. Admitting, as we do, the validity of his main conclusion,—viz., that sea air is a stimulant of the general process of vegetative life—the inquiry arises, What are the proximate causes of this result? After alluding to the various notions which have been advanced relative to the psychical impression which the aspect of the great sea produces on man, as well as to the supposed influence of the strongly reflected sunlight, to neither of which he attributes any importance, Dr. Beneke proceeds to notice the question of the influence of the greater barometric pressure of the atmosphere. In a recent paper in the 'Union Médicale,' by Dr. Pouget† of Bordeaux, the good results of sea air, particularly in early cases of phthisis, are attributed mainly to this condition, which he supposes "renders more complete the phenomena of hæmatosis and nutrition, and impresses on the economy a vital impulsion," &c. He principally supports his position on certain experiments by Dr. Pravaz of Lyons, on the efficacy of artificially compressed air as a therapeutic agent in this disease. Dr. Beneke attributes to the density of the air very little importance: 1. Because he found scarcely any difference in this respect between Wangeroge and Oldenburg; and 2. Because, as he correctly argues, the result would be in the opposite direction; for Vierordt found that under conditions in other respects similar, a rise of 5·6 mm. in the barometer is sufficient to induce a decrease of 0·309 per cent. in the proportion of carbonic acid in the expired air.

The hygrometric condition of the air appears to be of much greater importance. Lehmann[*] has shown that rabbits, placed in other respects under similar circumstances, expire fifty per cent. more carbonic acid in moist than in dry air. From this and other experiments he concludes that moist air exercises an accelerating influence on the respiratory process; whence it must necessarily follow that the quantities of carbonic acid and water expired in the same time vary inversely. Dr. Madden, in his work 'On the Climate of Torquay,' has promulgated the opinion that the evaporating power of the air is actually greater at that place than in others farther inland. We have made the following comparative calculations from the Meteorological Tables published by the Registrar-General for the years 1850—54. On the one side are ranged the mean values for the evaporating power[†] of the air at various places, all having a mean distance from the sea of about 120 miles; on the other, the corresponding numbers for the islands of Guernsey and Jersey.

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* It must ever be remembered that the norma cannot be assumed with any certainty, and that the condition of the functions of nutrition which existed at Wangeroge might as properly be considered to correspond with it as that which presented itself at Oldenburg. Whichever way we take it, the deduction is of the same value. Had it been possible for Dr. Beneke to avoid the mental fatigue which his laborious investigations involved, the results would doubtless have been more striking.

† De l'Influence de l'Atmosphère Maritime dans le traitement de la Phthisie, par le Dr. Pouget de Bordeaux: Union Médicale, Fev. 1855.

Table exhibiting the Mean Evaporating Power of the Atmosphere in the Channel Islands, and in the Midland Counties of England.

	Channel Islands.	120 miles inland.[‡]	Relation.
January to March . .	116·2	143·5	1 : 1·235
April to June	107·7	201·4	1 : 1·870
July to September . .	134·2	198·4	1 : 1·478
October to December .	140·7	124·05	1 : 0·8816

There is another subject closely connected with the one we have been considering, of equal if not greater interest, that, namely, of ozone. After explaining Schönbein's theory of the formation of this body, as a consequence of the excitation which atmospheric electricity communicates to the oxygen of the air, and particularly his observation relating to its power as a disinfectant, our author remarks, that we cannot but admit that the proportion of ozone in the air which we inhabit must be of the highest significance in relation to the general process of life and to the exchange of material. If we succeed in acquiring a knowledge of its actions, "we may hope that it will eventually prove the key to a more complete acquaintance with the undeniable influence which atmospheric electricity exercises on the organism." In order to ascertain whether or not there were indications of a large proportion of ozone in the air at Wangeroge, a series of observations were made on the sea-shore, at the height of about six feet from the ground. The ozonometric paper employed was that prepared by Bürgy at Basle, under Schönbein's superintendence. The strips were each exposed for twelve hours, and renewed night and morning. The indications afforded were very marked, and when compared with Schönbein's own observations seemed to indicate a very high proportion of ozone.

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[*] Loc. cit., Band iii. p. 387.

[†] The evaporating power of the air at any moment corresponds to the difference between the weight of aqueous vapour which a given volume of it contains, and the weight which would be required to saturate the same volume at the same temperature. It is conveniently expressed by a number corresponding to the value of this difference in terms of the latter quantity as unity, so that we have:

$$\text{Evaporating power} = \frac{\text{Weight of vapour actually present in any given volume,}}{\text{Weight of vapour required to saturate the same vol. at the same temp.}}$$

the former being of course equal to the weight of moisture required to saturate the same volume at the temperature of the dew point.

[‡] The numbers in this column are deduced from the mean results obtained at the observatories at Nottingham, Bedford, Cardington, and Linslade, the mean distance of which places from the sea is about 120 miles.

It is obviously unsafe, in the infancy of our knowledge of this subject, to bring forward any theory except with the greatest reserve and hesitation. We may, however, be allowed to remark, that if Schönbein's doctrine as to the origin of ozone be true, there must be a close relation between its production and the hygrometric condition of the atmosphere. All the phenomena of atmospheric electricity are accounted for by meteorologists on the ground of the existence of opposite electric tensions at the earth's surface, and in the higher strata of the atmosphere. The quantity of what Schönbein calls "strömende Electricität," which according to him is the necessary condition for the development of ozone, must obviously vary with the conducting power of the lower strata of the atmosphere; and as aqueous vapour is the great modifying agent, on the presence of which the conducting power depends, it becomes extremely probable that, in the hygrometric condition of the sea air, we have the most important co-efficient in the production of its physiological effects.

We subjoin, in conclusion, a summary of the results obtained by Dr. Esmarch of Kiel, in a series of experiments on the influence of the cold plunge bath on the pulse and temperature. Dr. Esmarch rose each day at five, and after dressing ascertained his pulse and temperature, and again on arriving at the sea-shore. Having rapidly undressed, he plunged thrice, swimming and moving actively in the water between each plunge, so as to occupy about three minutes. On leaving the water the pulse and temperature were observed, and again on arriving at home. In the course of the walk to the bath the temperature sank from one to two degrees, the weather being cold, while the pulse rose from twenty to twenty-four beats. During the bath the temperature fell about one degree, while the pulse rose about ten beats. During the ten minutes succeeding the bath, the temperature rose from one to two degrees, the pulse falling from ten to twenty beats. On arriving at home the pulse-number was, in the mean of twelve experiments, five per cent. less than on arriving at the sea-shore. Dr. Beneke confirms the above results, and adds, that some time after the bath the pulse always falls considerably below its original number. They are also in perfect accordance with the interesting results published by Dr. Sieveking on the influence of the shower-bath on the pulse.* In the mean of one series of twenty observations in which the shower-bath was used immediately after rising, the pulse numbers were, before the bath 67·90, and after it 61·33. All these facts point to the conclusion that both the plunge and shower-bath exercise a directly sedative influence on the heart's action—an effect which is probably closely associated with the increase of the cutaneous transpiration. It is much more instantaneous, and probably more powerful, in the shower than those in the plunge-bath, because in the latter it is in great measure warded off by the struggle with the waves or the exercise of swimming. In many persons long perseverance in swimming is followed by great and lasting exhaustion, which seems to be a joint result of the sedative action of the bath and the severity of the exercise.

J. Burdon Sanderson.

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* Ueber den Einfluss des kalten Sturzbades auf den Puls. Archiv für Heilkunde, Band II. Heft 1. It is worthy of note that although active exercise was used immediately after the bath, this was insufficient to raise the pulse to its natural standard.

REVIEW VII.

1. *On Lithotrity and Lithotomy.* By WILLIAM COULSON, Surgeon to St. Mary's Hospital, &c.—*London*, 1853. pp. 388.
2. *On the Relative Merit of the Two Operations for Stone.* Two Lectures delivered at the Royal College of Surgeons of England in 1854. By FRED. C. SKEY, F.R.S., Surgeon to St. Bartholomew's Hospital, &c.—*London*, 1854. pp. 55.
3. *Lithotomy Simplified, or a New Method of Operating for Stone in the Bladder.* By GEORGE ALLARTON, M.R.C.S., &c.—*London*, 1854. pp. 80.
4. *A Practical Treatise on the Urinary Organs.* By S. D. GROSS, M.D., Professor of Surgery in the University of Louisville, &c. Second Edition.—*Philadelphia*, 1855. pp. 925.
5. *Notes on Lithotrity, with an Account of the Results of the Operation in the Author's Practice.* By Sir B. C. BRODIE, Bart., D.C.L., V.P.R.S., &c. &c. A Paper read before the Medical and Chirurgical Society of London, March 13, 1855.—Published in the Society's 'Transactions' of the same year.

OF the many and varied topics which claim the attention and engage the interest of the practical surgeon, it may be affirmed, perhaps without fear of contradiction, that there is not one which proves more generally attractive than that of stone in the bladder. It is natural that it should be so. The "interesting case," in the language of the hospital *habitué*, is always one either of difficulty or danger; and stone in the bladder is often enough associated both with the one and the other. Then it happens also, in regard of this complaint, that the degree of success in treatment may, to a great extent, be fairly regarded as the measure of the operator's skill. Fortune does not favour the bungler, nor shield the errors of the ignorant and inexperienced; and treatment, when successful, as it so frequently is, constitutes indeed one of the noblest triumphs of our art. The miserable sufferer is speedily and most effectually relieved. His burden is not merely lightened, it is absolutely taken away. Most pertinently, indeed, did Deschamps, in his "Traité pratique et dogmatique de la Taille"—a source which all later writers have found rich in historical facts and early opinions respecting it—take for his motto, and affix to his title page the line, *Sublata causa, tollitur effectus.* No wonder that stone in the bladder becomes a subject of more than ordinary interest to the enterprising surgeon.

In the early days of our profession, when surgical art was associated with very little of surgical science, the discovery of a stone in a patient's bladder was pretty nearly tantamount to sentence of death. From the time of Celsus to the sixteenth century a chance of life was afforded by "cutting on the gripe," provided the patient was neither too old nor too young. Until so recent a period as about the year 1825, the knife was the only resource of the surgeon, and the patient was invariably "cut" for the stone. The last thirty years have witnessed the invention and the perfecting of another method, by which the knife is to a great extent superseded. So slow, however, has been its progress in this country as compared with that in France, so slender is the experience of English surgeons respecting it, that we have now for the first time to deal with any literature in our own language specially devoted to the consideration of the subject.

The only contribution to an exact knowledge of the value of lithotrity in this country, has just appeared in the form of a paper in the 'Medical and Chirurgical Transactions' just published, from the pen of Sir Benjamin Brodie. The profession is much indebted to that distinguished surgeon for a plain unvarnished tale of his past personal experience in the crushing of stone. He has unreservedly furnished the results of his practice, an act in itself not less worthy of our imitation, perhaps, than any other which could be selected from the many happy incidents of his successful career. May his example be followed by those who are emulous to attain a like experience. We shall return to Sir Benjamin's paper hereafter.

Mr. Coulson has supplied the only work which aims at presenting a systematic treatise 'On Lithotrity and Lithotomy.' A brief comparison of the two methods, the substance of which originally constituted two lectures delivered at the Royal College of Surgeons, appears in the form of a pamphlet from Mr. Skey. We shall first make a short examination of these, and afterwards compare their authors' views with those enunciated by a high American authority, Dr. Gross, of Louisville, whose work 'On the Urinary Organs' is well known in this country through its first edition. The rapid appearance of a second and considerably enlarged edition offers a chapter on the subject under consideration, which, as it contains no less printed matter than Mr. Coulson's octavo, may well be taken separately here. The design, as well as the limits allotted to this paper, do not permit us to extend our notice of the American work to any other portion of its contents, on this occasion.

Mr. Allarton's little volume is devoted solely to the recommendation of a peculiar mode of performing lithotomy, and will be discussed when that part of the subject comes under consideration.

The first chapter in Mr. Coulson's work is devoted to the history of lithotrity, and affords a brief but succinct account of the origin and progress of the method, throughout its multifarious stages, by which calculi have been removed from the urinary bladder without the aid of the knife. And while acknowledging many sources from which hints innumerable have been obtained, aiding to develop the grand result—viz., the accomplishment of lithotrity by the method now almost universally followed—"the right, not only to the discovery of the principle, but of the means by which it has been carried into practice," is claimed for M. Civiale. On the other hand, while it is admitted that M. Amussat was the first to propose a plan for destroying calculus by crushing, in contradistinction to M. Civiale's method by perforation, and also to design an instrument which was extremely inadequate for the purpose of effecting this object, we are told that "the instrumental portion of the crushing system unquestionably originated in England." We have no intention to open up this old and disputed question of priority, but feel it incumbent to say that we believe M. Civiale to have obtained a larger share of credit in this matter than the evidence respecting it appears to justify.

The second chapter, a very short one, describes the treatment preparatory to the operation of lithotrity, and presents little matter requiring notice. The subject of preparation is, however, held to be one of some importance by many operators, and we think it will not be deemed hypercritical to remark, in reference to this part of the subject, that we suspect Mr. Coulson very rarely finds the use of the lancet necessary as a preliminary measure in calculous patients, and that the following sentence is consequently to be regarded rather as a fragment of routine, than as a serious rule of practice:—"If, on the other hand, the circulation be much excited, general or local bleeding is indicated; but we must be cautious not to reduce the patient too much," &c. (p. 33.)

The manipulations which Mr. Coulson has found requisite in the practice of lithotrity, are described with some minuteness. He details the stages by which he is accustomed to proceed in dealing with a stone, and they appear to accord pretty nearly, as far as such matters can be judged of by verbal description, with the instructions which most lithotritists have given us in relation to this matter. Practical remarks from an experienced operator are, however, always valuable, and as such they shall be transcribed here:

> "We have now the instrument in the bladder. The next step is to seize the stone. This is easily done when the calculus is small, and the blades of the lithotrite large. The closed instrument is first directed towards the floor of the bladder, along which the curved part is made to pass, the point being gently turned, now to one side now to another; as soon as the instrument touches one of the sides of the stone, the female branch is fixed, the male branch is slowly drawn back, and the instrument is cautiously inclined towards the calculus. Should any difficulty be experienced in seizing the stone, from depression of the floor of the bladder or other causes, then the blades are moved in a sweeping direction, with the convexity downwards, from before backwards, along the bas-fond and posterior surface of the bladder; they are then drawn back a little, and the convexity directed to the right or left side, each of which is explored in its turn; lastly, the point of the instrument is directed upwards and then downwards to the space immediately behind the prostate. The instrument should then be carefully rotated, in order to come in contact with the foreign body. This found, the blades of the lithotrite are cautiously opened, and the instrument is pressed on the stone laterally, after which the blades are closed with the same caution, every effort being made to seize the stone as much towards its centre as is possible. This is an affair of dexterity, which practice and great tact alone can attain. It is of importance to remember that the female branch should be kept perfectly immovable while we are closing the instrument, otherwise we run the risk of displacing the stone, which generally lies against its ascending portion." (pp. 42—3.)

Side by side with this, and for the same reason, we will avail ourselves also of Mr. Skey's experience. Supposing the lithotrite to have been introduced into the bladder, he proceeds as follows:

> "In opening the blades, this rule appears to me important, to make each blade move equally from the centre between them, pressing the instrument forwards at the same instant that the near or convex blade is withdrawn. By this movement we avoid the painful pressure of the instrument against the neck of the bladder. If the neck of the bladder is touched by the blade, a start, or a movement, and an expression of suffering, invariably follows. The stone is now to be caught by the lithotrite, but in what manner? . . . . .
>
> "If, on having expanded the instrument, the lower blade be pressed downwards towards the rectum, by the elevation of the handle, the bladder will assume a

conical form, the apex of which is directed downwards. Into the apex of this cone the stone will fall *three times out of four;* and I believe I may say in a yet greater proportion. I have myself caught the stone on one occasion ten times in succession, and I have repeatedly fixed the stone nine times, the blades being expanded and closed twelve. No action can be more simple, or more easy of execution. If the stone adhere to the coats of the organ, or if it fail, from any other cause, to fall into the concave blade, a slight shake of the instrument, or, what is less annoying to the patient, a slight shake given to the pelvis with the open hand, will generally succeed.

"This mode of catching the stone is really so important as to be worthy repeated experiments on the dead subject, during which the remote blade should be pressed with moderate firmness against the bladder where it is in contact with the rectum, while the near blade is drawn out to the greatest capacity of the instrument, if the size of the stone be uncertain, and less so as it becomes reduced in size." (pp. 44—5.)

This description is trite and simple, and probably indicates a successful method in easy and uncomplicated cases. Cherishing, as we do, the utmost respect for the opinion of so able and experienced a surgeon as Mr. Skey, we are nevertheless compelled to regard the method described as likely to be sometimes inapplicable—that is, in those cases in which the bladder is somewhat irritable; and it is for the conduct of such, and not for simple cases, that the results of experience may become so valuable. We presume that the manipulation which consists in pressing downwards the lower blade towards the rectum, so as to impart to the bladder the form of a cone, of which the apex shall be downwards, can be successful only in the dead body, and that no such effect, however much the operator may desire it, can be obtained, or at all events ensured, with a permissible degree of force in the living. It was under similar circumstances, doubtless, that the stone was seized so many times in succession. But is it not doubtful whether such experiments can to any great extent guide our practice in the living body? We are compelled to be somewhat sceptical as to the advantage which is gained by practising lithotrity extensively upon the dead subject. Inestimably valuable—nay, necessary—as is such practice in respect of most other operations, surely it is not of equal value here. As regards the exercise of accustoming the operator to the mere handling of a lithotrite, it may have an elementary utility. But it must be well known to those who have so practised, that very little manipulative dexterity is called into exercise by the act of seizing and crushing a stone artificially introduced after death; and the practitioner will be deceived indeed who expects to find, in the uncontracting insensible bladder of the dead subject, anything like a parallel with that which he will meet with in the living. We think we have observed, that the pressing downwards of a lithotrite towards the rectum will, by exciting a strong contraction, sometimes completely fail in producing the desired cone in the living, although that manœuvre infallibly produces it in the dead body. The difficulties of lithotrity arise from the very fact that the bladder is not an insensible inactive bag; that the main obstacles presented to the operator are those very circumstances which the mysterious element of life involves: and the best—may we not say the only?—school in which to prepare for the emergencies of lithotrity, is that which affords constant exercise in the use of instruments in the living urethra and bladder, whatever the purpose for which they may be employed.

Sir Benjamin Brodie's method of procedure, as described by himself, is characterized by extreme caution at every step. The italics are his own:

"The rule should be to *move the forceps in the bladder as little as possible*, never using it as a sound for the purpose of exploring the bladder or ascertaining the position of the calculus. Such an examination does not assist the surgeon in seizing the calculus afterwards; it gives pain to the patient, excites the bladder to contract and expel the water which had been previously injected; and I know that instances have occurred, though not in my own practice, in which a rough handling of the forceps has caused great injury to the bladder, ending in the death of the patient. The rule for seizing the calculus (which I must acknowledge to have first learned from witnessing the very dexterous operations of M. Heurteloup) is as simple as possible. The patient lying on his back, the handle of the forceps is elevated, which of course brings the convex part of the curved extremity of it in contact with the posterior surface of the bladder, where it is contiguous to the rectum. The forceps is then to be opened, by withdrawing the sliding blade to a greater or less extent, according to the probable size of the calculus, the fixed blade being at the same time pressed gently downwards in the direction of the rectum. The object of this manipulation is, that the forceps, being below the level of the other parts of the bladder, the calculus may fall into it by its own weight; and it is generally successful. If it should not do so, the forceps, without being moved from its situation, may be gently struck with the hand on one side, or on its anterior part, and the slight concussion thus communicated to the bladder will probably be sufficient to discharge the calculus, and bring it within the grasp of the instrument. If it should be otherwise, the forceps, being closed, may be very gently and cautiously turned to one side or the other, so that the curved extremity of it may make an angle of twenty-five, or even thirty, degrees with the vertical line of the body, then opened, and passed in the direction of the rectum in the manner already described." (pp. 175-76.)

Sir Benjamin Brodie strongly objects, we know, to the turning round the lithotrite in the bladder, so as to point the beak towards the *bas fond* of the organ, a manœuvre described by Mr. Coulson as suitable for the purpose of seizing a stone which may perhaps lie behind an enlarged prostate, and not easily be caught otherwise. These are points worthy of mention, although they only can be settled by each operator for himself, by his own experience. It might be dogmatical for any one to assert that his own method was exclusively the safe and efficient one. Probably the only principle of universal application—and it is beyond all question the most important—is, that extreme care, gentleness, and delicacy of touch in the employment of the instruments engaged are absolutely necessary, not only to the achievement of success in removing the foreign body and for the comfort of the patient, but even for the preservation of life itself. There is no hazard to the patient so great as that which the rude or forcible use of instruments in the bladder involves. It is, indeed, impossible to overrate the importance of acting constantly under the influence of this conviction.

The injurious consequences, or "the accidents of lithotrity," pass next under review. That of most frequent occurrence is impaction of fragments in the course of the urethra. After this, severe pain following the operation; hæmorrhage into the bladder; retention of urine from atony or paralysis of the bladder; rarely, suppression; sub-acute inflammation of the bladder; extravasation of urine and perineal abscess; peritonitis, very rarely; rigors and febrile attacks; disturbance of the nervous system; fatal collapse, occurring usually in cases in which renal disease has previously existed; and finally, the ultimate retention of a fragment or fragments within the bladder, are enumerated and considered, with more or less detail, by each author.

The first-named is a very frequent source of anxiety to the surgeon, and of distress to the patient. It is said to occur, on an average, about once in four times. The methods commonly advised to rid the urethra of the impacted fragment are—gentle pressure; a large bougie, with a view to push it into the bladder if it be situated far back in the urethra, or a current of water directed along the canal; and, as a last resource only, the attempt to crush it with a species of lithotrite sufficiently small to work within the urethra, or with some similar instrument. If it be situated anteriorly to the bulbous portion, long and slender forceps, in a variety of forms, may be employed for the purpose of extracting it, or a common probe may sometimes suffice; and all these methods failing, an incision into the urethra from without. Sir Benjamin Brodie adds a method strongly characteristic of the safe and cautious practice which his experience has developed. It is evidently his chief resource, and must be noticed in his own words:

> "I cannot say that the forceps may never be required where a fragment is lodged in the posterior part of the urethra, or even that it may not be necessary to extract it by an incision in the perineum. Such a case, however, has not occurred in my own practice. It so happens that I have always succeeded in dislodging the fragment by two or three times introducing a rather small gum-elastic catheter, and thus altering the position which it occupied in the urethra." (p. 180.)

The French surgeons have exercised much ingenuity in the construction of instruments for the purpose of crushing calculous fragments impacted in the urethra, and appear to use them with considerable freedom. Such impaction is liable to occur under two very different conditions. The one, in adults, when the bladder is unusually irritable, and fragments are driven with preternatural force as far as to the membranous portion of the urethra, where they become detained. The other, in children in whom the neck of the bladder and adjacent part of the urethra are very dilatable, the prostate gland being very slightly developed. In these cases, frequently treated as they are by the lithotrite in France, impaction at the membranous portion is an exceedingly common occurrence, and extremely small urethral lithotrites are used in order to remove the obstructing body, a procedure which requires more than ordinary delicacy and caution, as may be well imagined. Thus, M. Ségalas, who has practised lithotrity to some extent on children of all ages—that is, from twenty-two months and upwards—speaking of their liability to become the subject of impaction, and of its treatment, makes the following statement:

> "I have found urethral lithotrity always possible, but sometimes very difficult, in children. I do not think there is much to choose between it and the *boutonnière*, but I think we should try the former in all cases."

Hæmorrhage, to an extent that at all approaches danger, is an accident which probably ought to be, and is, of rare occurrence; certainly a much less frequent result of the operation than it was formerly believed to be. With regard to the other consequences named, the reader may refer for useful hints to the works of each author named.

There is one very important question which naturally arises from an examination of the results of operative proceedings for the removal of calculus from the bladder—whether by lithotomy or lithotrity, which appears not to have been satisfactorily answered, nor, indeed, fully discussed—it is this: From which operation may be anticipated the better chances of success, other things being equal, when some signs of organic renal disease exist? Is lithotrity under these circumstances a more hazardous procedure than lithotomy? The importance of this inquiry is referred to by Mr. Coulson in the following passage:

> "The secreting organs themselves often become implicated during the progress of calculous affections of the bladder. . . . . These secondary diseases constitute a most important point in relation to lithotrity, for they exercise more influence on the results of the operation than even direct lesions of the bladder itself, while at the same time they are of so obscure and insidious a nature as to escape, in many cases, the notice of experienced practitioners." (p. 106.)

M. Civiale, it is pretty generally known, asserts that lithotomy has a greater tendency than lithotrity to exasperate an existing lesion of the kidneys. On the other hand, a contrary belief seems to obtain among English surgeons generally. Thus, Mr. Coulson says:

> "If we are to select lithotrity, it should, in my opinion, be only when the renal disease is not very far advanced, and when the condition of the bladder and nature of the calculus lead us to conclude that the operation may be completed within a short period." (p. 192.)

Further on he observes:

> "We may, however, have a case in which the symptoms of renal disease are obscure, while the patient earnestly entreats to be relieved from his more pressing and palpable malady. Here, it may be asked, supposing an operation to be decided on, whether we should select lithotomy or lithotrity. This comes to the same as if we asked which of the two operations is the more likely to develop the renal disease already existing. I confess that my mind is not quite made up on this difficult point." (pp. 244-45.)

Mr. Skey, speaking of lithotrity, remarks:

> "If disease exists, it is our duty, if possible, to detect it by inquiry and examination, and to reject the case as inapposite to the operation." (p. 26.)

If either this (incipient renal disease) or other lesions referred to are present, he believes that—

> "It is preferable to resort to lithotomy, because it presents the better prospect of a successful issue. It is more probable that the bladder, or even the kidney, will recover under one positive shock to its system, which encompasses the primary object of the removal of the stone, than from the repetition of a crisis of irritation," &c. (p. 27.)

The existence of considerable or confirmed renal disease is recognised pretty generally in this country as contra-indicating the employment of lithotrity; but the influence which incipient or more obscure but still recognisable forms or stages of diseased kidney may exert upon the result of either operation, has yet to be accurately determined by a rigorous comparison. This can only be accomplished by the practice of instituting a careful examination, in all calculous cases, of those circumstances which tend to throw light upon the diagnosis of renal disease. Thus, an examination of the patient's urine by microscopical and chemical tests should never be omitted, and its results on two or three separate occasions should be recorded for the purposes of clinical study. We are by no means prepared to say that a small quantity of albumen, or the occasional appearance of a few casts in the urine, should be held to determine the nature of the operation to be resorted to; but the observation of these and other signs of renal disease, in connexion with the results of subsequent operative measures, will tend to clear up existing obscurity. Undoubtedly, in the presence of the circumstances named, either operation will be more than ordinarily hazardous; but the comparative influence of the morbid condition upon either remains to be shown, we think, by future experiences, and it is a subject well deserving a painstaking and systematic inquiry.

Another subject in which carefully conducted researches will afford valuable information is that of the reappearance of calculus subsequent to operative measures for its removal. M. Civiale has investigated it to some extent, and Mr. Coulson has added the result of further inquiries. Important as the results arrived at are, there is confessedly a large field yet to be explored in connexion with this matter. After an examination of the records of the Norwich Hospital, the statistics which M. Civiale has collected from the hospitals of France and other continental countries, and the returns from the Luneville Hospital in Poland, on behalf of lithotomy; and of the outlines of the five hundred and forty-eight cases which M. Civiale states to have been subjected by him to the operation of crushing, on behalf of lithotrity, the following approximative result is arrived at: That reappearance of the calculus occurs after the operation of lithotrity in about one in every ten cases, and after lithotomy in about one in sixty. Without entering upon any detailed discussion of this question, we may state that there is some reason to believe that the reappearance of calculus after lithotomy is perhaps rather understated here. Whether or no, the difference noted between the results of the two operations must in great part be attributed to the inefficiency of lithotrity, even in the hands of the most experienced operator, to remove the whole of the concretion from the bladder. The persistence of the calculous diathesis must be held responsible for the reappearance of the calculus in a certain number of instances, probably in a much greater proportion than one in sixty, and the excess which appears to the disadvantage of lithotrity, whatever it be, must be attributed to some defect belonging to that method. Sir B. Brodie's practice seems, however, to have afforded a somewhat better result. Admitting that it requires extreme care and vigilance to arrive at anything like an absolute certainty that no fragment is left, he still says that the instances in his own practice are few in which a recurrence of the symptoms might be reasonably attributed to some fragment having been overlooked in the first instance.

We cannot forbear suggesting, in connexion with this question, whether it has not been somewhat overlooked by writers on calculus, that the presence of stone in the bladder, and of the "calculous diathesis," are by no means necessarily concomitant circumstances? Phosphatic stones are often the result of a merely local derangement of the bladder; as, for example, in cases of obstruction of the urethra, from stricture or enlarged prostate, producing a condition of the lining membrane of the bladder in which mucus is largely secreted, and phosphatic matter deposited. On the same principle, uric acid stones are frequently coated with phosphatic matter, but simply as a result of the *local* action referred to. May it not indeed be affirmed that phosphatic calculus is generally, if not always, a local affection, whereas uric acid and oxalic acid formations may, as far as we know, be considered as constitutional in their origin? The reappearance, then, of a phosphatic stone is not the result of a calculous diathesis, but of the persistence of a local disease in the mucous membrane of the bladder. We are not aware that this distinction has been sufficiently insisted on, at all events in connexion with this question, and believe that it is one of importance, beyond the bearing which it manifestly possesses on the subject of relapse now under consideration.

An important topic, embracing the "Indications and contra-indications of lithotrity," is very carefully treated by Mr. Coulson in the eighth chapter of his work. Assuming that a certain number of the cases which are presented to the surgeon are adapted to lithotrity, he arrives at a conclusion, based upon the united testimony of those who have published their experience on this subject, which may thus be briefly stated:—that of *adult* calculous patients, two-thirds may be treated by lithotrity, the remaining one-third being amenable only to lithotomy, or unfitted to be the subjects of any operative procedure whatever. That nearly half of all calculous patients being children, and these being given over to lithotomy, the proportions just named are almost exactly reversed, as applied to the total of patients of all ages; one-third only being fitted for lithotrity, and that two-thirds (including the few unfit for any operation) should be treated by the operation of lithotomy. The question, then, naturally follows, What are the extreme limits of those conditions which may be regarded as consistent with an attempt to crush stone in the bladder? First, as regards the number of calculi, Mr. Coulson thinks that a plurality of stones does not necessarily contra-indicate the attempt, provided they are at the same time small and tolerably friable. In regard of size, he says,

> "When the stone does not exceed an inch and a half in diameter, the operation of lithotrity, as far as the size of the calculus is concerned, is perfectly applicable. Even when the stone measures from an inch and a half to two inches and a half, it may be crushed, provided it be not very hard, and the urinary organs be free from disease." (p. 115.)

The density of a calculus may be sometimes so great that no legitimate force will produce an effect upon it, a statement which holds good of some examples of the oxalate of lime concretion. The condition of the urinary organs is not to be considered. Enlargement of the prostate, unless considerable or manifested chiefly in the middle lobe, is not to be regarded, as offering any insuperable obstacle, provided the stone be of moderate size, and the organ not very sensitive or obviously diseased. Catarrh of the bladder, when uncomplicated with serious lesion of the organ, is by no means to be regarded as an objection. Numerous facts show that this condition is often relieved or removed by the operation. If purulent matter is an habitual accompaniment of the mucous discharge, lithotrity must be positively rejected. Considerable hypertrophy of the bladder, conjoined with diminution of its cavity and excessive irritability, offer a condition in which it is manifestly impossible to use a lithotrite with effect, and unsafe to make the attempt. Mere columnar hypertrophy appears not of itself to involve a sufficient objection, but sacculation of the bladder is an extremely unfavourable circumstance. If the calculus be encysted, it is almost superfluous to observe that lithotrity is inapplicable, but lithotomy is equally so. The presence of fungoid tumours springing from the mucous membrane may be held as sufficient cause to decline any operation, although M. Civiale has related cases terminating favourably when this complication has existed. Paralysis of the bladder, depending on spinal or cerebral lesion, is a positive contra-indication. That simply local condition, so frequently observed, in which the bladder appears unable to entirely expel its contents, and to which the same term is often applied, is widely different. This, although rendering it doubly necessary to exercise a watchful care against accumulation of urine and retention of detritus after the use of the lithotrite, does not absolutely contra-indicate it. The existence of extreme morbid sensibility of the bladder, determined only by a trial, will probably compel the operator to desist from the attempt. The undeveloped condition of the urinary organs before the age of puberty, together with the known success which attends the operation of lithotomy in children, are held by Mr. Coulson to indicate that method as the safer proceeding for them. Aggravated cases of cachexia, associated with long-existing calculus, are deemed also unfit subjects for crushing. And lastly, the complication of renal disease, when its signs are unequivocal, render, in Mr. Coulson's opinion, lithotrity a more dangerous proceeding than lithotomy. On this point he differs from M. Civiale, who takes, as already seen, a converse view of the comparative danger arising from the two operations.

On one point Mr. Skey dissents from the conclusions arrived at by Mr. Coulson. Thus, in respect of age, he does not absolutely reject lithotrity during the period of boyhood, but advocates an attempt to dilate the urethra; if this succeed sufficiently to admit a lithotrite of the size of No. 7 catheter, and the stone be not disproportioned in size or density, the operation should be performed. He instances a successful operation by Mr. Wormald, on a lad of nine years of age. The French practice differs widely from our own in this particular. Lithotomy has long been practised on children of all ages by Civiale, Leroy, Ségalas, and others, although the number of instances bears a small proportion to their adult cases, and therefore this method must at present be regarded rather as the exception than the rule of French practice generally. In America also, Dr. Gross informs us lithotrity has been successfully employed on very young children. In this country the objection to this application of lithotrity in part consists in the presumed hazard of employing a lithotrite sufficiently small for the purpose. Thus, Mr. Skey, as just seen, appears to have considered the calibre of No. 7 as about the limit of safety in descending the scale of size. The French mechanists, however, are making, of a diameter not at all exceeding our No. 5, instruments possessing great power, and which we have ourselves seen subjected to extremely severe tests, without sustaining injury. But the main objection against the employment of lithotrity in the cases of children does not consist in the instrumental difficulty of its performance, but in the fact that it does not possess that superiority to lithotomy on the score of freedom from danger which it certainly maintains in adult cases. The mortality following lithotomy in children is very small as compared with that in the adult subject. In round numbers it may be regarded as one in fifteen against one in about five or six. Now one death in fifteen may probably be regarded as a fair average result from the operation of lithotrity, as applied to suitable patients of all ages. The inducement to change our present system of cutting for one of crushing is therefore comparatively small; and in the hands of the profession generally it is very doubtful whether our present result is likely to be improved by a change. In the hands of those who have achieved a large experience of crushing stone, a converse condition might perhaps be observed. Nevertheless, it cannot be denied that when the operation of lithotomy results, as we have all sometimes witnessed, in the removal of a concretion no bigger than a pea, or even smaller, it cannot but be forcibly suggested to the mind, whether a preliminary attempt to crush might not have been worth the trial. Were the initial proceeding of sounding conducted by means of a small lithotrite—and a better sound in the opinion of most operators it is impossible to employ—it is not improbable that so small a stone might sometimes not merely be detected, but even crushed at a single sitting; a result, we suppose, that no one will consider otherwise than the most favourable which could take place for the patient.

There is perhaps no subject respecting which it is more difficult to arrive at a conclusion than that of the mortality following operations for stone. Cases have been collected from the hospital records of various countries by various observers, the private practice of the most distinguished surgeons has been made to furnish its quota of information, and yet we have the most conflicting statements respecting this subject. The failure, for such it must be to some extent considered, of our statistical inquiries appears to have been due to the practice of seeking an extremely large number of cases, rather than a moderate number of well-authenticated reports. We are quite aware that it is absolutely necessary, in order to arrive at truth in statistics, to operate upon large numbers; but it is also equally necessary not to forget, in the process of obtaining materials, that any number of facts, however small, is superior to the greatest collection of observations of which an uncertain proportion cannot be authenticated. Unless we adopt a settled plan on which to regulate the records of all cases of lithotomy, comprehending at least a general understanding as to what shall be recognised as a death due to the operation, how is it possible to make a calculation that shall be even approximatively correct? A death from peritonitis within seven days of the operation, will, in one quarter of the globe, not be attributed to it, but be viewed as an independent accident. In another, an extension of inflammation to the kidney and suppression of urine will be similarly regarded. In a third, both of these will be looked upon as unfortunate but obvious results of the operative procedure, and will be so recorded. This is a mere hint, which might be greatly extended, as to the inaccuracy of the data upon which such conclusions are based. Mr. Coulson's table of 6369 operations, all but 83 from European sources, shows a result of 1 death in 6⅝ cases. In M. Civiale's table of 2368 operations, exclusively French, the result is 1 death in 5¾ cases. Dr. Gross has constructed a table, comprising the cases of selected operators, as Cheselden, Ponteau, Kern, Martineau, Liston Crichton, Brett of Calcutta, and others — in all, 1596 cases, in which the deaths are about 1 in 12; and another, comprising the operations of American surgeons alone—in all, 895 cases, in which the deaths are 1 in 20⅓—a result which differs astonishingly from our own experience. Mr. Skey believes that 1 in 5 is a fair deduction from the facts available, as regards our own country and France. Sir B. Brodie refers to the fact, that the result of the hospital operations in London during the year 1854 —in all, 59 cases—were 1 death in 6. Undoubtedly, the effects of climate and of the patient's habit of life exercise a large influence upon the success of lithotomy. Thus, some country hospitals will produce a far superior result to that which the practice of our crowded metropolitan hospitals exhibits. In India, again, the operation is scarcely looked upon as a very dangerous one. Mr. Brett's experience (Calcutta) has long been well known—viz., 101 cases with 7 deaths. Recently, Mr. Raddock has published an account of 77 cases with 5, or at most 6, deaths.*

For statistics of lithotrity we depend chiefly on the published accounts of M. Civiale and Sir B. Brodie. M. Civiale reports 848 patients and 14 deaths, or 1 in 42⅕. From the facts supplied in his own work, however, the mortality should, we think, in fairness be doubled. But relying upon the evidence of witnesses of the highest credibility, who have published to the world analyses of Civiale's cases with a result greatly differing from that which he deduces, we are compelled to regard that reduced ratio as still too flattering. Sir B. Brodie obviously displays an anxiety to lean towards the opposite direction, rather than present too favourable a view of an operation which he was one of the first to practise, and which he has done much to promote in our own country. He met with 9 deaths in about 115 cases, or about 1 in 12½. But an examination of the fatal cases renders it very questionable whether all of these can be charged to the account of the operation.

The subjects already discussed, with the exception of that of statistics just considered, are not those on which much additional information will be obtained by a reference to Dr. Gross' work. Some important topics —that, for example, of the treatment of impacted stone in the urethra, following lithotrity—are very lightly touched.

Dr. Gross, however, does not, we think, profess any very strong bias in favour of lithotrity, and appears to have had a much larger experience of the cutting operation. This appears to be, in accordance with the feeling of American surgeons generally, a result of their successful practice, which is perfectly natural. Then he tells us that lithotrity is not frequently performed in the New World, "where, if it be not on the wane, it is certainly not on the increase." (p. 533.) In relation to his remarks on the performance and management of lithotomy, there are many practical observations—those, for example, in connexion with the subject of arresting hæmorrhage—which are well worthy the reader's attention. On the other hand, we meet occasionally with a recommendation in which we think few surgeons would be induced to concur. Take the following as an example. Speaking of the difficulties of lithotomy, he writes:

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* **Annals of Medical Science, No. 4. April, 1855: Cases of Lithotomy, by C. E. Raddock, Esq., Sub-Assistant-Surgeon, Malwa Bheel Corps.**

"When the prostate has been much contused or lacerated, whether unavoidably or through inadvertence, the best practice is to cut away the injured part with a pair of long, curved, blunt-pointed scissors, such as surgeons are in the habit of using for excising the uvula. The wound is thus converted into a simple one, which does not slough, but heals by the granulating process." (p. 584.)

There is one subject, however, which has not been much investigated in this country since the labours of Dr. Prout were published, respecting which Dr. Gross has collected with great labour a vast mass of material. In an appendix he supplies some very extended and interesting details relating to the "Prevalence of stone in the bladder and calculous disorders in the United States, in Canada and Nova Scotia, and in foreign countries." We observe, that when speaking of England, he has been remarkably misled as to the prevalence of calculus in various districts. Thus, on the authority of Dr. Prout,* "he states that the greatest mortality appears to be at Manchester . . . . . while in Norfolk and Suffolk it is below the average." (p. 907.) Now it is well known that these two counties are the most prolific in calculous cases throughout Great Britain; but Dr. Gross has assumed mortality to be the index of prevalence, and has regarded the number of deaths in a district, and the number of cases occurring there, as facts of similar significance. Such a method, however, is extremely fallacious. For example, a large proportion of the fatal stone cases of London is imported from the country districts, and an enumeration of these can form no index to the frequency of calculous disorders in the metropolitan district. But Dr. Prout appreciated the distinction very clearly, for in the very next page of his book to that from whence the foregoing quotation was made, he alludes to the fallacy which might arise as regards the prevalence of calculous cases, saying, that "the large majority of such cases recover; this doubtless explains the apparent anomaly as regards Norfolk and other districts where calculous affections are *notoriously prevalent.*"

We regret that our limits render it impossible to afford anything like a digest of the information thus collected, and we must refer our readers to Dr. Gross' work, which amply deserves the attention of those who are interested in this subject.

A chapter is devoted by Mr. Coulson to the subject of calculus in the female. He remarks that, despite of a contrary opinion sometimes expressed, his statistical inquiries lead him to believe that "a considerable number of those operated on (cases of dilatation and cutting) labour ever afterwards under the distressing accident of incontinence of urine." (p. 259). Lithotrity is advocated as a safe proceeding for most cases. Our only surprise is that it should not long ago have become the rule of practice, instead of the exception. The occurrence of calculus in female children is not mentioned. Dr. Gross refers to an instance or two, but they are undoubtedly rare. Nevertheless, five or six cases have presented at the London hospitals during the last eighteen months. The details of one of these, in which a single application of lithotrity was completely successful in our own hands, was published some time since.† In France this method has frequently been adopted, and there can be no reason why this simple procedure should not in future supersede, at all events in many cases, the practice of cutting or dilating generally adopted.

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* The Nature and Treatment of Renal Diseases, fifth edition, p. 584.

† Lancet, Oct. 24th, 1854.

We have now briefly to notice Mr. Allarton's "short sketch of a modified operation for stone," entitled 'Lithotomy Simplified.' The proceeding, which he has on three occasions practised, and now recommends to his professional brethren, may be described as follows. The patient occupying the ordinary position, the operator passes a staff with a median groove, and confides it to an assistant to maintain steadily against the pubes. He then introduces into the rectum his left index finger, so as to recognise with its point the staff in the urethra; the walls of the bowel and substance of the prostate intervening. Next he transfixes with a long and straight bistoury, the cutting edge of which is directed upwards, the integuments of the perineum, in the median line, commencing about half an inch anterior to the anus, and carries it steadily onwards until it enters the groove in the staff and pierces the urethra about the membranous portion, the finger in the bowel rendering this manœuvre safe and easy. Having pushed the point of the knife onwards towards the bladder for the extent of a line or two, but not so as to incise the prostate, he cuts upwards, dividing the membranous portion, and making an external incision from an inch to an inch and a-half long, according to the presumed size of the stone. He then introduces a long ball-pointed probe through the wound into the bladder, to serve as a guide for the left index-finger, which, having been previously well greased, immediately follows, owing to the shallowness of the perineum here, directly into the bladder, and the staff is at the same time withdrawn. The stone may now be felt, and the wound dilated by means of the finger, but if this be insufficient for the purpose, the use of Dr. Arnott's hydraulic dilator is recommended. The action of the bladder, says Mr. Allarton, forces the calculus towards or into the wound, and it is easily extracted by the finger, scoop, or forceps. Mr. Allarton has operated by this method on a patient of nine years, on another of two and a-half years, and on another of twenty years of age, in each case with complete success, and without having recourse to the hydraulic dilator. The after symptoms appear to have been remarkably inconsiderable, as two of the patients "were up and out the day after the operation, and one was walking out on the third day (a cold, snowy, frosty day)." Two other cases have been treated in the same manner, one by Mr. J. Hinton, of South Wales, reported in the 'Association Journal,' April 6th, 1855, attended with equal success; another by Dr. F. J. Brown, of Chatham, reported in the 'Lancet,' Oct. 6th, 1855, in which death followed eighteen days after the operation. In this case there were several calculi; two were removed by the forceps, and three were found in the bladder after death, when a gangrenous aperture was also discovered, opening from the bladder into the peritoneal cavity.

The principle of operating in the median line, as Mr. Allarton himself observes, is not a novelty. The Marian operation, first practised in the sixteenth century, consisted in cutting into the bulbous and membranous portions of the urethra, in subsequently introducing numerous metallic "dilators," so called, and in tearing asunder by their means the urethra up to the neck of the bladder. No wonder that its results were unsatisfactory. The method before us differs from the Marian, inasmuch as it opens the urethra at a point further back, so that the prostate bears (as in the ordinary lateral operation) the stress of the dilating action; and also because this latter, exerted, as it has hitherto been, by the finger alone, has been in reality what it professes to be, dilatation, and not laceration.

Since that time, several methods of operating in the median line of the perineum have been designed and executed, chiefly with the view of making an opening into the bladder by a short cut, and with less risk of hæmorrhage than the lateral operation involves. Several surgeons, among whom the names of Vacca and Sanson are pre-eminent, made incisions similar to those already described, but extending in addition, more or less, through the anterior wall of the rectum, constituting the recto-vesical operation. Manzoni of Verona, in the beginning of the present century, referred to by Mr. Allarton, performed the median incision on essentially the same method. Dr. de Borsa of Verona has subsequently continued the practice, and both obtained remarkably successful results. Professor Rizzioli, also an Italian surgeon, has still more recently called attention to the operation.* Mr. Allarton claims to differ from them in the following particulars: first, in introducing his finger into the rectum, for the purpose of steadying the staff, and acting as a guide to the knife; secondly, in making the requisite opening by a single incision; and lastly, in employing a ball-pointed probe by which to conduct the index-finger into the bladder subsequently. The principle of the operation is therefore essentially the same as that of Manzoni and De Borsa, but in its details it appears to be rendered more certain and safe in the performance.

The great objection to be taken against this proceeding consists in the limited opening which it affords, and in its consequent unsuitability for the extraction of large stones. It is confessedly necessary to employ a hydraulic dilator, or to extend the wound by lateral incisions, where the stone is of a full average size. In short, the operation seems most applicable to those cases in which lithotrity is now generally resorted to, at all events in private practice. On the other hand, it must be admitted that the incisions do not involve dangers which are commonly met with and often prove fatal in the lateral operation. They are less considerable in themselves, more easy to accomplish; they divide less important parts; the bladder is reached by a shorter route, and there is little or no risk of hæmorrhage. These are facts worthy the consideration of the profession. For we cannot admit that there is good reason to rest completely satisfied with the existing practice of lithotomy, when we regard our experience of its mortality, already referred to. Supposing it to be affirmed that some operators (for it cannot be said of all) have been able by a large experience to achieve unusual success, and that their results exhibit a comparatively small per-centage of mortality,—is that fact to be regarded as favourable to the operation? Assuredly not. On the contrary, it exhibits a strong objection; it indicates it to be a proceeding in which success must be preceded by a long and peradventure fatal practice. We are not, therefore, disposed to treat lightly a suggestion which, it must be admitted, has much to recommend it, either on the ground of its not being absolutely new, or on that of a self-complacent satisfaction with things as they are. Depend on it, there are improvements yet unborn in the art of extracting stone. The Italian practice has been attended with extraordinary success, and it is not to be ignored or explained away. If patients in a hospital are still to be cut for the stone, whose circumstances and symptoms are such as to indicate the employment of lithotrity, were they met with in the walk of private practice, there certainly appears to be no good reason for not testing the median operation, in order to ascertain its value for such cases. May it not also prove to be a more safe and simple method than the lateral operation for the purpose of extracting some foreign bodies, not being calculi, which have been inadvertently introduced into the bladder, such as fragments of bougies, sounds, &c.?

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* Bulletino delle Sc. Med., vol. xvii. p. 271.

We cannot forbear remarking at the close of this necessarily short notice of a most interesting and important subject, that the tendency of progress in the surgical art will probably render us increasingly familiar with lithotrity, and diminish our experience of lithotomy. Lithotrity is the offspring of increasing mechanical skill and of improving scientific knowledge, the limits of which it is impossible to foresee. The present methods of lithotomy depend on anatomical and physiological laws which cannot change; we have no reason, therefore, to anticipate anything beyond a modification of that practice which experience has already developed. But the present system of crushing, perfect as we may be inclined at present to regard it, is probably but a step towards a far more safe and successful mode of removing the stone, which the future will disclose. And who shall say that chemistry may not, in this particular instance, add another to her numerous triumphs over the mechanical powers, and ultimately supply the most simple and efficient means of ridding man of one of the most distressing and fatal ills to which his flesh is heir?

Henry Thompson.

REVIEW VIII.

Letters to a Young Physician just entering upon Practice. By JAMES JACKSON, M.D., LL.D., Professor Emeritus of the Theory and Practice of Physic in the University at Cambridge; late Physician in the Massachusetts General Hospital; Honorary Member of the Medico-Chirurgical Society of London; Corresponding Member of the Academy of Medicine at Paris, &c. &c.—*Boston and New York*, 1855. pp. 344.

AMONG the fascinations which surround the study and exercise of medicine, not the least is the constant stimulus to comparison between present and past experience, which can scarcely fail to result in the acquisition of some new fact, if not of some leading principle. The greater the grasp of the mind, the stronger, of a necessity, will the hold be which this constant and ever-intertwining woof of relations exerts upon the individual. In no class of men is there so habitual and devout a reference to and intercourse with nature in all its phases as in the medical profession; in none, as a body, is there so perfect an understanding between the older and younger members as regards the principles at issue and the objects to be attained, provided always that the high aims of science are not clouded and degraded by the exclusive pursuit of gain. It appears to us that modern times are eminently distinguished by the easier intercourse between older and younger minds, and the more ready and mutual interchange of opinions. The more earnest our pursuit of a high goal, the purer our aims, the more shall we find that, however parties may be necessary for the development of political perfection, in science we may accelerate the wheels of progress by a unity of pursuit, but can only retard it by sectarianism. The less selfishness and the more humility the individual possesses, the more readily will he accept from his fellow-labourers assistance in the pursuit of truth, which alone should be the common aim. Such and similar thoughts have often passed through our minds, but they have been especially called up by the perusal of a collection of letters addressed to a young physician, by the octogenarian American physician, Dr. Jackson, of Boston, and in which we scarcely know whether most to admire the vigour of memory and comparison, the absence of dogmatism, the respect for the advances of science made during the author's advance in years, or the earnest and unfaltering love that he, the octogenarian, bears to his profession, coupled with that large philanthropy which always characterizes the truly successful medical man. The letters must not be read for what they are not. They are not a Cosmos of medicine; they do not give an æthereal extract of the whole art and science; they publish no startling novelties or wonderful discoveries; they are simply the simple and unadorned account of the experience of a long and arduous life on some points in medicine, upon which the old man (we say it reverentially) wishes to impart instruction to younger men. It is the tone which pervades the whole, the moral as well as the intellectual atmosphere which meets the reader that commends it to the readers of all ages, and, we think, justifies us in calling it emphatically, a good book.

Such being our opinion, we shall allow the author to speak for himself; as our present object will be rather to introduce the man and the views which above fifty years' experience have matured in him, to our readers, than to establish any new point or to terminate any controversial discussion. And, first, with regard to the ethics of the profession. What better lesson than the rules which guided our author in his intercourse with his friend of many years, to whom he dedicates the book, Dr. John Warren?

> "You and I began our active lives in this city nearly at the same time. It was when Boston had about one-sixth of its present population, and, I suppose, much less than a sixth of its present wealth. We were so circumstanced as to be peculiarly rivals. Our business led us across each other's paths every day for a long series of years. What one gained, the other seemed to lose. It would have been very easy for us to have got up a pretty quarrel at any moment; and having once begun, we might each have got partisans, and all the usual entanglements to such cases appertaining might have followed. Happily, we pursued a different course. We met together with the feelings we had had as fellow-students. We took much delight in consultation and discussion on professional subjects, and were ever ready to help each other. We have, indeed, maintained a strong personal interest in each other's welfare, and promoted each other's happiness. We do not resemble each other in temperament, and cannot see all things alike. From this cause, and not always looking at objects from the same point of view, we often differed in opinion. But we have always agreed to differ. We have not often disputed, and never have quarrelled, on account of this difference of opinion, nor on any other account. In our intercourse with the sick, each has given the other credit for what was good in him, instead of studying and publishing the

other's faults. In every work for the promotion of medical science, or for elevating the profession, we have co-operated heartily, neither of us trying to push the other aside. And thus it is, that being now, as regards age, in the front rank in our profession, we have continued to this day on terms of intimacy and friendship. This is something to rejoice in, and something for which we may properly thank God; and I know you will join me in giving thanks reverently.

"As we are near the end of our journey, I hope I may be excused for stating this experiment and its results. I would show to young men how grateful these results are. I can say to them that our interests have been promoted by our friendly treatment of each other; that each of us has gained by it much more than either of us could have done by the sharpest quarrels. If they believe me, any two of them, placed side by side, as we were, may be induced to try the plan of a peaceful competition." (pp. 2, 3.)

So much for the past. The man who could thus readily, in the turmoil of life, agree in a noble and friendly rivalry under circumstances peculiarly trying, would necessarily appreciate the labours of the younger generation springing up around him; who, to use his own terms, no longer required the log-huts which were good for the pioneers of a new and uncultivated country, but wanted and built up accordingly, more lasting and stately edifices:

"It has been my delight," he says, "for many years past, as I believe it has been yours, to point out to others what a respectable body of junior practitioners has been rising up around us. Compare the power of distinguishing diseases, and the discretion in treating them, shown in young men of the present day, with the like characteristics of our medical men forty years ago, and you will find the advance to be very honourable. Medical science has been increasing in Europe and in this country. Our younger men have had great masters. Not to mention our own countrymen, they have had Laennec and Louis, and many others among the French; in Great Britain and Ireland, too, many may be named; and a host of surgeons, of whom you could best give the list. It may be said that it would have been shameful if they had not acquired much. But no such reproach falls on them; on the contrary, it is due to them to say, that they have fully availed themselves of their opportunities. For my own part, in looking at one, and now another, who have succeeded me in office, I think it enough to boast of that I aided in teaching them how to learn. I should be sorry to believe that they had not gone ahead of their predecessor. I only beg that they will allow him to be a sort of honorary member in the corps of *young physic*." (p. 4.)

The general principles which should guide the practitioner are laid down more fully in the introductory chapter. It is here more especially that the author explains the aims and duties of the medical man. He adverts to the numerous difficulties that beset his path, while he meets the scepticism or overweening confidence that equally prove stumbling-blocks of the hasty man, by the calm judgment of the man who evidently has himself experienced the pain of doubt, and still sympathizes with those who are in danger of being entangled in the mazes of uncertainty. To those who have not yet arrived at the firm convictions necessary to insure a truly satisfactory practice—we speak only of intellectual satisfaction—we would recommend the following observations:

"It is my own practice to avoid drugs as much as possible; and I more frequently find it difficult to persuade people to abstain from using them, than to induce them to take them. But I hope that you will not believe me to be distrustful of the power of drugs to do real service to the sick, under proper circumstances. I am far otherwise. And, in reference to this point, I wish to tell you that your success in the use of medicines may depend somewhat on the temper in which you give them. You must be hopeful and feel an interest in them. Do not, like a cold step-father, leave them to make their own way in the world; but watch them in their course. You cannot make a fire burn well if you put the wood on the andirons with a feeling of indifference. You must study to know the power of the drug you prescribe, the proper dose, and the tests of a sufficient dose, the mode of preparation of the medicine, and then of the patient for the medicine, and all the management requisite for a good result. Do not be in a hurry to give credit to your prescription, as soon as the patient shows any sign of amendment, nor be discouraged if relief do not follow as soon as you had anticipated. In this last case, see if there has not been some error in the management of the affair, or if some counteracting cause has not interfered. Do not despair because the medicine has failed on your first trial of it. Try it again, before you condemn what has been recommended as beneficial by one well qualified to form a judgment on it. I have wandered from my point.

"It is a very narrow and unjust view of the practice of medicine, to suppose it to consist altogether in the use of powerful drugs, or of drugs of any kind. Far from it. It is true that the common question addressed to the physician by the patient is, *What shall I take?* That question implies that there is a drug adapted to every disease. But the enlightened physician first considers whether the patient shall take anything. He considers what other modes of relief there are besides pills and draughts. He looks to diet and regimen." (pp. 13—15.)

Nor can we refrain from extracting the remarks which our author makes on the oft-mooted question of the conduct of the physician when he finds that drugs cease to be of avail. Has the medical man or has he not duties to perform, apart from writing the prescription, which call for his attendance upon the patient when the store of the pharmacopœia is exhausted?

"I have sometimes had patients say I was not doing anything for them, because I had not ordered any medicine to be taken. It may be that the patient in such a case thinks that no medicine will remove the disease, and is right in his opinion; yet something is to be done, but not by medicine. By diet and regimen much may be done to mitigate suffering and prolong life. In all cases, in the worst, there is one course more prudent than another. If the ship is running on to the shore, or is even breaking up on the rocks, there may be one course better than another in the management of affairs. In the worst peril, when you must leave the bark to which you had trusted yourself, in whose guidance would you place most confidence? Would you leave yourself to the mercy of the waves? Would you trust an ordinary sailor because he bawled the loudest? Or would you follow the advice of the experienced ship-master?" (p. 17.)

The second letter is devoted to the consideration of the physician's conduct in the sick-room, a point which probably more frequently determines the success or failure of the medical man than the amount of actual knowledge which he possesses. Above all things, let the patient and the patient's condition be his sole care: a forgetfulness of self is nowhere more called for than in the sick room. Dr. Jackson rather discourages note-taking at the bed-side:

"The physician puts his knowledge on paper without fixing in his mind; we would desire that the physician should give himself to the examination of the case until it should become daguerreotyped upon his mind. However," he afterwards adds, "men differ in their capacities, and some are able to get the benefit of notes without the evils which I have suggested."

The third letter commences the actual consideration of various forms of disease and morbid symptoms which have especially arrested the author's attention; to these all the succeeding letters, amounting to fifteen, are devoted. The subjects are as follows: 1. On the Nervous System and Headache. 2. On Epilepsy and the Convulsive Fits of Young Children. 3. On Apoplexy and Palsy. 4. On Chorea, Neuralgia, and Pain. 5. On Somnambulism, Magnetism, and Insanity. 6. On Dentition and the Period of Weaning. 7. On Cholera Infantum, the Second Dentition, and Ulcuscula Oris. 8. On Abscess in the Tonsils, Elongated Uvula, Bronchitis and Pneumonitis, Rheumatism and Gout. 9. On Phthisis and Hæmoptysis. 10. On Dyspepsy. 11. On some Diseases of the Intestines, particularly of the Cæcum and Colon. 12. On Constipation of the Bowels. 13. On Bilious Diseases, Biliary and Urinary Calculi, and Irritable Bladder. 14. On Boils. 15. On the Treatment of Typhoid Fever. The desultory character of the work and our limits preclude an attempt at an abridged reproduction of Dr. Jackson's views on the numerous topics treated of. We shall select from the nosegay which he offers to us a few of the flowers that have especially riveted our attention. If they preserve their odour in the handling, we doubt not that our readers will turn to the collection itself to enjoy the full perfume.

Throughout the book we find a special stress laid upon the importance, in the treatment of disease, of diet and regimen. This is also done in his observations made upon the causes and treatment of headache, which occupy the first letter. In regard to one form of headache, intermittent hemicrania, the author observes:

> "This is one of the very few diseases in which I can venture to say that it may certainly be removed by medicine. The treatment is the same as for intermittent fever. I have employed the cinchona in the earlier part of my life, and subsequently the sulphate of quinia; likewise, both formerly and latterly, I have employed the solution of the arsenite of potash. This last I found much more convenient, at least, than any preparation of cinchona, before the introduction of quinia; and I may say that, were it not for a reluctance to use metallic articles, and especially one which has so bad a name, I should employ it in most instances at the present day. Whichever article is employed, it should be given in as large a dose as the patient can conveniently bear. In giving the quinia, I administer it only during the intermission. For an adult, I first order twelve to sixteen grains during this period, and in the next intermission increase the quantity to a scruple, and go on to increase it until the buzzing of the ears, or sense of tension in the head, shows that the dose is sufficient. Subsequently, I keep at as large a dose as can be borne without much inconvenience, until the patient has passed the periods of two paroxysms without any return of the pain." (pp. 55, 56.)

It is the apportionment of the dose to the intensity of the disease and the constitution of the individual, by which we recognise the apparently intuitive tact of the successful practitioner, for which, however, we possess as yet no scientific guide. The greater the velocity which carries a steam carriage along the railway, the more powerful must be the break to arrest its course suddenly; with similar propriety we should adapt the force of our remedy to the intensity of disease, wherever we have a means of determining the relation of the two elements of treatment. But so far from vigorous treatment, as it is called, being always required, there are many cases in which it would be pernicious, and when our motto must be, *festina lente.* Thus, in the treatment of the convulsive fits of children, which our author justly regards as bearing a close relation to epilepsy, after speaking of the exciting and predisposing causes, he observes:

> "Children subject to these fits should be guarded against all the exciting causes. But for this purpose they should not be kept in a nursery, but should be invigorated by exercise in the open air, and be inured, as far as possible, to the irritations necessarily attendant on life, though protected from all extraordinary causes of excitement. Simplicity and regularity in diet should be rigorously enforced. The hours for sleep should be regular, and they should go to their sleep in a calm state, so far as it is possible to effect it." (pp. 71, 72.)

The vegetable diet, which Dr. Jackson regards as the main stay in the treatment of epilepsy, he also advocates strongly as a preventive measure, in persons who have shown symptoms of an apoplectic character:

> "Besides advising moderation in all things, I have directed the diet just mentioned (vegetable diet) to be continued indefinitely. If one says to a man in middle life that he should never eat meat any more, he may rebel. He will think that this rule calls for more self-denial than it really does. It is enough to suggest abstinence for the present, and at the end of several months, or pretty certainly in a year, most men become more indifferent on this subject than they had anticipated. So far from losing muscular power, a man under this treatment may get to endure long-continued labour, and to make as great exertions as prudence would permit. The exercise is important, and should be a part of his treatment; but he should never permit himself to make violent efforts. He should also be enjoined, as far as possible, to abstain from anger and anxiety. And, further, when I advise abstinence from animal food for an indefinite period, I do not mean that this abstinence should continue for life. I know not what length should be fixed upon; but this appears to me reasonable, that the patient should not return to the use of animal food so long as he has very good health without it. Whenever it appears that he suffers for want of such food, let him cautiously resume the use of it." (pp. 76, 77.)

We must, however, push on rapidly, only observing *en passant*, that an admirable illustration of the real nature of the connexion that exists between the magnetiser and his subject is given in the chapter On Somnambulism, showing the power asserted to be exercised, without the consciousness of the magnetisee, to be a delusion and a snare. Some excellent remarks on the causes and management of infantile diarrhœa arrest the reader in the ninth letter.

In the letter On Phthisis and Hæmoptysis, we find the author advocating, with all the fervour of an enthusiast, the necessity of prevention and anticipation, the imperative duty of, at all stages of the disease, regarding regimen and the supply of pure air to the patient as the first consideration:

> "We must endeavour to prevent the cachexy, if that has not appeared, or to overcome it when it has. To effect this purpose we must not rely on medicinal drugs. We must pursue a course calculated to increase the general vigour of the system, trusting to the natural efforts to overcome the disease, if that be possible; and we may do this with the more confidence, as such a course may, at least, prolong life, if we cannot save it. To this end we should direct a nutritious diet; but we must not leave the patient to judge what articles are comprehended in such a diet. For nutrition, we must direct animal food, milk, and the farinaceous articles. These are sufficient for that purpose; but if they should be used alone, the functions of the bowels would not be well performed. In that case, costiveness would lead to dyspepsy, to dryness of the mouth, to heat of the skin, and,

perhaps, to trouble about the head, with uneasy sleep. So far, therefore, as the bowels require it, there should be added fruit and other articles of a laxative character. Next to the diet, and of all things most important, is exercise in the open air. This should be carried as far as the vigour of the patient will permit. It should not be done rashly, but boldly. If possible, the patient should be made to have faith in it; for without this, he is not likely to pursue it as far as he can, and then he will not derive from it all the benefit which it can afford." (pp. 174, 175.)

The following case is a parallel to the remarkable one given by Dr. Graves, of a gentleman in an advanced stage of phthisis, who, to all intents and purposes, was cured by duck-shooting and brandy and water, after having been sent home by the doctor to die:

"An instance occurred to me, nearly fifty years ago, which I have often related since, as well calculated to produce a proper faith. It confirmed my previous convictions on the subject. A man presented himself, in the month of May, who lived in a retired part of Maine, below Penobscot river. He had come from his home, with great inconvenience, to seek for medical aid. I found that he had the usual symptoms of phthisis; he had been confined to his house in January, at which time he sweat profusely in the night, was much reduced in strength, and wretchedly sick. He saw, however, that he and his family must starve, if he could not engage in his usual winter employment of cutting wood. After much reflection he went forth, on the first of February, with his axe on his shoulder. He laboured for half an hour, when he was so exhausted that he was forced to lie down upon the snow. Thus ended his first day's trial. He persevered, however, and by degrees gathered strength, so that at the end of the season he could do a moderate day's work. This story he told me with many details, which I need not repeat. They were such as to show conclusively that his was a case of phthisis." (pp. 175, 176.)

The course pursued in this instance is not exactly that recommended for all cases; but while Dr. Jackson advocates horse exercise for one and working in the garden for another, he urges that in all the earlier stages of the disease the patient should be made to feel that the *risk is in staying in the house*, and not in going out of it, provided the skin be properly protected, and especial care is taken to avoid the chilling effects of the atmosphere in passive exercises. Some interesting remarks are made on the subject of hæmoptysis. During his whole life, Dr. Jackson has only met with two cases in which it proved directly fatal in phthisis. His chief remedy in severe hæmoptysis is a combination of sulphate of copper and opium:

"In an urgent case, which had continued four days, and which I then saw in consultation, I gave a grain of the sulphate with an equal quantity of opium. The bleeding lessened very much soon after; a second dose was given at the end of twelve hours, from which time the bleeding ceased. No inconvenience was experienced from the copper. A single case like this is not offered as a proof; but this came to me in corroboration of the benefit in many other cases, where there was hæmorrhage from other parts of the body, besides the lungs." (pp. 190, 191.)

He disapproves of the habitual employment in this disorder of blisters and venesection, but justly lays great stress on the necessity of a rigid adherence to the dietetic and regiminal rules, which may be comprised under the head of bodily and mental rest, vegetable diet, and fresh mild air. The same judicious attention to mental and physical influences is given and recommended to the dyspeptic, to whom the twelfth letter is devoted. From the frequency with which our apeptic and dyspeptic patients refer their malady to anxiety and other mental influences, we should sometimes almost be tempted to locate the anima rather in the vicinity of the umbilicus than in the cerebrum. Long before we thought of handling Hippocrates or Sydenham, this was forcibly brought home to us in the person of a reverend and revered pedagogue, who invariably put on a black velvet skull-cap, and refused all the dishes at dinner, when any disturbance had taken place in the school. The effect was so uniform that it could be calculated upon. But to return to our author. He observes on this point:

> "In the treatment of a dyspeptic, then, more than of most other invalids, it is the first object to ascertain the remote causes of the disease in his case. To this end you must get a brief history of his life. This requires some cross-questioning; for the patient will often hold back important facts, either because he regards them as unimportant, or because they are such as he does not wish to disclose. If you suspect the last-named difficulty, it is well to say to him that you wish to know whether he has had any secret causes of anxiety or trouble; that, if so, it is enough for him to make a general answer, that you would rather not have the charge of his secrets. You will have the best chance of aiding your patient if you can keep him under your eye and under your care for awhile, so as to ascertain his character and habits, and so as to educate him as to his mode of life. In going over the history of his life from day to day, you may satisfy yourself and make him realize what are the errors of his ways; that he may be convinced that a good life will lead to health; that he must not sin for a week, and seek absolution at the end of it by the aid of the apothecary. In this last course such a man loses ground constantly.
>
> "In many instances, instead of prescribing a medicine I have found it necessary to give my dyspeptic patient a moral lecture; and that, even though he wore a black coat. My lecture has, indeed, most often had reference to sensual indulgences; but sometimes it has turned upon points of a very different character. Not unfrequently I have had to descant upon the evils and the impropriety, if not the sin, of over-conscientiousness; of too great an anxiety to do right, and of distressing regrets from the fear of having erred, unintentionally, in some minute particular. In this morbid state a man may waste his present *hours* in lamenting the waste of *minutes* in time past." (pp. 215, 216.)

We cannot avoid observing, that although we agree in the main with the views of Dr. Jackson on the subject of dyspepsia, and although we make all allowances for the character of the work before us, we think the author has adverted too little to the various forms which dyspepsia assumes, and which demand most undoubtedly as various a mode of treatment, medicinal and regiminal. The letter certainly makes the impression as if there were but one, an atonic, form of dyspepsia, to be treated on a tonic system. We are satisfied that it is not Dr. Jackson's intention to convey such a view to the student or the young physician. The succeeding letter, On Some Diseases of the Intestines, commences with remarks on the functions of the intestines, among which the following are suggestive:

> "The contents of the smaller, after the removal of the chyle, are discharged into the larger, and they are not permitted to return; for a valve, placed in the colon, prevents it. Having passed this barrier, the mass falls into a blind sac, the cæcum, evidently designed to retain it for a certain time. Unquestionably some change is wrought in the mass while in this receptacle, and something, not chyle,

> must be absorbed from it, while it is transported through the long tract of the colon. What the change is, and what the material absorbed, has never been explained, so far as I know. I do not speak of what has been guessed, but of what has been ascertained. I have never heard of any shrewd guesses even. Suppose it proved that some muriatic or other acid is found in the cœcum; that will suggest that this acid has some purpose; but the question is, *What purpose?* We must attach more importance to the operations of the large intestine, when we notice that they are not designed to carry forward their contents rapidly, but the contrary. After its resting-spell in the cæcum, the fæcal mass, a dead weight, must be started up from that pouch, be carried through the ascending, transverse, and descending colon, and in its course must meet an obstacle, evidently designed, at the angle formed between these two last portions of that intestine. I may mention, also, some delay in the passage through the sigmoid flexure of the same intestine. Where there are provisions so evidently fitted to hold back the mass in its course, we can see how easily obstructions may take place to the easy and perfect accomplishment of the functions of this machinery. In addition to the mechanical obstacles to the rapid passage of the fæces, we know that the change of the semi-fluid mass to a state of comparative solidity may be carried beyond its due point, and thus a new difficulty arises in the process of defæcation.
>
> "Some explanation is thus suggested of the habitual constipation so common among persons not leading natural lives; and some explanation, also, in regard to other diseases of the bowels." (pp. 233—235.)

There are other parts in this letter to which we would direct the attention of our readers by transferring them to our pages, but we pass them over because we cannot find the space, and because we do not attribute the same importance or originality to Dr. Jackson's observation of a painful inflammatory affection of the cæcum forming a tumour, that he evidently thinks it deserves. We would only refer him to the article, 'Cæcum,' in Dr. Copland's dictionary, in proof of our statement. Dr. Jackson's regiminal and dietetic proceedings find full scope in the subject-matter of the fourteenth letter, in which he discusses Constipation of the Bowels. A disquisition on the use of various articles of diet, and especially of fruits, occupies a considerable part of it. In one part we were reminded of the old adage, about fruit being gold in the morning, silver at mid-day, and some very base metal in the evening. Dr. Jackson says that he thinks "the best time for the use of fruit is at breakfast, though it may be taken at dinner, if proper room be allowed for it." It would be very interesting to ascertain with whom this doctrine first originated; for we are perfectly satisfied that the saying is not the result of extensive experiment, otherwise the practice of lower and higher orders, throughout numerous countries that we are personally acquainted with, could not be so directly opposed to the proverbial assertion. However, much truly practical matter is contained in the chapter in reference to diet, the use of enemata and medicines, exercise, and the like. Among the remedies we cannot forbear adverting to one which we do not remember seeing recommended for such uses; it is the resin of guaiacum, of which Dr. Jackson says that it acts "so pleasantly on the bowels that it would be frequently employed if it could be brought into a small bulk." The dose in which he gives it is one drachm. In the absence of all knowledge of the employment of the drug in this dose and for this purpose, we confine ourselves to quoting the author's statement. To those who, in a complaint of this kind, and elsewhere, are disposed to the *nimia diligentia medici*, we would specially address the author's words, which we find towards the end of this letter:

> "Let me add further, that in blind cases a masterly inactivity should be adopted. This requires *more true courage* than the exhibition of the most heroic remedies." (p. 295.)

There are other passages in this and the remaining letters, which we should yet bring before our readers without fearing to tire them—for the author ceases to be the author long before the end of the book—he becomes our personal friend; one to whom we look with the grateful feeling which an honest, clear, and genial mind always inspires in recounting the results of past experience; but we have duties to perform as well as inclinations to follow; and if our extracts have carried the impression to others which they have conveyed to ourselves, our readers will not be satisfied with extracts, however long, but will place the book, not on their shelves, but on their desks.

REVIEW IX.

1. *Traité Theorique et Pratique des Maladies des Yeux.* Par C. DENONVILLIERS, Professeur d'Anatomie à la Faculté de Médecine de Paris, Chirurgien de l'Hôpital Saint-Louis, Membre de la Société de Chirurgie, Chevalier de la Légion d'Honneur, etc.; et L. GOSSELIN, Agrégé libre et Ancien Chef des Travaux Anatomiques de la Faculté de Médecine de Paris, Chirurgien de l'Hôpital Cochin, Membre de la Société de Chirurgie, Chevalier de la Légion d'Honneur.—*Paris*, 1855. pp. 955.

Theoretical and Practical Treatise on Diseases of the Eyes. By C. DENONVILLIERS, Surgeon of the Hospital St. Louis, and L. GOSSELIN, Surgeon of the Hospital Cochin, &c.

2. *Du Pannus et de son Traitement, avec trente Observations de la Cure Radicale de cette Affection par l'Inoculation Blennorrhagique.* Par EVARISTE WARLOMONT, Docteur en Médecine, en Chirurgie, et en Accouchements; ancien Médecin Militaire, et Médecin des Pauvres de la Ville de Bruxelles; Membre Correspondent de l'Académie Royale de Médecine de Belgique; Redacteur et Directeur Gérant des 'Annales d'Oculistique.'—*Bruxelles.* Octavo, pp. 98.

On Pannus and its Treatment, with Thirty Observations on the Radical Cure of this Affection by Blennorrhagic Inoculation. By E. WARLOMONT, M.D.

THE authors of the first treatise announce in the preface, that it was not their original intention to publish a distinct work on ophthalmic subjects, but that the chapters upon those subjects which had appeared in their 'Compendium of Practical Surgery' had met with such high approval, that many distinguished persons had strongly urged their republication in a separate form: hence the volume before us.

The work is divided into five books, the subjects of which are respectively, Diseases of the Eyebrows, of the Eyelids, of the Lachrymal Appendages, of the Eyeball, and of the Orbit.

Of the first book little need be said; contusions, wounds, and cysts of the brows are described, with the treatment appropriate to each, but there is no point of novelty or interest to call for remark.

In the second book, *wounds of the eyelids* are fully described, and mention is made of an interesting illustration of the powers of Nature in overcoming the ill effects of mutilations. M. Gerdy removed the whole upper eyelid external to the lachrymal punctum, and a considerable portion of the lower lid; yet the patient ultimately acquired the power of covering the eye, by a forcible contraction of the remaining parts; and even in repose the eye was less bare than might have been expected, considering the great loss of substance.

Where one of the lachrymal ducts has been severed, it has been recommended by M. Rognetta and others to pass into the punctum a piece of catgut, which is to be carried through the divided duct into the sac. This is just one of those pieces of advice which it is far less easy to follow than to receive; one of the authors of this work (*which* is not mentioned) says that he tried it, and signally failed. He had great difficulty in finding the orifice of the divided canal, but at length managed to pass the catgut into it. So far good—but in inserting sutures, the catgut slipped out, and another tedious search ensued, irritating to the surgeon and the patient; at length both were tired of it, and the attempt was given up in despair: the wound was dressed in the ordinary way, and the patient made a perfectly good recovery, without any epiphora. Indeed, more harm than good is likely to result from such irritating proceedings. The cut surfaces should be accurately adjusted, a fine suture or two inserted, if need be a little collodion applied, and the chances are greatly in favour of a satisfactory cure.

Operations for the relief of *Ptosis* require much judgment, and our authors consider that there are only two conditions under which they are admissible—where the ptosis is so complete on both sides that sight is entirely prevented, or when there is ptosis on one side, the sight of the other eye being destroyed. Such cases are happily rare, but when met with, the following is the operation recommended: A reversed V-shaped incision is to be made through the whole substance of the lid, and a piece excised, sufficiently large to lay bare the greater part of the cornea, producing in fact an artificial coloboma of the lid. If subsequently the patient should have the good fortune to get rid of his ptosis, the gap may be closed by another operation. On this point we may remark, that care should be taken to leave as smooth a cicatrix as possible; we have recently seen a case in which a piece had been excised from the centre of the upper lid, and the cut edges so clumsily approximated, that a hard, rough scar remains on the inner side, which has rendered the cornea opaque, precisely as we find in cases of granular lid.

A considerable space is devoted to *Ectropium*, and the various modes of treatment; and we may remark *en passant*, that the unhappy fatality which so often attends the spelling of English names by foreign writers has cast its shadow over this book. It may be a weakness, perhaps, though an excusable one, which leads a man to prefer seeing his name printed in its native simplicity, rather than adorned with the embroidery of foreign ingenuity. We have no doubt, for instance, that Mr. Wharton Jones is not in the habit of writing his name "Warthon," as in this book:

nor, probably, does Mr. Mackesy sign himself, when he has the option, "Mackezy," nor Dr. O'Beirne "O'Beirn," nor Mr. O'Ferrall "O'Ferral." A man has a right to be punctilious in such matters, and to cling to a letter with cephalopodous tenacity. The desire for correctness will doubtless lead the learned authors to remove these little defects in their next edition. The section devoted to *Tarsal Tumours* is carefully executed, and from personal experience we can testify to the correctness of the following remarks; indeed, operations about the lids are far more painful than the generality of those on the eye itself, and embarrassment may occur if the operator is without assistance:

> "These different operations (for tumours), like all those performed in the oculo-palpebral region, are often attended with syncope: it is proper, therefore, to be prepared, and to have remedies at hand. If there are no persons to assist, the operation had better be performed with the patient in the recumbent posture." (p. 62.)

The treatment recommended for those very troublesome *erectile tumours* which present themselves on the lids, is vaccination, if the infant has not been vaccinated—setons if he has. That which we have found of most service in such cases has been passing through the tumour numerous stout loosely-twisted silks, steeped in a saturated solution of sulphate of copper, and slowly dried. Subcutaneous ligature is sometimes useful, but is far from being always adapted to such cases; and escharotics are not admissible on account of the scar and the contraction they cause, to say nothing of the danger to the eye from their incautious use. In the treatment of the more superficial nævi, the saturated solution of iodine, as recommended by Mr. Edwards, is likely to be of service.

A very teasing and obstinate affection is *trichiasis*—it is in the power of one little hair to embitter life by constantly irritating the eye; and in no matter more than in this does a sufferer feel the importance of having "the right thing in the right place." A rebellious eyelash, that will shun the society of its fellows and turn its back upon the world, deserves no mercy, and must be cast out, root and branch. To eradicate such offenders, Dr. Carron du Villards adopts the following plan. He inserts an entomologist's needle to the depth of the bulb of each inverted eyelash, and binds all the needles firmly together by means of a well-twisted silver wire; then he seizes the whole group with pliers heated to a white heat: immediately the needles glow, and all the parts with which they are in contact are cauterized and destroyed. During the proceeding the eyeball is to be protected by wet rag: the chief difficulty of this ingenious proceeding must, we imagine, be to bind together the sheaf of needles without displacing them, as their hold upon the lid must be very slight. We think it right to mention a simple, and as we know from frequent experience an efficacious, mode of dealing with cases where a few irregular distorted hairs are the source of irritation. It was suggested by Mr. Wilde of Dublin, and is thus described by him:

> "A single lash, or one or two lashes, will sometimes turn in upon the eye, and produce the greatest annoyance. The patient gets tired of plucking them out, and applies for surgical relief. In such cases, placing the horn spatula within the lid, I make an incision with a small knife down to the root of the inverted lash; and having waited till the hæmorrhage has ceased, I apply a point of nitrate of silver by means of a small *port-caustique* down to the bottom of the wound, and *then*

remove the lash. It seldom fails, but frequently it destroys two or three of the neighbouring cilia."*

We may here remark, that though some English writers are quoted, there are many of whom Messrs. Denonvilliers and Gosselin appear to be ignorant; we are sure, for instance, that the publications of the gentleman just mentioned, might be consulted by them with advantage.

There is a disease, happily almost unknown in Great Britain, but which seems to be common in certain parts of France, and also in the Havannah; it is called "*Pustule Maligne*," is the result of infection, and seems to have the character of glanders. It is spoken of in this work on the authority of M. Bourgeois as presenting two forms: one, in which it begins in the skin; the other, in which it first attacks the conjunctiva. The conjunctiva, when it has received the infection, presents neither pustule nor induration, but a pale, soft, bluish, semi-transparent swelling or chemosis, without pain. At the expiration of three or four days the characteristic vesicles and eschars show themselves on the skin. It seems to be a disease little amenable to treatment, and very terrible in its consequences. The direct mode of infection is often obscure, but in some instances it has been distinctly traced to the bites of flies which have flown direct from diseased animals, and so communicated the poison. In like manner the flies are a fertile source of contagion in Egypt, where they carry the matter of purulent ophthalmia from eye to eye. Speaking of flies, we are reminded of a curious fact mentioned by Dr. Carron du Villards, in a recent number of the 'Annales d'Oculistique.' In May, 1841, he was hunting in the magnificent forests of Frèsne, and was struck with the vast numbers of the cantharides fly which swarmed in the trees and in the air. The horses sneezed, the eyes of the horsemen streamed with tears, and their throats burned from the pungent aroma which impregnated the air, resembling that of a cantharidine manufactory. They were obliged to hasten from the spot, and several of the party were at once attacked with violent conjunctivitis. The following day, twenty persons of all ages, attacked with inflammation of the eyes from the same cause, consulted Dr. du Villards, and in one the inflammation had already assumed a purulent type. She had been struck in the eye by a flying insect, and on examination there were discovered beneath the lid two antennæ and one of the feet. The treatment found most efficacious in neutralizing the poison and relieving the suffering, was constant ablution of the eyes with a solution of ammonia.

In other unexpected manners may the eyes suffer. The strawberry growers in the environs of Paris encourage the presence of toads among the plants, as they are useful in destroying insects. One of these gardeners seized a large toad by the hind leg, on which the reptile immediately ejected a fluid which struck him in the eyes. The sensation resembled that of boiling oil; the lids swelled immediately, and he was obliged to be led home. The following day violent purulent ophthalmia set in, which was only subdued by most energetic treatment.

In warm climates it is usual to substitute honey for grease in the composition of ointments; at the Havannah this is dangerous. Dr. du Villards was greatly surprised at finding certain simple eye-salves, so prepared, gave rise to agonizing pain, until a native explained that the bees obtained their honey from well-known irritating plants, and that it contained their objectionable qualities.

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* Contributions to Ophthalmic Surgery. By W. R. Wilde. Part II. page 37.

We are glad to find that in this work the tendency to complication—we may almost say mystification—of the inflammatory affections of the eye, which has been carried to an absurd extent by some of the continental writers, is discountenanced. The following observations are so much in accordance with our own views and those of some of the best English ophthalmalogists, that we extract them. After having mentioned an elaborate classification of inflammations into *ophthalmies catarrhale, blennorrhagique, contagieuse, erysipélateuse, varioleuse, morbilleuse, scarlatineuse, dartreuse, scorbutique, veineuse* (*abdominale et arthritique*), *rhumatismale, scrofuleuse, syphilitique,* subdivided into *ophthalmies catarrho-rhumatismale, rhumatismo-catarrhale, catarrho-scrofuleuse, rhumatismo-scrofuleuse, scrofulo-catarrhale,* &c., the writers say:

> "The classification which has been briefly given rests on a just principle, but of which there appears to us to have been much abuse. That etiological circumstances exercise on the seat, the form, and the progress of the ophthalmiæ a certain influence, of which account is to be taken in the diagnosis and in the treatment, no one doubts; but that this influence is so marked as to impress on the disease such a particular stamp—such a decided aspect—as to make in a word a distinct species, appears to us questionable. There are special ophthalmias, decided in form, of which no person can deny the existence, and which require a separate description; such are the purulent, scrofulous, and syphilitic ophthalmiæ. But is it so with the ophthalmiæ called erysipelatous, catarrhal, abdominal, arthritic, rheumatismal, &c.? Such is not our opinion." (p. 382.)

Though we entirely agree with the writers as to the perplexity, unbalanced by corresponding advantage, caused by useless subdivisions, we can by no means assent to the rather startling proposition of striking out "sclerotitis" from the list of ophthalmic disorders. Yet this proposition is seriously made, and, strange to say, under the assumed support of Dr. Mackenzie:

> "On this point," says the author, "we are quite disposed to adopt the opinion of Mackenzie, and to deny the specific character (*la spécificité*) of the form of ophthalmia described under the name of sclerotitis. Much rather we would ask, with M. Velpeau, if there exists in reality a sclerotitis,—that is to say, an inflammation susceptible of beginning, under the influence of certain causes, in the fibrous membrane, of localising itself there, and of running through its different periods? To this question we do not hesitate to answer in the negative. In the descriptions of authors we find, in effect, but an assemblage of phenomena, dissimilar and variable, which we have already indicated, or shall indicate hereafter, as the symptoms of inflammation of some one of the membranes of the eye—the conjunctiva, the cornea, the iris, or the retina, &c." (p. 489.)

The first remark we have to make on this is, that Dr. Mackenzie does not anywhere in his treatise deny the existence of the form of inflammation described under the name of sclerotitis. What he says is this—we quote from the last edition:

> "By rheumatic ophthalmia I mean simply inflammation of the fibrous membrane of the eye (the sclerotica) and of the adjacent parts, of similar structure, excited by exposure to cold. I do not regard this ophthalmia as an inflammation differing *in kind* from common inflammation in consequence of the existence of what has been called the rheumatic habit, or diathesis. The train of symptoms seems to

depend, not on the constitution of the person, but on the structure and functions of the part affected." (p. 506.)

How this can be interpreted into a denial of the existence of sclerotitis we are at a loss to imagine; it merely refers to a form of sclerotitis; and really we feel that we should be trifling with our readers if we were to enter into an elaborate discussion on a point which, with those practically acquainted with ophthalmic diseases, can admit of no doubt. That an inflammation commencing in one tunic of the eye is likely soon to involve other tunics, and to take on the characteristic symptoms of their inflammations no one will deny; but to say that the conjunctiva, the iris, the cornea, the choroid, the retina, may each have its inflammation, and the sclerotica—that dense fibrous tunic—is to enjoy an immunity, is so opposed to common sense and to experience, that it would be a mere waste of words to argue the point further.

In the section devoted to *Conjunctivitis*, there is only one point which calls for remark. Speaking of "revulsive" measures, the authors say: "The English surgeons prefer calomel: some of them have even proposed to administer it with a view to obtain salivation." (p. 443.) If this is intended, as it appears to be, to convey an impression of the English style of treatment, we beg to protest against it. There prevails on the Continent an absurdly extravagant notion of the fondness of the British for calomel, which we are supposed to prescribe in fabulous quantities, and for the slightest ailments. Amusing enough is the following grave protest against our (imaginary) treatment:

> "A moderate purgation is certainly useful in inflammation of the conjunctiva, and all means which produce it are equally advantageous; but we have renounced the giving of calomel to the extent of salivation, because this salivation is often inconvenient, sometimes followed by lively pains and much exhaustion, and the patient can very well be cured by the employment of medicines which do not possess this inconvenience." (p. 443.)

Undoubtedly! and we should like to know what English modern writer recommends salivation for the cure of simple conjunctivitis? Certainly the sins of the fathers may be said to be visited upon the children in the bad odour that English practitioners have derived from the heroic practice of some of our predecessors; but it is to be hoped that this, as well as some other misconceptions as to our habits, customs, and character, will melt away under the genial influence of the "*entente cordiale*."

The more British ophthalmic practice is studied by our continental brethren, the more, we will venture to say, they will have reason to be satisfied with it as a whole; and as we are under many and great obligations to them, so they, perhaps, may not find it injurious or derogatory here and there to take a leaf out of our book.

One of the distressing consequences which occasionally follows purulent ophthalmia and inflammation of the cornea, is the production of the vascular opacity to which the term "*pannus*" has been applied—an opacity exceedingly intractable, and rendering the organ almost useless. There is a mode of treatment, the credit of originating which is given to Jäger, of Vienna, but which, in reality, attaches to the late Dr. Henry Walker, who published an account of it in the 'Edinburgh Medical and Surgical

Journal' so early as 1811. To Jäger and Piringer undoubtedly belong the demonstration of the utility of this practice of inoculation of the eye with matter from an eye acutely inflamed, and so setting up a new action in the part. In Belgium and Germany this mode of treatment is common. Messieurs Denonvilliers and Gosselin say: "The French surgeons have no faith, at present, in these results,—at least, we do not know of any case in which inoculation has been practised by them."

In August, 1854, we had the pleasure of visiting the clinique of M. Desmarres, at Paris, and there saw two cases in which this plan had been adopted with highly satisfactory results—the cornea in each being at that time merely somewhat hazy. The subject deserves attention; and we may refer our readers who may desire the fullest information concerning it, to an excellent essay by Dr. Warlomont, of Brussels, to which we shall hereafter more particularly allude, remarking, *en passant*, that an impartial and minute account of the progress of two cases in the practice of Jäger, and watched by Mr. Wilde, is given by that gentleman in his work on 'Austria and its Medical Institutions,' p. 251.

Staphyloma pellucidum, or conical cornea, is spoken of as very rare, and we are inclined to think that it is more so on the Continent than in England. We speak within the mark when we say that we have seen at least thirty cases, whereas some of the continental authorities state that no example has ever come under their notice. The writer says: "This affection, which is definitely very rare, has not been sufficiently studied for it to be known positively whether or not there be thinning of the summit of the cone."

On this point we are able to speak from recent observation. A clergyman consulted us for well-marked conical cornea. The projection in the left eye was extreme, and a few days after his first visit he called to say, that the previous evening he had slightly struck the eye, and he fancied that it had burst, for a great flow of fluid came from it, and he no longer felt the sharp apex of the cone. On examination, we found that he was perfectly correct: the cone was notably diminished, and, with a lens, a small fissure was visible at the extreme apex, indicating the seat of the rupture. This spot was touched with nitrate of silver on three successive days; and the fact of especial interest is, that the sight has steadily improved, and the form of the cornea is no longer that of a sharp cone throwing off and breaking up the luminous rays, but the stimulus of the caustic has produced thickening, contraction, and rounding of the apex, and corresponding amendment of vision.

The authors make no allusion to optical apparatus as a means of assisting the sight in conical cornea, but we may suggest to them that an opaque diaphragm, with a transverse slit or a simple aperture, either by itself or combined with a plano-convex lens, will in many cases prove a valuable acquisition.

Mydriasis, or permanent dilatation of the pupil, is frequently a very intractable, always a very annoying, affection; for not only is the sight of the affected eye troubled and rendered different from that of the other, but the retina, being deprived of the protection afforded by the beautiful action of the iris, always, as it were, on guard to admit only such light as is proper for perfect vision, is constantly overwhelmed by the flood poured in upon it. According to our own experience, mydriasis, whether from natural causes or artificially produced, is but little amenable to treatment, until the exciting cause has ceased. We have tried ergot of rye as snuff, tincture of aconite, solutions of opium, and the application of nitrate of silver, but without evident effect. It would be a great desideratum if we could meet with a certain and prompt means of causing the pupil to contract after dilatation with belladonna. Patients often complain of the annoyance caused by such dilatation continuing for some days after the pupil has been expanded for the purpose of examination; and not unfrequently lay the blame of any subsequent aggravation of blindness upon the surgeon who has so treated them. The authors allude to the treatment proposed by M. Serres, of Uzès, of touching the cornea every two or three days with nitrate of silver, but do not mention the less painful, and, in many cases, more efficacious mode proposed by Fronmüller, which consists in making the patient read with the affected eye for a certain time each day, with a high convex lens, gradually diminishing the power as the pupil contracts. The retina is thus stimulated, and the stimulation is conveyed to the brain, and from it is carried to the ciliary nerves and the third pair. We may mention a highly useful palliative in the treatment of mydriasis, which has in many cases afforded great relief. It is by excluding the excess of light by means of an opaque screen, or diaphragm of horn or blackened tortoise-shell, surrounded by black silk, and having only a small central aperture, or a slit. As this mechanically imitates the contracted pupil, it not only affords comfort, but materially assists the sight by excluding those circumferential rays which, not being sufficiently refracted, render the picture on the retina confused and indistinct.

Under the section devoted to Paralysis of the Muscles of the Eye, some interesting remarks are to be found. These cases too frequently baffle our art. There are some in which we are fortunate enough to diagnose their precise cause, and in which, by judicious treatment, a cure may be effected. Under this head falls that class in which ptosis and other indications of paralysis occur in connexion with tertiary syphilitic symptoms. The connexion between the two was long since pointed out by Ricord. M. Gosselin had under his care at the Hôtel-Dieu a female affected with exostoses and pains in the bones in both legs, and in whom there was, at the same time, paralysis of the left third pair. A four weeks' course of iodide of potassium effected a complete cure. The case is considered by M. Gosselin to have been plastic effusion into the fibrous canal of the dura mater, whence resulted the compression of the nerve, and consequent paralysis.

Messieurs Marchal* and Nottat† have pointed out the connexion which occasionally exists between paralysis of the third pair and neuralgia of the fifth. An interesting case of this description fell under our own notice in June, 1854. A lady was attacked with erysipelatous inflammation of the head. This was followed by ptosis of the right lid, and eversion of the eye, causing double vision. There was also a depraved sensibility of the forehead and right side of the head. Slight touching was not felt, but the least disturbance of the hair, especially brushing, however light, caused such pain as to be insupportable, and she was greatly annoyed with a tingling sensation of the forehead. There was also this curious symptom: any attempt to turn the right eye inwards, whether to follow an object or to look straight, caused a profuse flow of tears and violent sneezing. This lady derived great benefit from a chloroform embrocation locally, and the iodide of potassium combined with iron internally.

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* Archives Générales de Médecine, quatrième série, t. xi. p. 261.

† Idem, cinquième série, t. iv. p. 290.

Hemeralopia, or night blindness, is a disorder which has puzzled many an able practitioner. Years ago it was suggested by Mr. Telford, in Sir Gilbert Blane's 'Treatise on Diseases of Seamen,' that it may occur as a symptom of scurvy. Mr. Bampfield was so convinced of this, that he made *Hemeralopia Scorbutica* a class of itself; yet, until recently, this connexion attracted little further attention. The fact is just alluded to by Messieurs Denonvilliers and Gosselin; but a mass of information has lately been brought forward, proving this connexion to be strongly marked, and existing when little suspected. This information has been partly afforded by reports from the Black Sea fleet, furnished by Mr. Reece and Dr. Nicholls. Mr. Ranald Martin and Dr. Murchison inform us that the connexion between bad food and hemeralopia was a common subject of remark in India and in Burmah, and three well-marked cases of hemeralopia, clearly traceable to a scorbutic condition produced by the wretched food and overwork which decimated our troops in the trenches before Sebastopol during the sad winter of 1854, have fallen under our own observation. The practical point deducible is, that the proper treatment is not bleeding, nor purging, nor lowering, as might be necessary if the retina had been over-excited by strong glare, but good, nutritious diet, tonics, and the free administration of fresh fruits and other anti-scorbutics. A striking fact connected with this interesting class of cases, is the extreme mental depression which prostrates the sufferers. Hope seems to be abandoned; and so powerful is the sympathetic influence, that the whole crew of a vessel have been driven almost to desperation from the morbid fear of total blindness, when hemeralopia —scorbutic, doubtless—appeared amongst them.

We may mention, that the particular form of medicine which we found eminently serviceable, was twenty drops of liquor cinchonæ, with two tablespoonfuls of lemon-juice in water, thrice a-day. This combination seemed to exert a more powerful influence on the malady than any of the ordinary formulæ, whether of quinine or iron, separately or united.

A striking illustration of the credulity of the English, especially that portion facetiously denominated "the upper ten thousand," is afforded by the eagerness with which they listen to the marvels said to be accomplished in the cure of cataract without operation. There is generally some charlatan in fashion for whose skill and success our high nobility and clergy are ready to do battle. At present the fortunate individuals who enjoy the popular favour are to be found, the one in Paris, the other in Rhineland, and to these our countrymen and countrywomen flock, literally, in crowds. That they return light in pocket and heavy in heart, in too many instances, is a dismal fact. That the Parisian ophthalmologists have not yet recognised the value of the shining light which has appeared amongst them, liberally fed though it be with British gold, is also evident from the following passage, in which Messieurs Denonvilliers and Gosselin speak like men of science and honour:

> "Cataract can only be cured by operation. Certain charlatans have announced that they have succeeded by means of pomades or liquids. These preparations contain belladonna, by means of which the pupil is dilated and the sight a little assisted. Not only is there no means of curing cataract, but we know of nothing which can retard the progress of the disease when it is declared." (p. 678.)

Considerable advances have of late been made in our knowledge of the true nature of the changes which take place in the crystalline and in its capsule, giving rise to the different varieties of cataract. In 1842, Malgaigne put forward the opinion, that whatever the species of cataract, the true capsule never becomes opaque. This gave rise to a warm controversy, extending up to the present time, in which Sichel, Guepin, Leroy d'Etoilles, Szokalski, Häring, Bosch, A. Richard, and others, have taken part. The researches of Stellwag, however, have thrown much light upon the question; and the results obtained from the microscopical examination of about fifty apparently well-marked capsular cataracts, are, that in no instance was the opaque material deposited in the tissue of the capsules, but invariably upon, and attached to, their lenticular surface—an important distinction.

The latest investigations respecting the pathological changes in the crystalline are those of M. Sichel, and are contained in the sixth chapter of his admirable 'Iconographie Ophthalmologique.' He shows conclusively the extensive alterations which take place in the lens from defective nutrition, in cases of senile cataract, adding, with Messieurs Denonvilliers and Gosselin, his conviction that it is hopeless to attempt to cure such cataracts without operation.

M. Sichel divides the elementary alterations of structure into four groups—alterations of the actual fibres of the crystalline; deposits of granular material in, or between these fibres; deposits of fatty matter in the fibres, or more frequently between them; and accidental productions.

The true fibres may become slightly granular, sprinkled, as it were, with molecular granules, or with an exceedingly fine powder, or finely striated longitudinally; at other times they are brittle, thin, and irregularly wavy. These changes take place especially in the soft, pultaceous, diffluent cortical layers. The fibres lose their sharp outline, their contour becoming indistinct. The nuclei of the nucleated fibres may disappear altogether. There are also certain changes in the disposition of the fasciculi of the crystalline bands. Sometimes they are disarranged, resembling the fibrillæ of cellular tissue; at other times, disposed in bundles, and separable with difficulty, they become more coherent, so as to form masses which it is very difficult to subdivide into fibres, and which present an homogeneous aspect, occasionally striated.

The fine granular matter is found in the substance or in the interspaces of the layers of the crystalline. It assumes the form of globules, or spherical or oval corpuscles. This degeneration plays an important part in the opacification of the lens, and, according to M. Gros, the principal part; strictly, it is the production of an amorphous material among the microscopical elements of that body.

Between the fibres of the crystalline, or more rarely, in the fibres themselves, there may be found fatty matters of different kinds, some liquid, having the aspect of oil globules, or oily and fatty drops, most numerous in the soft superficial portion, or of drops fatty in appearance, but less marked, and not presenting the yellow tint of ordinary fat; others are solid, occasionally amorphous, but at other times having the form of crystals of cholesterine. The cholesterine, M. Sichel remarks, is probably a pathological product when in considerable quantity; in moderate quantity it naturally exists, though not crystallized, in the nucleated fibres of the sound crystalline. The fatty transformation is particularly evident in certain cases where there are points of fatty degeneration strongly marked between the altered fasciculi of the lens.

This summary of the chief facts may interest some of our readers, especially as they are not to be found in ordinary works on ophthalmology.

The account given by Messieurs Denonvilliers and Gosselin of 'Myodésopsie,' or muscæ volitantes, is not only meagre, but by no means up to the knowledge of the day. For instance, they seem to be unacquainted with the very elaborate and valuable researches of Dr. Mackenzie, which have thrown so much light on this obscure subject—far more, indeed, than either of the authorities quoted. The authors themselves have arrived at the opinion that

> "Muscæ volitantes, like incomplete amaurosis and kopiopia, are due to a weakness of the retina; only here, the weakness which is produced by fatigue—a momentary congestion—has for its special character that of shifting and occupying successively many limited points of the membrane, so that the filament (the musca) shifts, simply because the point, momentarily insensible, changes every instant." (p. 782.)

Before the publication of a second edition, we strongly recommend to these gentlemen a careful perusal of the papers by Dr. Stark* and Dr. Mackenzie† in the 'Edinburgh Medical and Surgical Journal,' or, better still, the article Myodesopia, at page 949 of the fourth edition of the 'Practical Treatise on Diseases of the Eye,' by the latter distinguished writer. They will there find information which will probably lead them to alter their views as to the character of the affection in question.

On the whole, this work may be regarded as a fair representation of the state of ophthalmic science in France. The circumstance of the authors being general surgeons leads them, in many parts, to rely more on the experience and statements of others than their own observations. This imparts to the volume much of the character of a compilation, and renders its performance unequal, some portions, evidently written *con amore*, being better executed than others, in which the authors were less at home. It has not appeared to us necessary to analyse, *seriatim*, each subject contained in its chapters, for the reason that there is little novelty to be found. As we turn over page after page, we meet with familiar facts, familiarly and clearly stated—in itself a merit, but affording small scope for criticism.

Messieurs Denonvilliers and Gosselin have evidently bestowed pains upon their work; and although, in some respects, it might have been improved, especially as concerns the writers of other countries, yet it is a meritorious production, and will be a useful book of reference to those interested in the subjects of which it treats.

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* On the Nature, Locality, and Optical Phenomena of Muscæ Volitantes, vol. lx. p. 899.

† On the Vision of Objects on and in the Eye, vol. lxiv. p. 38.

Mention has already been made of the work which stands second in the heading of this article. Purulent ophthalmia seems to be a common disease in the Belgian army, and Dr. Warlomont, who was for some years a military surgeon, enjoyed extensive opportunities of observing it and its effects, including, of course, the morbid changes termed 'pannus.' Struck with the unsatisfactory results of the ordinary modes of treatment in use for this disease, he turned his attention especially to it; and his chief object in the publication of the essay before us, is to bring into prominent notice a remedy which, in his opinion, has not received the attention it merits.

Such a formidable disease as 'pannus' has naturally attracted the attention of ophthalmologists, and it has formed the subject of theses by Hagen, Bratsch, Schrey, Vorstmann, Holtzinger, &c.; still, it must be confessed that the treatment by inoculation has not made much way. It is now upwards of forty years since the proposition was enunciated, to set up a new action in the diseased cornea by introducing into the eye pus, either gonorrhœal or from a purulent eye, with the intention of exciting acute inflammation—an artificial purulent ophthalmia. Jäger of Vienna, Piringer of Gratz, Hairon of Louvain, and Von Roosbroeck of Ghent, have severally tried this system extensively, and the results obtained by them and by others, as stated by Dr. Warlomont, are unquestionably worthy of consideration.

In this treatise, the anatomy, normal and pathological, of the cornea, the etiology, symptomatology, and treatment, general and local, of pannus, are first elaborately passed in review; and the volume is completed by a minute description of the practice by inoculation, supported and elucidated by thirty cases. It is on this portion alone that it is necessary for us to dwell.

Inoculation is not adapted for all cases of pannus. Dr. Warlomont warns us that it is only admissible where a dense network of vessels and fibres covers and obscures every portion of the cornea, depriving the eye of sight, it is true, but shielding the transparent membrane behind the mass from the pernicious effects of a new purulent inflammation. It is contra-indicated where there are portions of the cornea clear and unaffected with disease.

The idea of deliberately establishing in an eye the most formidable and destructive of the many forms of inflammation to which the organ is subject, is repugnant to most practitioners. The risk is thought too great; and, indeed, it would only be admissible upon very strong evidence. On this point Dr. Warlomont remarks:

"The practice which we recommend, with the profound conviction inspired in us by the energetic language of *facts*, will find numerous adversaries, but chiefly among those practitioners who have not tried nor seen the treatment. With such, the idea of imparting a disease of which the direful effects are known to them, strikes them with fear, and makes them recoil from the experiment. A word to reassure them: if, in sound eyes, the most grave disorders are often the consequence of blennorrhœa, we may affirm that it is otherwise when the cornea is protected by the vasculo-membranous web which covers it. On this protecting covering the morbid action spends itself, and when it has caused it to disappear the action stops. That these phenomena may be difficult to explain we admit; but when experience has spoken, its decision must be accepted." (p. 85.)

This sentence, in fact, states in a few words the principle on which the treatment is supposed to proceed. The new action excited under the inflammation alters the whole condition of the parts, softens the deposits which cause the opaque hypertrophy, and produces such an effect, that when the acute inflammation subsides under the treatment applicable to ordinary purulent ophthalmia (which is directed to be vigorously employed), the vasculo-nebulous opacity passes away, leaving the cornea in a more or less clear and useful condition.

In questions such as these, involving important consequences, much will depend on the weight to be attached to the testimony adduced. The names of Von Roosbroeck and Warlomont (who are responsible for the cases related in this treatise) are unexceptionable; the cases, though briefly, are fairly narrated; and the results, it must be allowed, are highly encouraging.

There is, then, full reason to believe that sight may be greatly benefited by purulent inoculation in many of those otherwise hopeless cases which stand as an opprobrium to the healing art—perplexing the practitioner and wearing out the patience of the sufferer. But we are convinced that cautious discrimination is necessary in selecting the cases, and feel sure that no man ought to expose an eye to the torture of the accompanying inflammation, nor the patient to the severe treatment necessary to subdue that inflammation, who cannot exercise the strictest supervision, and devote to his patient the time and sedulous attention such a case imperatively demands.

White Cooper.

REVIEW X.

Beitrag zur Pathologie des Menschlichen Eies. Von — SCANZONI.
('Prager Vierteljahrsschrift,' i. 1849.)*

(*Continued from No. 29, p. 172.*)

IN preceding articles we have described some of the pathological states of the placenta which especially involve the fœtal element, reserving others for more convenient discussion hereafter. We proceed with the investigation of those diseases which are dependent upon, or associated with, abnormal conditions of the mother's blood, or of the uterine elements of the placenta.

We have already described that form of congestion which may be called fœtal, in contradistinction to the congestion of the maternal portion of the placenta. We pointed out, that in the placenta approaching maturity, fœtal congestion is, probably, in most cases accompanied by maternal congestion, constituting a mixed form, or general placental congestion. In the same way as asphyxia in the air-breathing animal induces congestion of the circulatory system of the *lung*, so does interruption to the flow of maternal blood through the placenta, or asphyxia in the blood-breathing embryo, induce congestion in the fœtal vessels of the placenta. There are many ways in which such asphyxia, more or less complete, may arise. Local mechanical causes may produce it; and so may the circulation in the maternal system of impure blood, that is, of blood incapable of adequate oxygenizing action; or, not less certainly, a feeble, tardy maternal circulation, that fails to bring into contact with the fœtal blood a sufficient body of oxygenated blood for the purposes of elimination and nutrition.

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* The titles of numerous other works on Diseases of the Placenta will be found at the head of the preceding articles on this subject, Nos. 27 and 29.

It is doubtful whether fœtal congestion often leads to extravasation of blood from the fœtal vessels; that is, it is doubtful whether what is usually called apoplexy of the placenta often depends upon rupture of, or exudation through, the fœtal placental vessels. Apoplexy, in the great majority of instances, is the result of extravasation of maternal blood; and this is true both in the case of the mature and of the newly-formed placenta. Congestion of the maternal placenta easily leads to extravasation. The delicate walls of the canals in which the maternal blood flows, composed, as they are, of a simple extension of the lining membrane of the uterine vessels, covered externally with what in many parts vanishes into a mere theoretical extension of the decidua, readily yield before any extraordinary pressure. The frequent consequence, then, of congestion is apoplexy; and thus we are naturally lead to the study of this affection.

Apoplexy of the placenta has been studied with great care by several observers, more especially by Cruveilhier, Dubois, Scanzoni, Gierse, and H. Meckel.

Many errors relating to its etiology, and particularly as to its consequences, have nevertheless been enunciated. Our endeavour will be to set forth a clear and consistent view of the pathology of this affection.

Causes of blood-extravasations in the placental tissue.—Not to omit reference to placental and fœtal conditions disposing to extravasation, as fatty degeneration, it will be inferred from what we have already said, that the principal causes of extravasation are the causes of congestion. Thus, all conditions that lead to deterioration of the mother's blood—anæmia and hyperæmia, defective power of circulation, and excessive vascular excitement—may be causes of congestion, and, secondarily, of extravasation. These conditions are homologous with deterioration of the air, either by deficiency of oxygen, or contamination with carbonic acid and other asphyxiating or poisonous gases, in extra-uterine life,—conditions which necessarily induce congestion of the pulmonary vascular system, and sometimes extravasation.

To enumerate these conditions would be to give a list of all those diseases which cause dyscrasia of the blood, or toxæmia. The fevers exert a marked influence; typhus, small-pox, measles, scarlatina, acute rheumatism; acute inflammations, especially pleuritis and pneumonia; many chronic diseases, as phthisis, scrofula, scurvy, obstructive heart disease, cirrhosis of the liver, granular degeneration of the kidneys, uterine or ovarian irritation or disease; and conditions leading to exhaustion, as hæmorrhages and lactation. We have, in a special memoir, proved by numerical researches the great influence of menstruation and lactation in producing abortion.* Some of these diseases—for example, typhus and scurvy—especially dispose the blood to transudation. Others—as pneumonia and pleurisy, regurgitant heart disease, cirrhosis of the liver, and granular degeneration of the kidneys—not only predispose to placental congestion and extravasation through the attendant dyscrasic or toxical properties of the blood, but may also act by retarding the return of blood from the pelvic organs. Uterine congestion and inflammation can scarcely exist without occasioning placental congestion. But Gierse undoubtedly takes far too narrow a view of the etiology of this affection, when he states in general terms, that it depends upon a diseased condition of the uterus and its mucous membrane. Deficient exercise and improper diet also frequently lead to the same result, although no ostensible disease ensue. If the mother's nutritive and eliminative functions be imperfectly performed, the placental circulation is sure to be affected. Functional disturbance of the lungs, or liver, or alimentary canal, independent of organic disease, may have a similar effect. It is well known what influence an attack of gastric irritation may exert in producing congestion of the uterus. Should such an attack occur during gestation, the uterine congestion is necessarily extended to the maternal placenta, and it may even proceed to extravasation of blood into the parenchyma of that organ, to discharge of blood from the cavity of the womb, and to abortion. To these internal causes we must add the influence of emotion, of ovarian irritation, of spasmodic action of the uterus, and of direct violence to the uterus. The influence of each of the conditions referred to is proved by the frequent occurrence of abortion during their existence or after their operation.

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* An Inquiry into some of the Relations between Menstruation, Conception, and Lactation; and the Influence of Lactation in causing Abortion. Lancet, vol. ii. 1852.

In a large proportion of the cases of abortion so occurring, not only very marked congestion, but extravasations in various forms, are found in the membranes or placenta. The abortion from congestion or extravasation is brought about in one of two ways. The embryo may or may not be destroyed prior to the expulsion of the ovum. The process of exclusion may be slow and gradual, or abrupt. But the chief distinction that pathological observation leads us to make, is that between abortions following upon simple congestion, and abortions from congestion complicated with extravasation. In the first class the process is as follows: In those conditions of the maternal system which bring about a gradual deterioration of the blood, the placental congestion is of a passive character; the nutritive and eliminative changes required by the fœtus are consequently imperfectly carried on; but it is only gradually that the embryo suffers, and the moment of its death may be long postponed. In abortions of this class the death of the fœtus is the *first* step or stage. The *second* consists in the death of the placenta, which, for the most part, but not always, soon follows upon the withdrawal of that attractive force which the life-processes of the embryo supply. When we say that the death of the placenta, for the most part, but not always, soon follows upon the death of the embryo, it must be remembered that we speak of that organ in the aggregate. The fœtal portion, we believe, always dies, and immediately, as necessarily as does the lung of the air-breathing animal, with the death of its other organs. It is the maternal portion that may, and sometimes does, live on for an indefinite period. The circumstances under which this may occur we shall consider hereafter. But whether the whole die immediately after the death of the embryo, or whether the maternal portion retains for a time its vascular connexion with the uterus, the probability is great that the embryo will soon be expelled from the cavity of the womb. It often happens, especially in the case of abortion after the fourth month, that the embryo is expelled before the placenta and membranes, these last retaining a more or less intimate relation to the uterus for some time longer. But in abortions at an earlier period of gestation, and especially as we approach the third and second month, the ovum usually comes away in a mass. But for this to happen, a *third* stage must be completed: the death of the fœtal and maternal placenta and envelopes taking place retrogressively, the vascular connexions between the uterus and placenta are cut off; the uterus itself, no longer stimulated to active growth, falls back towards the unimpregnated condition, that is, it undergoes a process of involution, its blood-supply diminishes, and its muscular structures, first feeling the want of nutritive elements, and then undergoing fatty metamorphosis, the whole body of the uterus rapidly contracts in all its dimensions. But simultaneously with the involution of the muscular structure of the uterus, the decidua or mucous membrane is undergoing a similar process. The end of the involution of the uterine mucous membrane is exfoliation, or detachment; and the minute observation of a considerable number of aborted ova, in cases where the abortion followed upon death of the embryo, has satisfied us that this gradual detachment of the mucous membrane or decidua is effected by a fatty metamorphosis of its elements. This detachment effected, the ovum lies loose in the cavity of the uterus, and is, in all respects, a foreign body. When the contraction of the uterus attending its advancing involution has attained a certain point, the dead ovum is pressed upon by the walls of the diminishing cavity. The contraction which up to this moment had been simply atrophic and passive, is now exchanged for active muscular contraction, the result of reflex or diastaltic excitation. Under this spasmodic action, the expulsion of the ovum—the *fourth* stage of abortion—is soon effected. But sometimes the stimulus to expulsive uterine contraction is of a different kind; the diastaltic arc does not begin and end in the uterus itself. To explain this form we will avail ourselves of the language of Dr. Tyler Smith, in describing the theory of the "genesial cycle." We assume, as in the case just described, that the involution of the uterine muscular structure and mucous membrane has set in; from this moment the period of uterine domination over the mammary and ovarian elements is at an end; the period of mammary domination is, in the case of abortion, but transitory; the ovaries resume their sway, and on the first occurrence of a menstrual nisus, conditions ensue which rarely fail to bring about the expulsion of the dead ovum. The ovary is, in this case, the commencement of the diastaltic arc, the uterus the termination. The active uterine contraction thus excited may anticipate the period of complete detachment, by involution of the decidua, and may effect a forcible separation. What violence sometimes attends this separation of the maternal element of the placenta from the uterus, may be judged of from a fact we have often observed: the uterus, contracting with spasmodic fury, not only casts off its mucous membrane, but even numerous muscular fibres; and these may be seen, by the aid of the microscope, attached to the decidua on the external surface of the expelled ovum. Portions of the muscular wall are rent off along with the mucous membrane.

Such is the course of the abortive process, as it usually takes place when the death of the fœtus, ensuing upon passive congestion, or slow asphyxia, constitutes the first step. But abortion frequently happens much more suddenly. Under the influence of any of the causes producing congestion, it may be that, owing to a peculiar hæmorrhagic condition of the blood, the exaggerated force with which the congestive cause acts, or more frequently, to the intercurrence of some powerful exciting cause, active congestion of the uterus, of course extending to the decidua or maternal placenta, is induced, which is quickly followed by extravasation into the placental parenchyma. Should the extravasation be extensive, so as to disable *suddenly* a large portion of the placenta, the fœtus is destroyed immediately; and in all probability the commotion set up in the uterus goes on to excite active contraction, so that the forcible separation of the ovum and its expulsion are effected. Sometimes, however, although the extravasation is extensive, the embryo is not so immediately killed, but that the complete detachment and expulsion of the ovum precede, the embryo being born alive. In such cases it commonly happens that the ovum is ruptured by the violent compression of the uterus, and the embryo expelled before the membranes. This we have observed in several cases of abortion from active congestion and hæmorrhage occurring between the end of the fourth and beginning of the eighth month.

We cannot pursue the history of abortion, although it is intimately associated with the question before us, so far as to discuss, with the minuteness the subject deserves, the relative efficacy in producing abortion, of the various causes of placental congestion and hæmorrhage which we have enumerated. We wish, however, to state our opinion, that various influences which have been considered as operating primarily and specifically in producing abortion, have in most cases but a secondary, and frequently an accidental, action. To draw up an enumeration of the causes of abortion, discriminating absolutely between predisposing and exciting causes, as some authors have done, is a difficult if not impossible task. But we believe that a true theory of the etiology of abortion must recognise such a distinction; and that if we cannot at present realize it, we must hold out that want before us as an incentive to a more diligent and precise study of the subject. Without asserting absolutely that there must be in every case of abortion a predisposing *and* an exciting cause, we believe that a correct analysis of almost any given case in which we possess full information as to the antecedent history of the mother, the course of the symptoms that immediately preceded and terminated in the expulsion of the ovum, and especially as to that which is most generally overlooked—the condition of the embryo and membranes—would demonstrate the existence of some condition anterior to that which appeared to be the immediate, and was assumed as the real, cause of the abortion. For example, it is not unusual to ascribe an abortion to the influence of emotion, as fear, grief, or joy. The influence of emotion over the uterus is indeed great, but we should be careful not to over-estimate it. In many women of great nervous susceptibility, a strong emotion, suddenly caused, will, either in the non-pregnant or pregnant condition, instantly determine a flow of blood to the uterus; sometimes the only evidence of this is found in the sudden pain, and sense of weight and heat, in the region of the womb; but not seldom the congestion is so great as to find relief in a copious transudation of blood from the uterine mucous membrane, which escapes externally. When the exciting emotion occurs near the menstrual epoch, the advent of the menstrual discharge is commonly hastened. If it occur during the menstrual epoch, the discharge is rendered excessive. During gestation, the same susceptibility to emotion may induce a similar local determination of blood. External hæmorrhages occasionally happen, not necessarily interfering with the due progress of gestation, but certainly in some cases acting as the immediate cause of abortion. The occurrence of abortion depends, we believe, very much upon the spot whence the blood issues, and upon whether it find a ready outlet from the womb. In the early months of pregnancy, when the whole chorion is surrounded by a thick and highly vascular decidua, the hæmorrhage may proceed from a part of the uterus distant from the forming placenta, and the adhesion between the decidua at this part and the chorion not being as yet intimate, the blood runs between the two membranes, and may find an escape externally. In this case, abortion may be averted—a fact of therapeutical importance. But the blood may *not* escape externally: in this case it may spread beyond the limits of the spot whence it issued, so as to infiltrate the decidua throughout a large extent of its circumference, to break up the yet delicate connexions between the decidua and chorion, to compress the placental vessels, and so to cut off the means of life of the embryo. Abortion so induced—assuming that there is no complication from maternal blood-affection, disease of uterine mucous membrane, or of the ovum proper—may be attributed primarily and solely to emotion. But such cases are rare. In a healthy woman, carrying a healthy embryo, emotion, as the rule, has no such effect.

The same reasoning applies to all those sources of excitation of diastaltic action, which is often the precursor of abortion. It is true that a source of irritation existing in the breasts, ovaries, alimentary canal, bladder, or in the uterus, may evoke the energy of the spinal system, which may expend itself in uterine contraction, and uterine contraction long kept up will probably end in abortion. But we venture to affirm that abortion, induced purely in this manner, is of very rare occurrence. The healthy uterus, containing a healthy ovum, is not at the mercy of every accidental emotion or diastaltic excitation. Such a doctrine would be an impeachment against the conservative foresight of Nature. The part commonly played by diastaltic action is indeed important, but it is essentially secondary and complementary to other causes. The efficient cause of abortion having already operated, the embryo having been destroyed, the fœtal envelopes or the maternal structures having been rendered unfit for their office, the diastaltic function may be called into action as the agent for discharging the uterus of its contents. Diastaltic action, in fine, is the mechanical force which completes the abortion, not its cause.

The forms in which blood-extravasations occur in the placenta.—Excluding, now, hæmorrhage from the fœtal vessels, and confining ourselves to extravasations from the maternal vessels as by far the most frequent and important, we shall endeavour to explain the various forms that are observed. The maternal source and seat of hæmorrhage are most unequivocally manifested in early abortions. In young ova the entire decidua is often found thickened to an enormous extent by universal infiltration with blood, part of which is still fluid, part freshly-coagulated, part condensed into firm masses of fibrin. Most commonly the decidual cavity, the space between the decidua uterina and decidua reflexa, is free from blood; but the quantity effused into the substance of the decidua compresses the uterine and reflected laminæ together, obliterating the cavity. Sometimes the effusion of blood is entirely confined to the decidua; but occasionally some escapes beyond the limits of this membrane, and flows into the loose tissue formed by the villi of the chorion. Very rarely is the membrane of the chorion or the amnion ruptured so that blood is found in the cavity of the amnion. When this does happen, it is probably most frequently owing to the violent compression exercised by the contraction of the uterus during the act of expulsion. But although rarely rupturing the amniotic sac, the blood effused or forced into the yielding tissue of the chorion-villi or new-forming placenta, forms rounded masses that raise the membrane of the amnion into irregular knobbed elevations, which looked at from within, have, in their blueish-black colour, irregular shape and aspect, some resemblance to varicose veins. This is the condition and appearance described by Baudelocque and Granville as "tuberculated ova;" an unfortunate name, which, faintly depicting the physical aspect, suggests a false idea of the pathological nature of the affection. This condition is well described and accurately interpreted by Dr. Simpson. Hæmorrhage may also take place between the uterus and the decidua, or between the two layers of the decidua. Sometimes the effused blood forms an uniform layer of considerable thickness, lying between the decidua and chorion, so as completely to invest the ovum.

In ova a little more advanced, that is, when the placenta is marked out, it is very rare to find extravasation of blood in the decidua, or between its layers, without also finding blood in the substance of the placenta. In this structure, the blood is commonly seen in the form of more or less rounded masses, the villi and loose parenchymatous tissue being torn, and sometimes so broken up that unless portions be submitted to microscopic examination, it is impossible to recognise the proper placental elements.

In ova of a still more advanced period, when the placenta is fully formed, the appearances assumed by hæmorrhages are different. The decidua is no longer so thick or vascular as in the earlier period; the placenta itself has become the principal seat of vascular development. Hæmorrhages between the uterus and decidua, into the decidua, or between the two layers of the decidua, are now rare. The almost exclusive seat of blood extravasations is the parenchyma of the placenta. Several distinct forms, depending either on the cause and source of the hæmorrhage, or on the peculiarities of structure of the part of the placenta into which the hæmorrhage has occurred, are observed.

Blood-effusions in the placenta appear in three principal forms:—

1. The extravasated blood forms for itself a wide irregular cavity in the centre of a cotyledon, often communicating with smaller cavities in

the vicinity. That a cavity of this kind be formed, it is obvious that a considerable quantity of blood must be effused suddenly; and this cannot take place without causing more or less tearing or breaking-up of the delicate parenchyma. The tissue surrounding the cavity is coloured dark and brown-red by imbibition. Owing to the laceration of the placental tissue and the compression caused by the effused blood, it is seldom that we are able to find remains of villi in the extravasation. But in the periphery of the cavity, villi, altered in various ways, may be detected. The condition of the blood will vary according to the length of time it has escaped. It may be fluid, semi-coagulated, or quite solid.

2. The extravasations may assume a lobular form, and be enclosed in sharply defined cavities, varying in size from that of a bean to that of a walnut. The seat of these may be near the fœtal or the uterine surface, and may cause projections, seen and felt under the normal tissues.

3. Scanzoni describes another form, in which one or more cotyledons are found dark-coloured, harder to the feel, the tissue more fragile, but no cavity containing blood. On section, however, there are seen several pear-shaped, dark-red foci, containing fluid blood, surrounded by hyperæmic tissue. Scanzoni has found this form exclusively in cases in which a long-continued pressure upon the cord, as in breech births, prolapsus of the cord, &c., has arrested the circulation. He infers that these extravasations arise from rupture of the fœtal vessels.

The first form of extravasation is beautifully illustrated by Cruveilhier in his plates, and described under the name of apoplexy.

A most important and interesting point of inquiry is the changes undergone by the effused blood. The appearances exhibited by blood-extravasations at different dates of the effusion have been variously interpreted; and some of those who have most minutely examined the subject go so far as to contend, that what was taken by others as evidence of inflammation of the placenta, was nothing more than effused blood variously altered: they deny the existence of placentitis altogether. But the evidence as to placentitis we must examine further on. We will now trace some of the undoubted changes to which effused blood is liable.

If abortion and exclusion of the placenta do not follow immediately upon hæmorrhage into its parenchyma, the blood soon loses its fluidity and dark-red colour. The mass first undergoes a separation into its fibrinous and serous elements, as it does when out of the body. The freed serum partly infiltrating the surrounding healthy tissue is gradually absorbed; part, surrounding for a time the contracted fibrin, serves for a macerating medium, and helps to extract the colouring matter; the fibrin itself goes on contracting, hardening, and losing colour. It is now obvious that, through the removal of the serum and the contraction of the fibrin, the fibrin, being all that remains, cannot occupy the same space as the mass originally effused; the placental parenchyma is not a contractile tissue; the surrounding structure does not, at least as the rule, collapse upon the diminished mass; there must, therefore, result a vacant space or cavity. Now cysts, or empty cavities of various sizes, sometimes as large as a walnut, are not very unfrequently seen in placentas; and we believe that their formation may be accounted for in the manner we have described. We have found the parenchyma forming the walls of, and immediately surrounding, these cysts, more hardened than normal, and the villi more or less atrophied, obliterated, or absent. These cysts may properly be called apoplectic cysts, and are strikingly analogous in origin and mode of formation to the apoplectic cysts of the brain. The process we have described, resulting in the formation of cysts, may be looked upon as one of the modes of cure of placental hæmorrhage. In one case in which we found five or six such cysts, accompanied by consolidation of other parts of the placenta, obviously from extravasated blood, gestation went on to the full time, but the child was born alive, although very small and feeble.

But it may happen that the extravasation may be neither so sudden and extensive as to cause immediate abortion, nor so dependent upon one accidental transitory condition as to end in one simple attack, leaving a large portion of the placenta unaffected, and tending to a cure. The morbid causes may be persistent, and the hæmorrhage recurrent. In this case we shall witness those appearances which are so faithfully depicted by Cruveilhier. We shall be able to trace in the same placenta all, or the greater number, of the transformations that sanguineous effusions can undergo. Confined to one cotyledon, or extending into several, we shall see a *foyer*, composed of several defined strata concentrically disposed, resembling closely the successively-deposited layers of an aneurismal tumour. On making a section of a placenta so affected, the diseased mass will be seen imbedded in the parenchyma; the layers of the circumference are commonly found to be composed of fibrin, condensed, and freed from colouring matter. These are evidently the result of effusions of a date long anterior to the expulsion of the placenta. Internal to these are layers of fibrin less condensed, and deprived to a lesser extent of the colouring matter: the result of more recent effusions. And the centre is occupied by blood partly coagulated, partly fluid, and still dark-red, or black: the result of effusion immediately preceding the expulsion of the placenta. Accompanying this condition, it is usual to find the tissue surrounding the seat of effusion more or less infiltrated with blood, partially indurated from the consolidation of this blood.

In another form of recurrent placental hæmorrhage, the blood is not extravasated in one or two large *foyers*, as in the preceding case, but in numerous small round masses, dispersed throughout every part of the organ, and having healthy tissue between them. In a placenta affected in this manner, we may sometimes see individual *foyers* exhibiting blood or fibrin in the different stages of metamorphosis that indicate distinct periods of extravasation; and also different *foyers*, some showing the hard, colourless fibrin of long-standing, and others consisting entirely of freshly-extravasated blood. In such cases there is commonly some disease of the placental tissues, such as fatty degeneration, predisposing the vessels to yield under moderate distension.

There is another form of placental hæmorrhage especially deserving attention, namely, that occasioned by partial detachment, as in cases of placenta prævia. In such cases, the phenomena presented by the extravasation of blood into the placenta may be observed in all their simplicity. The hæmorrhage depends upon purely mechanical causes; and the elements of the placenta and the blood itself may be perfectly healthy.

Here, also, we have frequently the opportunity of observing the different appearances assumed by different portions of blood poured out at different epochs. It is a familiar fact, that when the placenta is implanted on the neck of the uterus, the patient is liable to successive hæmorrhages, that may occur at intervals more or less distant. It was advanced by Gendrin, and the opinion is sanctioned by Simpson, that the occasional arrests of the hæmorrhage in these cases was owing to the coagulation of the blood poured into the separated portion of the placenta. This coagulation undoubtedly takes place, and must of course operate in impeding the continued flow of blood through the particular portion which is the seat of the coagulation. But it is not sufficient to explain the whole case. How it is that the flooding is arrested in placenta prævia, we have described elsewhere;* and as we shall in a distinct memoir revert to this subject, we shall not dwell upon it in this place. Dr. Simpson has well observed and described the changes referred to. We will quote his words:†—

> "The blood, diffused and infiltrated into and upon the detached portion of the placental structure, undergoes a series of changes; . . . . and after a time the separated and ecchymosed tissue of the placenta itself becomes yellowish and atrophied, partly from the alterations which occur in the blood infiltrated through it, and partly from the obliteration of its vessels, and the consequent degeneration and desiccation of its tissues. In cases of placenta prævia, in which there has been a repeated recurrence of hæmorrhage, and as frequently an arrest of it, we can occasionally trace in the placenta, after its expulsion, different parts of it, showing a series and gradation of pathological changes arising from successive partial detachments, and successive apoplectic infiltrations and obliterations of its substance, from coagulated blood of different ages lodged in its structures. These alterations are confined to the detached portion; and the part always presenting the most recent stages of the pathological changes in question, is that lying nearest the line of junction between the separated and affixed divisions of the organ. The part showing the most advanced stage of the changes will be found situated furthest from this point; or is, in other terms, the part which was first and earliest detached. In cases of direct and central implantation of the placenta over the os, the centre of the organ, having in general become first detached, will be found to present the oldest morbid alterations; and the newer forms and phases of it may be sometimes traced in successive departments or layers, from this to the circumference of the detached portion."

We are able to confirm this account; and in the paper referred to we described three placentas which exhibited these features. In these cases, the blood poured out is undoubtedly maternal. The mother shows unequivocal proofs of the loss of blood: in fatal cases her circulatory system is found empty; whilst the child exhibits no deficiency of blood, unless some of the larger umbilical vessels have been accidentally ruptured.

Blood undoubtedly maternal.—In cases of this kind the pathological changes observed in the parts of the placenta immediately surrounding the infiltrated spot, such as the loss of colour, the hardening, and the obliteration and atrophy of the villi, may for the most part be looked upon as secondary, and owing to the effused blood. But in cases other than those of placenta prævia and of hæmorrhage caused by mechanical violence, it is not unusual to meet with extensive organic alterations in the tissue of the placenta, some at a distance from the apoplectic spots; and which, it may be presumed, existed before the hæmorrhagic effusions, and which probably were the conditions that caused the hæmorrhage. We have already said that a diseased condition of the placenta is a frequent cause of hæmorrhage. It is stated in the Reviewer's first Memoir (1851):

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* On Flooding before Delivery arising from Adhesion of Placenta to the Os and Cervix Uteri.—Lancet, vol. i. 1847.

† On the Spontaneous Expulsion and Artificial Extraction of the Placenta before the Child in Placental Presentations. Lond. and Edin. Monthly Journ. March, 1845; and Obstetric Works, by Priestley and Storer, 1855, p. 729.

> "It is possible that the coats of the umbilical vessels may be so weakened by granular degeneration at a period when blood is still circulating in them, that rupture and hæmorrhage may ensue. This may be one cause of placental apoplexy, resembling that form of cerebral apoplexy described by Mr. Paget as occurring as a consequence of granular degeneration of the capillaries of the brain."

We have since seen cases which confirm this conjecture: cases in which there were well-marked and advanced fatty degeneration of the chorion-villi, and apoplectic clots, manifestly of recent formation. Several instances of this kind will be found in the Reviewer's second Memoir in the Medico-Chirurgical Transactions, 1853. Sanguineous effusions into the developed placenta from this cause, will, we believe, be found to be more frequently of fœtal than of maternal origin. One result of our observations—a result which we freely admit is liable to be reversed by larger experience—is, that in the early ovum fatty degeneration is more common in the decidua than in the chorion, whilst the converse is the case in the more mature placenta. But, with this exception, it may be taken as a general rule that hæmorrhage into the placental parenchyma is of maternal origin.

Robert Barnes.

(To be continued.)

PART SECOND.

Bibliographical Record.

ART. I.—*On the Influence of Education and Training in Preventing Diseases of the Nervous System.* By ROBERT BRUDENELL CARTER, M.R.C.S. Eng., Fellow of the Royal Medical and Chirurgical Society. *London*, 1855. pp. 438.

WE hold that there is no more interesting or more honourable occupation than that of instruction, when viewed in the proper light; but it is only too often observed, that parents and professional tutors regard education as a matter of routine, rather than as a question involving the highest and noblest aims of humanity, the deepest and most enduring interests of society. One would have supposed that the teacher would, above all things, consider it his duty to make himself acquainted with the nature and mutual relation of the materials that he has to deal with; that he and the public should regard as the first and most essential qualification for imparting knowledge, for moulding the young heart and intellect, for training the future man,—a knowledge of the various and varying powers of body and mind, an appreciation of the influences that affect, and the reactions which result from, all and every relation into which the child is brought.

There was a time when such remarks would have been regarded as chimerical, but there is much evidence of an advent of better things. The bed of Procrustes is no longer the schoolmaster's model, and the hatred which our great lexicographer asserted to be an inherent element in the relation of pupil and teacher, is giving way to mutual intelligence and attachment. It is especially the province of the medical man to assist in removing those barriers which intervene between the child and his instructor. This duty adds to, and still further hallows, his most sacred calling. But while the obligation appears to be acknowledged by many, few writers possess the gifts necessary to discourse well on subjects bearing in this direction. To those thinking parents and teachers who know that in their dealings with the infant and youthful mind they are laying the seeds of eternity, who feel that the spirit they cherish and the talents they foster can only achieve their full and best destiny by the harmonious development of the physical, the moral, and intellectual powers, we would recommend such a book as Mr. Carter's. We have perused his work with an interest that increased as we advanced, and it is but justice to the author to state that the intricate and often perilous ground upon which he treads, is passed over with a security and a knowledge of the regions he is traversing, which assures and convinces those who accompany him. While we give our cordial commendation both to the spirit which pervades the book and the general mode of execution, we decline adopting all our author's views. We assent to the general line of his argument, and cannot but admire the elegance of his language and the correct choice of his illustrations. At the same time, a condensation of some of the chapters and a reconstruction of others will, we feel assured, be desired by the author himself, on his return from the perils of war, to which, as we are informed in the preface, he was called, almost before the completion of his self-imposed task.

ART. II.—*On the Pathology of Hooping-cough.* By GRAILY HEWITT, M.B., Lond., Lecturer on Comparative Anatomy and Zoology at St. Mary's Hospital Medical School, and Surgical Registrar to the Hospital. Read before the Harveian Society of London, May 3rd, 1855.—*London,* 1855. pp. 27.

Morbid anatomists have hitherto failed in demonstrating the presence of an uniform and visible lesion in subjects who have died of hooping-cough. This may be due to various circumstances. The disease may not be of a character to prove fatal without the supervention of other diseases, which may then mask the original derangement; the disease may affect tissues in which we have not hitherto sought the *causa proxima* of the morbid action; the disease may produce lesions which our previous knowledge or mode of investigation does not enable us to estimate. Whether such or other causes have been at work, the fact remains, that while theories innumerable have risen to the surface of medical literature in relation to this topic, neither in the domain of pathology nor therapeutics do we find anything very positive regarding it. We must therefore gratefully receive any contribution to our knowledge on so vague a subject, and it is the more acceptable when it comes in the definite and concise form presented in the memoir of Dr. Graily Hewitt. The author fully appreciates the true relation of post-mortem and vital phenomena:

> "Simple cases of hooping-cough," he observes, "are rarely fatal; the complications and consequences of the disease, then, can only be expected to be found after death, and it is to the study of these complications and consequences that pathological investigations must be necessarily directed." (p. 4.)

An epidemic of pertussis gave Dr. Hewitt the opportunity of examining a series of fatal cases in the infirmary and workhouse of St. Marylebone. The results of nineteen observations, in children varying from one month to four years, showed the chief lesion found after death to be collapse of the lung substance, a condition also known under other names —as fœtal condition, carnification, atelectasis, and the like. The author reports the degree of this lesion as follows:

> "On the *right lung* portions of the upper lobe were found collapsed in six cases, and in four more to a less degree. The middle lobe was collapsed wholly or in part in sixteen cases. The lower lobe was more or less affected with collapse in eighteen cases. In the *left lung,* the upper lobe presented the same lesion in fifteen cases, the whole of the anterior tongue-like prolongation being, in most of the cases, affected. The lower lobe was collapsed more or less in eighteen cases.

In seven of the cases the portions collapsed were also congested, in some to a high degree." p. 10.

The presence of the collapse was determined by direct experiment, the test employed being that suggested by MM. Bailly and Legendre, of inflating the lung, the effect of which process, in a simply collapsed lung or portion of lung, is to produce uniform distension; whereas if a portion be the seat of actual pneumonia, no such uniform distension would take place. In all the cases but one, in which the lungs were extensively tuberculated, this test exhibited a greater or less extent of lung affected with collapse. Dr. Hewitt describes the appearance of the collapsed portions as follows:

"They were abruptly separated from the adjoining healthy lobules; depressed below the general surface of the lung; less bulky than the unaffected portions. The colour varied from a reddish violet to a deep purple; the firmness was variable, in most cases, however, having a great resemblance to that of a piece of flesh; non-crepitant; sinking immediately in water; lobular-cellular interspaces well marked; no air-cells visible in the surface; slightly friable in some cases, and emitting on squeezing a small quantity of non-aërated puriform fluid. The lung substance did not break down under pressure, as is seen in hepatization. When a blowpipe was introduced into the bronchus leading to the affected portions, and inflation performed, the aspect of the collapsed portions underwent a striking change. They immediately assumed the appearance of the adjacent healthy lobules, and were in nowise to be distinguished from them, becoming enlarged, and the air-cells on the surface easily distinguishable by the aid of a lens. The colour was changed from a dark violet to a light pinkish hue, such as is habitually seen in the healthy lungs of children. The lung substance was found then to float readily on water, and to have become crepitant. When these inflated portions were left to themselves for a short time, they became to a certain degree collapsed, the lung contracting and expelling a portion of the air artificially introduced. The inflation was performed with ease in most of the cases; in some, however, the force necessary to be used was more considerable, and some portions were not inflated at all by the additional force used. The portions which occasionally resisted full inflation were the posterior surfaces of the lower lobes. The depth to which the lung substance was implicated was variable. In all cases the collapse exhibited a preference for the portions of the lobes most distant from the roots of the lung; thus the margins of the lobes were found chiefly affected. A great part of a whole lobe was, in many cases, collapsed deeply as well as superficially; the upper lobes, however, were never found very deeply affected. The anterior tongue-like prolongations of the two upper lobes were, in nearly all the cases, collapsed, and were thin, pliable, and *lobulated* to the feel, if I may be allowed the use of such a term. The external surface of the upper lobes often presented little digital pits or depressions, the depressed surface being of a colour approximating to violet, and constituted by lobules in a semi-collapsed state. Inflation quickly gave the lobe a uniform smooth appearance." (p. 11.)

A tenacious muco-purulent fluid was found in the bronchi of almost all the cases, and it was observed that the air-cells of the lobules adjoining the collapsed portions were slightly but visibly enlarged. True inflammation of the lung was observed in four cases only, and but in one to any extent. The larynx and trachea were found normal, except that they generally contained a quantity of puriform fluid. The emphysematous distension of the air-vesicles adjoining the collapsed portions of lungs is a significant fact, which we think affords indirect evidence of the correctness of Dr. Hewitt's explanation of the mode in which the collapse is brought about. For this, as well as for other inferences and deductions given by the author, we must refer to the book itself; not, however, without expressing a hope that we may meet Dr. Hewitt again in the same field of pathological inquiry.

ART. III.—*Transactions of the Pathological Society of London.* Vol. VI. *Including the Report of the Proceedings for the Session* 1854—1855. pp. 404.

FOR some years past, the increasing zeal and assiduity of the members of the Pathological Society have enabled the secretaries to prepare a volume which has each year acquired a deservedly increasing reputation. There can be no doubt that the labour bestowed upon the preparation and arrangement of the Report—which those only can fully appreciate who have undertaken similar work—has reacted most beneficially upon the Society, and has stimulated the members to select their cases, and give the descriptions and histories of them, with more care. The present volume is a rich mine of pathological wealth, which shows the value of the co-operation already effected by the agency of the Society, while it promises an enlargement of the sphere of research to an extent to be achieved only by an union of so much scientific zeal as the Pathological Society manifestly embraces. The present volume contains much that is of extreme interest, and presents a storehouse which, like the former Reports, will be constantly referred to by the student of pathological anatomy. Numerous admirable and well-digested reports on specimens exhibited before the Society, and officially submitted to members for examination, are scattered through the volume, which may serve the student both as a guide in his studies, and as a model for his own inquiries. The illustrations are effective and well brought out: their value is enhanced by the fact that, with scarcely an exception, the original drawings are executed by the observers themselves.

As it has been stated publicly at the opening meeting of the Society, that the chief share of the labour in preparing the volume has devolved upon one of the honorary secretaries, Dr. Quain, we violate no confidence if we acknowledge the success with which he has executed his task, and may be permitted to express a hope that his services will be secured to the Pathological Society for a long time to come.

ART. IV.—*On the Sanitary Applications of Charcoal, and on Ventilation.* By J. FORBES WATSON, A.M., M.D., Bombay Army.—*London,* 1855. (Reprinted from the 'Journal of the Society of Arts.') pp. 14.

The Medicinal and Economic Properties of Vegetable Charcoal, with Practical Remarks on its Use in Chronic Affections of the Stomach and Bowels. By JAMES BIRD, M.R.C.S., late Surgeon of the Royal Glamorgan Militia.—*London,* 1855. pp. 82.

WE have recently drawn attention* to the very important bearing that vegetable charcoal has upon sanitary questions, owing to the oxidizing property it possesses, in addition to an extraordinary power of absorbing gases. Dr. Forbes Watson enlarges upon the investigations and discoveries of Dr. Stenhouse, and explains the practical appliances by which the atmosphere may be purified that enters our dwelling apartments, or by which noxious vapours may be neutralized in their very passage to our nostrils. His observations on the employment of charcoal are accompanied by some valuable remarks and suggestions on ventilation. By the conjoined persevering efforts of physicians, chemists, and engineers, we may yet hope to overcome the difficulties that beset this question. We are unable to go more fully into the subject of Dr. Watson's paper at present; we cannot, however, part from him without expressing a hope that he will prosecute this important matter, and also that we may have more details of the microscopic changes which he has detected in the blood during the prevalence of the monsoon in India, and which he speaks of, incidentally, as a fatty degeneration of the blood-corpuscles.

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* Medico-Chirurgical Review, July, 1855, p. 151.

Mr. Bird's book treats of the internal administration of charcoal as a remedy for dyspepsia of a neuralgic character, flatulency, and dysenteric states. The *pièce de résistance* is a report of a committee of the Académie de Médecine on a memoir of a French army surgeon (Dr. Belloc), in which that gentleman describes the benefit he had himself derived from the use of powdered charcoal, in doses increased from a few grains to 500 grammes (about sixteen ounces) per day, after having previously exhausted all other means of treatment. Dr. Belloc gives several other cures effected by the same agent. The members of the committee, Récamier, Caventou, and Patissier, expressed themselves strongly in favour of the remedy. Mr. Bird quotes the experience of Dr. Borland, Dr. Robert Jackson, and Mr. Ranald Martin in testimony of the value of charcoal in the treatment of fevers, dysentery, and stomach and bowel complaints generally, and concludes his observations by a brief account of the sanitary uses to which charcoal may be applied.

From the results of our own observation of the use of powdered charcoal in flatulent dyspepsia, we should be inclined to favour its exhibition as an internal remedy, while its sanitary applications can scarcely be urged too strongly.

Art. V. — 1. *Bildliche Darstellung der Krankheiten des menschlichen Auges: Erste und Zweite Lieferung; Physikalische Untersuchung des Auges.* Von Dr. C. G. T. Ruete.—*Leipzig*, 1854. Folio, pp. 63, 7 coloured plates.

Pictorial Illustrations of the Diseases of the Human Eye: First and Second Parts; Physical Examination of the Eye. By Dr. C. G. T. Ruete.

2. *Beiträge zur Pathologie des Auges.* Von Dr. Eduard Jaeger.—*Wien*, 1855. Folio, pp. 23, 8 coloured plates.

Contributions to the Pathology of the Eye. By Dr. Eduard Jaeger.

Want of space at present compels us to give our readers but a very brief notice of these important works. We have placed them together because, in the fasciculi hitherto published, both authors treat of the same subject, namely, the morbid changes occurring in the deep-seated tissues of the eyeball, as examined by means of the ophthalmoscope. In future numbers they purpose completing a series of all the more remarkable characteristics of ophthalmic disease.

The two publications are got up with equal care, and, as regards beauty of typography, leave nothing to be desired. Dr. Jaeger's figures are executed in chromo-lithography, but are nevertheless, in point of artistic effect, superior to the more highly-finished, but hard, figures of Professor Ruete, which are engraved in line and stipple, and coloured by hand. In the former work, each figure represents, on a black ground, a disk of illuminated retina, such as really meets the eye of an observer. Ruete's artist, on the contrary, combines two effects, which it is evident could never co-exist, and, while exhibiting the interior of the globe as seen with the ophthalmoscope in a darkened room, depicts in lively colours the iris, conjunctiva, lids, &c., as they would appear under ordinary daylight. This imparts an air of untruthfulness to drawings which, no doubt, were conscientiously copied from nature. In his preface, the Professor patriotically appeals to his plates as proofs "that the Germans are not artistically behind the English and French." But, proudly as Germany stands in the region of high art, she has still much to do in the humble domain of pathological illustration ere she can successfully rival the works of Dalrymple and Sichel. Several introductory pages are devoted to a description of various forms of the ophthalmoscope, as invented by Ruete himself, by Helmholtz, Coccius, Jaeger, Meyerstein, and others; and the principles and mode of employing these instruments are explained with great clearness and precision. In Dr. Jaeger's 'Beiträge' all this explanatory matter is omitted, but an octavo pamphlet,* circulated with the larger work, supplies the deficiency. This pamphlet, which contains the substance of a paper read before the Imperial Academy of Science at Vienna, gives a careful description of the appearances of the healthy retina; and especial notice is directed to the peculiarities presented by the entrance of the optic nerve and the "macula lutea." Some of the explanations of the morbid changes in the choroid appear to us rather arbitrary, and requiring for their verification the precise test of post-mortem dissection. One fault also strikes us as common to all the well-executed figures of the retina—namely, an exaggerated contrast between its arteries and veins. Such a marked difference we have never seen in nature, and however allowable in a mere diagram, it ought not to be represented in drawings which profess to be actual transcripts from the living eye.

We shall anxiously look for the continuation of both these illustrated works, and shall communicate to our readers any interesting facts they may disclose in a field so new and so important as that revealed by the ophthalmoscope.

ART. VI.—*Observations on the Term of Utero-Gestation.* By CHARLES CLAY, M.D. Pamphlet, 8vo, pp. 24.—*London*, 1855.

IN a medico-legal point of view, it is of the greatest importance that we should attain to some degree of accuracy in our knowledge or opinions regarding the period of human utero-gestation. Dr. Clay considers that he has arrived at some definite conclusions as to the laws by which this period is governed. The first of these is, that the period of gestation is entirely regulated by the ages of the individuals concerned in the act; the second, that the younger the parties concerned, the shorter the period of utero-gestation, and *vice versâ.* These statements are supported by cases, and by observations upon various species of lower animals. Dr. Clay adds—

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* Ergebnisse der Untersuchung des menschlichen Auges mit dem Augenspiegel.

> "I boldly deny that the gestative period ever did extend to months, or even weeks, beyond the term natural to the age of the parents, where the fœtus and the mother are normal in all their bearings. Indeed, I very much question if it could be extended to more than a few hours beyond the point fixed by the ages."

We fully concur with Dr. Clay, that the only cases upon which any inference as to the gestative period can be deduced, are those in which pregnancy follows a single *coitus.* Fifty-one instances of this kind, collected by Dr. Clay, confirm his views. The fallacies, both moral and physical, which the histories of these cases may reveal, are carefully pointed out by the author. Dr. Clay has undoubtedly pointed out the direction in which lies the solution of this problem, and the prospect of unanimity upon a question hitherto so much disputed.

Art. VII.—*The Correlation of Physical Forces.* By W. R. Grove, Q.C., M.A., F.R.S., Corr. Member of the Academies of Rome, Turin, &c. Third edition.—*London,* 1855. pp. 229.

The pamphlet which Mr. Grove published in 1846, 'On the Correlation of Physical Forces,' is now enlarged to the dimensions of a goodly book; but though the arguments and illustrations are more copious, the train of reasoning is the same in both. As a model of clear and logical induction, of pure and vigorous writing, as well as on account of the wide range of thought and profound appreciation of the limits of human investigation displayed in it, we would urge the study of the 'Correlation of Physical Forces' more particularly upon our professional brethren.

Art. VIII.—*Outlines of Military Surgery.* By Sir George Ballingall, M.D., F.R.S.E., Surgeon to the Queen and to H.R.H. the Duchess of Kent; Regius Professor of Surgery in the University, Fellow of the Royal College of Surgeons, Consulting Surgeon to the Royal Infirmary and to the Lock Hospital of Edinburgh. Fifth edition, illustrated with woodcuts.—*Edinburgh,* 1855. pp. 634.

The important lessons taught us by the present war have not been overlooked by Sir George Ballingall in the most recent edition of his classical work. In noticing a former edition, we expressed a desire to see the portions embracing 'Military Economics' enlarged. We perceive that this has been done; but still we think that even more prominence should be given to the hygiene of armies, if the army surgeon is indeed to accomplish his high destiny. There can be no inherent necessity for the military hospitals being crowded as we have seen them, or of our armies losing many more soldiers by disease than by the enemy. If Chemistry and Physics can enable us to commit more havoc among the hostile ranks than was possible formerly, surely the advance of medical knowledge ought to enable us to save more of our own troops. This part of political economy is yet fearfully in the background. In again recommending Sir George's work, we must notice the very useful addition of well-executed illustrations of ambulances and other objects of special interest to the military surgeon.

ART. IX.—*Chloroform: How shall we ensure Safety in its Administration.* By PATRICK BLACK, M.D., F.R.C.P., Assistant Physician to, and Lecturer on Medical Jurisprudence at, St. Bartholomew's Hospital.—*London*, 1855. pp. 40.

DR. BLACK investigates the grounds upon which, in cases of death resulting during the administration of chloroform, the issue is attributed to paralysis of the heart. The conclusion to which his inquiry leads him, is that death is produced by asphyxia; or, to use his own words, that chloroform proves fatal "by its influence in restraining the respiratory movements at the earliest periods of its being administered, when its pungency would suddenly arrest its inhalation." Dr. Black analyses the symptoms accompanying some of the cases on record, and he certainly appears to establish his view—the practical corollary being that, in administering chloroform,

> "Our attention must be wholly given to the breathing, from the observation of which we must not allow ourselves to be diverted for a moment. If the patient breathes easily, he is in safety; if his breathing be attended by frequent coughing —and still more, if it appear to be restrained, with deepening turgescence of the head and face, we have before us the distinct warnings of danger; and unless we give immediate heed to them, they will be speedily realized."

ART. X.—1. *On the Effects of the Thermal Waters of Ems.* By LOUIS SPENGLER, M.D., Physician for the Baths of Ems, &c.—*London*, 1854. pp. 103.

2. *L'eau Amère de Friedrich's Hall.* Par le Dr. Eisenmann.—*Wurzbourg*, 1855. pp. 32.

The Bitter Waters of Friedrich's Hall. By Dr. Eisenmann.

THESE two essays are written with the intention of establishing upon a physiological and pathological basis the indications for the employment of the respective Brunnens.

The Ems waters are essentially alkaline, and Dr. Spengler argues that the cures which are undoubtedly effected at Ems, are due to the solvent powers of the chief constituent—the bicarbonate of soda, of which the four chief springs contain on an average above fifteen grains to the pound avoirdupois. Chronic inflammatory affections of the mucous membranes, and their results, are the class of cases especially and almost exclusively benefited by the waters; and to these their application ought therefore to be restricted.

Dr. Eisenmann's essay is intended to bring more into notice the powerful saline waters of Friedrich's-hall, a small watering-place in the Grand Duchy of Saxe Meiningen, which is less in the beaten track of English tourists than Ems. The quantity of salts contained in this water amounts, according to Liebig's analysis, to above 194 grains in 16 ounces; of these the proportions of sulphate of soda were 46·5 grains, of sulphate of magnesia 39·5 grains, and of chloride of sodium 61·1 grains. That such water should act according to the quantity taken, more or less powerfully, upon the gastro-intestinal or urinary passages, is in accordance with our ordinary experience.

It is a curious circumstance, that two German physicians of eminence should think it desirable to publish their works in foreign languages. We trust that it is not owing to a disregard for the power and beauty of the language of Fichte and Schiller.

Art. XI.—*On the Nature, Treatment, and Prevention of Pulmonary Consumption, and incidentally of Scrofula; with a Demonstration of the Cause of the Disease.* By Henry M'Cormac, M.D.—*London*, 1855. pp. 112.

The importance of pure air in reference to the prevention and cure of disease can scarcely be over-estimated; but we must not forget that, although the great object of most sanitary enactments is to secure an unpolluted atmosphere in and external to our houses, the thing itself still remains a desideratum. Under all circumstances, the variations of temperature, the watery and gaseous contents of the atmosphere, are elements that also deserve consideration. This Dr. M'Cormac appears to overlook entirely; and although we admire the extensive research which he has evidently bestowed on the subject to which he particularly directs our attention,—the influence of vitiated air in causing tubercular disease in its various forms,—we must warn our readers against being carried away by the enthusiastic advocacy with which he pleads his cause. He cuts the Gordian knot of all the difficulties which oppose themselves to the inquiry after the best means of anticipating tuberculous disease thus:—

> "The constant formula to be used by rich and poor is to sleep with the upper portions of their windows largely open by night, to wash in cold or tepid water every morning on rising from head to foot, to go much into the open air, and to take or send their children out." p. 52.

With a demurrer against the first part of this formula, we can recommend the book as one containing much that is useful and practical.

Art. XII.—*Medical Anatomy.* By Francis Sibson, M.D., F.R.S., Physician to St. Mary's Hospital. Fasciculi II. and III.

The present numbers of Dr. Sibson's 'Medical Anatomy' in every way justify the encomiums which we had occasion to bestow upon the first fasciculus. The drawings are executed with care and elegance by Mr. Fairland, and the explanatory letter-press contains much important and very valuable matter in addition to the detailed exposition of the different viscera. The chief subjects of the second fasciculus are, the pericardium with the heart and great vessels, in their normal and abnormal relations. The effect of diseases of the heart and great vessels, of other thoracic organs and of the abdominal viscera, in determining the size and position of the heart, are carefully reviewed. The third fasciculus treats mainly of the organs of respiration; the larynx, trachea, bronchi, and lungs, with the ribs and diaphragm.

In the diagnosis of morbid conditions of internal organs, an intimate knowledge of their relative position is one of the most important elements. We have ourselves frequently wished to refer to a work like that now offered to the profession by Dr. Sibson, and are satisfied that a similar want is very generally felt by medical men. We have no hesitation in saying, that the faithful transcript of nature which Dr. Sibson now offers to us, will be welcomed by all earnest students as a most material aid in the diagnosis of disease.

We shall return more fully to the consideration of Dr. Sibson's 'Medical Anatomy' on a future occasion.

ART. XIII.—1. *A Lecture delivered at the Opening of the Medical and Surgical College of St. Thomas's Hospital, for the Session* 1855-56, *Oct.* 1, 1855. By THOMAS B. PEACOCK, M.D., Fellow of the Royal College of Physicians, Assistant-Physician to, and Lecturer on Materia Medica at, St. Thomas's Hospital; Physician to the Hospital for Diseases of the Chest, Victoria Park.—*London*, 1855. pp. 18.

2. *An Introductory Lecture at the Opening of the Session of the Chatham Street School of Medicine, Manchester, Oct.* 1, 1855. By DANIEL STONE, F.C.S., Lecturer on Chemistry at the Chatham Street School of Medicine.—*Manchester*, 1855.

3. *The Present State of the Theory and Practice of Medicine. An Introductory Lecture to the Class of Institutes of Medicine in the University of Edinburgh.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Institutes of Medicine and of Clinical Medicine in the University.—*Edinburgh*, 1855. pp. 23.

THE spirit that pervades the lectures, the titles of which we have placed at the head of this notice, is one that cannot fail to influence beneficially the pupils to whom they were addressed. The lecturers dwell upon the sources of intellectual gratification which the study and pursuit of the medical profession holds out to its zealous cultivator, and show what encouragement is to be found in the large strides which medicine and the allied sciences have made within the memory of man.

To those who are inclined to scepticism on this point, we would especially recommend a perusal of Dr. Bennett's lecture. We entirely concur in the sentiment expressed in his closing words:—"Everything promises that before long a law of true harmony will be formed out of the discordant materials which surround us; and if *we*, your predecessors, have failed, to *you*, I trust, will belong the honour of building up a system of medicine, which from its consistency, simplicity, and truth, may at the same time attract the confidence of the public and command the respect of the scientific world."

PART THIRD.

Original Communications.

Art. I.

On Ulcer of the Stomach. By **William Brinton**, M.D., Fellow of the Royal College of Physicians, Lecturer on Physiology at St. Thomas's Hospital, Physician to the Royal Free Hospital.

In the following article, I propose to lay before the readers of this review a few considerations respecting the morbid anatomy of the ulcer of the stomach.

For some years past, I have made the diseases of this organ an object of special clinical investigation, and have found only too ample materials for such an inquiry in the practice of the hospital to which I am at present attached as physician.

But although, as regards the study of the symptoms and treatment of these maladies, and their ordinary appearances after death, my opportunities have left me little to desire, there are many of their pathological details with respect to which I have constantly felt the want of a far wider field of observation than my own experience could afford. And, as one means of satisfying this want, I have undertaken a troublesome search through various British and foreign journals and reports, which seemed to promise the kind of information I desired. The peculiar interest with which I have long regarded the ulcer of the stomach has especially led me to adopt a similar course respecting it. I have thus been enabled to add, to the various necropsies of this lesion which my own practice has afforded me, the results of about one thousand more, the majority of which have never before been collected together, much less compared with each other.

The comparison of such a large number of cases seems to afford some inductions of great clinical interest. Of these inductions, however, the limits of this article forbid my giving any complete analysis. I shall be satisfied if the following pages furnish the reader with an outline of the information afforded by the careful examination of the gastric ulcer in the dead body; and shall postpone to a future opportunity all attempts at the practical application of that information, either as an explanation of the symptoms of the disease, or as a clue to its rational and successful treatment.

The *frequency* of ulcer of the stomach may be best inferred from the number of times that this lesion has been observed in a given number of persons, dying from all diseases, and subjected to careful necropsy.

The few data of this kind that I have been able to meet with are the following:

Dr. T. K. Chambers[*] states that, in 2265 post-mortem examinations made at St. George's and St. Mary's Hospitals, ulcer of the stomach was present 22 times. But as cicatrices of ulcers are not mentioned in his valuable memoir, it may be a question whether we ought not to add to this number, what we shall by and by find reason to conjecture is the average relative number of scars—viz., an equal proportion, or 22. Even with this conjectural addition, however, the frequency of ulcer would be only 44 in 2265, or less than 2 per cent. of the total deaths.

The writings of Dr. Gairdner,[†] Dr. Habershon,[‡] and Dr. Handfield Jones,[§] have afforded me some results which may be conveniently grouped together in the statement, that they observed 11 open ulcers, and 4 cicatrices, in all 15 ulcers, in 435 autopsies; a proportion that corresponds to 3½ per cent.

Jaksch[‖] found that 2330 autopsies afforded 113 ulcers; of which 57 were open sores, 56 cicatrices. This is a proportion of somewhat less than 5 per cent., or 1 in 20 dead bodies.

Dittrich[¶] gives the results of the examinations after death of 396 adults; in whom he found 25 ulcers, 5 open, 20 cicatrized. This affords a total proportion of about 6 per cent., or 1 in 16.

Willigk[**] examined 1600 bodies, and found 139 ulcers, 74 open, 65 cicatrized. This number corresponds to 8½ per cent., or 1 in 12.

Lastly, Dahlerup[††] found 26 ulcers in 200 corpses; 20 of these 26 being open, 6 cicatrized. This affords the proportion of 13 per cent. to the total deaths, or 1 in 8.

As regards any general comparison of the above proportions, it is to be regretted the brief accounts from which I am obliged to quote, do not specifically state that the ulcers and scars above mentioned always occupied the stomachs of different individuals. But they seem to imply this fact. While the presence of an ulcer and a scar in the same stomach must be not only an infrequent coincidence, but a possibility which, in the small number of cases adduced by Dahlerup and Dittrich, might be almost dismissed from notice.

Assuming the accuracy of the above statements, we may sum up their more important results in the following propositions.—1. The ulcer of the stomach is so far from being a rare lesion, that evidence of its present or previous existence may be found in from 2 to 13 per cent. of persons dying from all causes; and that the ulcer itself, open and unhealed, may be observed in from 1 to 10 per cent. 2. The 7226 necropsies thus collected offer us about 360 ulcers, which are pretty equally divided into

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[*] London Journal of Medicine, vol. iv. p. 597. 1852.

[†] Private communication.

[‡] Medical Gazette, vol. xxx. p. 612.

[§] Transactions of the Medico-Chirurgical Society for the year 1854.

[‖] Schmidt's Jahrbuecher, vol. xliv. p. 300. 1844. (From the Prag. Vierteljahrschrift, i. 3.) The 75 hæmorrhagic erosions he mentions, I omit, as not trustworthy evidence of commencing ulcers.

[¶] Schmidt's Jahrbuecher, vol. iv. p. 302. 1854. (Abridged from Papellier's Inaugural Dissertation, Erlangen, 1854.)

[**] Schmidt's Jahrbuecher, vol. iii p. 92. 1853. (From the Prag. Vierteljahrschrift, x. 2. 1853.)

[††] Canstatt and Eisenmann's Journ. for 1842. (De ulcere ventriculi perforante, Havniæ, 1841.)

190 open ulcers, and about 170 scars. These numbers tolerably correspond to a total proportion of 5 per cent.; which is divisible into $2\frac{3}{5}$, and $2\frac{2}{5}$ per cent., for these two conditions respectively. 3. The above range of frequency is so remarkable as to suggest some special cause or causes. These, however, could only be determined by a careful analysis of the class, age, and sex, of the patients received into the hospitals in which these observations were made. Failing such an analysis, I will only point out, that the maximum frequency of the ulcer, as stated by Dahlerup, occurs in the spirit-drinking population of Copenhagen; and that its larger proportion in the German Krankenhaeuser may be plausibly referred to the inmates of these institutions being, on an average, of greater age, if not of more destitute circumstances, than the persons usually received into English hospitals.

The little I have been able to observe as to the frequency of the ulcer in the living subject, seems to confirm the experience of the British authorities mentioned above.

In order definitely to diagnose the existence of an ulcer of the stomach, I am in the habit of requiring the presence of a set of symptoms, the concurrence of which would certainly understate rather than exaggerate the frequency of the disorder. And yet I am disposed to think that at least 40 instances of this malady come under my notice yearly, in an out-patient practice which numbers about 4000 new cases within this space of time.

Sex.—As has long been believed, the ulcer is more frequent in the female than in the male. Among the autopsies I have collected, are 654 which mention the sex. Of these, 440 are female, and 214 male; numbers which nearly correspond to the proportion of 2 to 1.*

With respect to the ages at which the ulcer has been detected, I can only cite 226 necropsies that include perforations, open ulcers, and cicatrices in natural proportions to each other. The persons in whom they were found had an average age of $42\frac{1}{6}$ years.

These 226 cases may be arranged in decades of years as follows:

	Under the age of								
	10	20	30	40	50	60	70	80	90
Number of cases of gastric ulcer.	2	18	45	39	38	32	32	15	5
	65			77		84			

But in order to gather, from these numbers, the liability of living individuals of these several ages to become the subject of the ulcer, we must, of course, correct them by the comparative numbers of persons living in each corresponding decade. Such an arrangement (see next page) shows that the liability gradually rises, from what is almost a *zero* at the age of ten, to a high rate, which it maintains through the period of middle life; at the end of which period it again ascends, to reach its maximum at the extreme age of ninety. We may therefore conclude that the ulcer of the stomach is specially, though not exclusively, a disease of middle and advancing life.

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* It is only in the 74 ulcers mentioned by Willigk (*loc. cit.*) that I have been able to correct the number of the ulcers in each sex, proportionally to the deaths from all causes, by the numbers of male and female subjects examined. The result has been to lower his previous ratio of 12 to 62, to one of not more than 12 to 55.

This view is strongly confirmed by the following comparison of the liability to gastric ulcer with that of two other diseases of youth and age respectively—viz., consumption and apoplexy.[*]

	Under the age of								
	10	20	30	40	50	60	70	80	90
Liability to ulcer of stomach, taking 100 as maximum	0	20	51	49	47	56	80	75	100[†]
To pulmonary consumption, with the same maximum	47	33	73	92	100	95	65	23	7⅔
To apoplexy (cerebral hæmorrhage?) with the same maximum . . .	0	½	2⅓	7	16	40	61	100	69

The *situation* of the ulcer I deduce from 220 cases, that include scars, ulcers, and perforations indifferently, as observed in the successive necropsy of a large number of subjects. Of these 220 cases,[‡] in 86 the ulcer occupied the posterior surface of the stomach; in 55, its lesser curvature; in 32, its pyloric extremity; in 13, its anterior and its posterior surfaces, often at two opposite places; in 10, its anterior surface only; in 5, its greater curvature; in 4, its cardiac pouch[§]. But in comparing these numbers, we must recollect that the various regions of the stomach merge into each other by such inexact and even changeable boundaries, that minute accuracy in speaking of them is quite impossible. For example, in such a nomenclature as the above, the lesser curvature, which is, in strictness, not so much a surface as a line, defined by the attachment of the gastro-hepatic omentum, really includes a variable extent of the adjacent portions of the anterior and posterior surfaces. In any case, however, these numbers claim a marked preponderance of liability for the posterior surface, the lesser curvature, and the pyloric pouch, over the anterior surface, the greater curvature, and the cardiac sac respectively; the ulcers in the three first situations together making up 187, or nearly 85½ per cent. of the whole.

In *size*, the ulcer is rarely much smaller than a fourpenny-piece, or larger than a crown-piece. But no precise limits can be assigned it. Thus an ulcer not larger than a pea may exhibit all the characteristic appearances of this lesion, and may give rise to fatal hæmatemesis, or to perforation. While, conversely, an ulcer has often been known to attain a diameter of five or six inches; or in other words, a superficial extent amounting to ⅛th or ⅑th of the total mucous surface of the organ.

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[*] These round numbers I have reduced from the Registrar-General's Returns for 1847.

[†] Against such a marked and progressive rise of liability it militates little to point out, that so many of these cases died of intercurrent maladies, that the date of detecting the malady has no definite relation to that of the occurrence of the lesion. Unless it be contended that an ulcer in the stomach tends to increase longevity, by warding off other maladies, I can see no other conclusion than the above: namely, that advancing age heightens the chances of such a lesion. I might easily adduce various cases in support of this deduction, which is one of great importance to the pathology and treatment of the disease.

[‡] These 220 cases include 15 mentioned by Jaksch (*op. cit.*), in which the ulcer occupied both the posterior surface and lesser curvature. The latter I have excluded from the above numbers, but admitted into the 187 mentioned lower down.

[§] The respective per-centages of ulcers in these situations are so nearly obtained by halving the above figures, that I have not thought it necessary to mention them separately.

The *shape* of the ulcer is usually circular or slightly oval. But an equal variation obtains in this respect. Thus, it is often oblong, its direction being either parallel or transverse to the axis of the stomach; and in rare instances it has formed a zone around the pyloric valve, or the neighbouring extremity of the organ. But some of these irregular shapes are evidently due to the fusion of two or more ulcers into one, by the progressive enlargement of their adjacent margins.

As regards the *number* of ulcers, two or more are frequently present in the same stomach. Out of 536 cases which specify such details, a plurality of ulcers was present in 113; a number that corresponds to rather more than 1 in every 5 cases, or about 21 per cent. Of these 113, 97 (corresponding to 463 instances of ulcer) offered the following numbers: in 57, there were two ulcers; in 16, three; and of the remaining 24, in which "several" ulcers were present, 3 cases offered four, and 2 cases five ulcers each; while in 4 there are reasons to suppose even this number was exceeded.

Margin. The appearances of the tissues in and around the ulcer are subject to just as much variety as its size and shape. In some instances, there is little or no evidence of inflammation in the neighbourhood of the lesion; which consists of a mere removal of the mucous membrane over a circular space, that forms a shallow but level pit, with a sharp, smooth, vertical edge, as though it had been punched out. In other cases, which appear to form the majority, the mucous membrane that constitutes the immediate margin of the ulcer is somewhat swollen, so as to be raised a little above the level of the adjacent mucous surface. And a microscopic examination shows that this thickening, which is always accompanied by induration, depends upon an exsudation of lymph into the areolar tissue beneath the mucous membrane, as well as into the matrix of the latter texture itself. In short, there can be no reasonable doubt that we have here a slight but appreciable amount of inflammatory reaction; and that, in respect of its nature, this reaction is closely akin to that adhesive inflammation of the peritoneum or neighbouring viscera to which we shall presently allude.

In many instances, indeed, the swelling and induration around the ulcer far exceed that just mentioned; and convert the mucous membrane, for the distance of half an inch, an inch, or more, into a thick brawny mass, which has been sometimes mistaken for cancer. Rarely, however, will a careful examination of the ulcer leave us in any doubt as to its nature. Even when best marked, the total increase of thickness in the parietes of the organ is but moderate. The exsudation which causes this increase of thickness is almost exclusively confined to the mucous membrane, and to the areolar tissue immediately beneath it: and consists of fibres, in which it is usually very difficult to find even moderate quantities of the cell growth from which such fibres appear to be developed. Hence the new substance has neither the structure nor the situation of the cancerous deposit. The mucous membrane itself, however thickened, remains in what is essentially a healthy state. Indeed, in many such instances it is little more than hypertrophied, in the strictest sense of this term. And, lastly, the history of the lesion would generally afford sufficient grounds for a decision, even prior to an inspection of its appearances.

The latter allusion we may connect with what seems to be the most obvious explanation of that maximum, minimum, and medium of inflammatory reaction and thickening, which we have indicated in the above remarks. As one might expect, the simple, punched-out ulcer is usually either a small or recent lesion, on the one hand, or is associated with a weakly or cachectic (in the female, often a chlorotic) state of constitution, on the other. While the maximum of thickening is generally found in connexion with the same circumstances which favour the occurrence of adhesive inflammation on the exterior of the stomach:—and among these, especially with the size and previous duration of the ulcer. It is however curious to notice, how frequently it occurs in comparatively young subjects, many of the best instances recorded having been persons of about the age of twenty or twenty-five. Still this fact does not qualify the preceding statement as to the usual duration and diameter of the indurated ulcer; but seems merely to express the degree in which the inflammatory process is capable of being heightened by the vigour of youth.

Such varieties in the size, shape, and appearances of the lesion, added to what we have already noticed with respect to the number in which it is often present, may well show with what restrictions we ought to make use of the ordinary nomenclature by which it is known in medical treatises. It is usually called the "simple," or "chronic," or "perforating" "ulcer" of the stomach. Now, as regards the noun-substantive itself, an important exception may be taken to its use. For any comparison of a large number of specimens would conclusively show, that there is no specific or pathological distinction between "ulcer" and "ulceration" of the stomach; and that all the distinctive characters which the most minute description could assign to either, merge into those of the other by infinite gradations. It is true that the numerous or large ulcerations which are sometimes produced by a rapid process of destructive absorption, are pretty sure not to be accompanied by any of those appearances which imply even a moderate duration;—that they will not, for example, have raised edges or a hard margin, like most of the ulcers, and will rarely penetrate the larger vessels, or even the total parietes, of the stomach. But, practically speaking, all this only amounts to the statement of a very obvious fact—namely, that such lesions destroy so large a fraction of an organ essential to nutrition and life, that the unhappy subject of them generally dies before they have time to offer appearances of reaction, or is too prostrate to be amenable to the inflammatory process. While every one of the three adjectives applied to the ulcer might be fairly quoted as illustrating the principle of "*lucus à non lucendo.*" It is called *the* ulcer, because it is not essentially single either in its occurrence or (if we may so far anticipate) in its nature and origin. It is called the *simple* ulcer, because its characters are generally a compound of two processes of absorption and reaction, the latter of which certain instances show to be quite independent of the former. It is called the ***chronic*** ulcer, because its progress is sometimes so rapid as to penetrate the stomach and destroy life in a few days. And, finally, it is called the *perforating* ulcer because, in seven out of every eight cases, it does ***not*** perforate.

The *base* of the ulcer, so long as it is formed by the tissues of the stomach itself, presents appearances similar to those of its margin. Its usually firm and hard consistence is derived, partly from the density of the areolar and muscular tissues originally present, partly from an increase of their cohesion, due to that infiltration of fluid or exsudation of lymph, which inflammation generally brings about. In other (and by no means infrequent) cases, the progress of ulceration, apart from any such reaction, is betrayed by the soft, flocculent, or even gelatinous consistence of the floor of the ulcer; where we sometimes find flakes of dead tissue, the size of which almost entitles us to regard them as sloughs.

But since the ulcer, beginning in the mucous membrane, gradually extends in the direction of depth, as well as of surface, through the coats of the stomach, the nature of its base and margin necessarily undergoes constant changes. Still, the mode by which it penetrates the various tissues of the stomach is so characteristic, that there is little alteration in the shape of the ulcer, so long as it does not pass beyond them.

The whole depth of the ulcer forms a cone, the base of which is at the free internal surface of the stomach, while its apex points towards the peritoneum. The smooth, sharp, vertical edge which forms the lateral boundary of the ulcer as it passes through the mucous membrane, is exchanged for a smaller and less regular circle where it reaches the submucous areolar tissue. In like manner, the gradually narrowing aperture by which the ulcer eats its way through the subjacent muscular coat, dwindles, as it reaches the peritoneum, to what is little more than a point, corresponding to the centre of the conical ulcer. And it is in this point that the perforation which forms the last event of simple gastric ulceration occurs; generally as the immediate result of the rupture or detachment of the pale yellow slough, into which the peritoneum has previously been converted, allowing the contents of the stomach to escape into the abdominal cavity.

It is obvious that a progressive increase in the depth of a gastric ulcer would always end in the perforation of the stomach. But this event is in most instances prevented or deferred by the occurrence of adhesion, which, by uniting this organ to some neighbouring surface, obliterates the peritoneal cavity at and around the base of the ulcer. The peritoneum covering the affected part of the stomach undergoes inflammation; its smooth serous surface acquires a dull, roughened aspect, and becomes the seat of an exsudation of coagulable lymph, by means of which it is soon fixed and united to the adjacent serous surface of any viscus with which it may be in contact.

The frequency of such intimate adhesion must, of course, depend chiefly on the number of protracted cases met with; of which protraction we may regard it as equally cause and effect. My own observations only entitle me to corroborate the statement of Jaksch, who found 22 such adhesions in 57 cases of ulcer; a proportion of about 40 per cent. The site of these adhesions, and the viscus to which they attached and fixed the organ, exhibited a tolerable correspondence with those parts of the stomach which we have already specified as the most frequent situations of the ulcer. Thus of these 22, 15 united the pancreas to the posterior surface, or lesser curvature, of the organ; 5 attached the pylorus or lesser curvature to the adjacent liver; 1 involved the mesentery; and 1 the spleen. But there can be little doubt that the formation of these adhesions is seriously affected by another cause :—namely, by the movement of the stomach upon the surface opposed to it. It is only thus we can explain the rarity of adhesion of the anterior wall of the stomach to the parietes of the belly, coupled, as we have already noticed, with a by no means infrequent position of the ulcer on this wall, and with what we shall see is an extreme liability to perforation when so placed. And it is interesting to notice, that the situation of the ulcer seems not only to regulate the occurrence of adhesion, but also to affect its structure, and thus to influence its efficiency as a means of protection against perforation. The adhesions which occupy the omentum are often little more than a thickening of the delicate fibrous tissue of the peritoneum by an interstitial deposit of inflammatory lymph; and are of so little avail in warding off perforation, as to be ruptured by very slight exertions or shocks, such as coughing or sneezing with a moderately distended stomach. While the adhesions which unite the stomach with the liver or pancreas often possess a fibro-cartilaginous character, that almost precludes all danger of perforation.

The duration of the disease is very variable, and is in most instances rather to be deduced from the symptoms observed during life, than from the appearances found after death. The latter would, however, generally permit a conjecture. Thus, when we find a large, shallow ulceration, of irregular shape, unattended by any marks of adhesion on its peritoneal aspect, or by any elevation or thickening of its mucous edge, there is fair ground for presuming it of recent formation. While, conversely, adhesion and thickening around an ulcer, or an exactly circular shape, tend to show that a certain time has elapsed since the first occurrence of the destructive process. The clue sometimes afforded by the symptoms can hardly be alluded to here, save to point out that there is great danger of assigning to an ulcer far too long a duration, in consequence of the liability of the malady to a return. In fact, nothing short of a tolerably complete continuity of the symptoms during a series of years, entitles us to regard an ulcer as really open during the whole of the time. In like manner, unless the symptoms of ulceration during life were very marked and persistent, we should hardly be justified in denying that the ulcer before us might not have existed before the visible attack of illness that first called for medical advice.

Even with these limitations, however, the range of duration is remarkable. In what are certainly exceptional cases, the ulcer has been known to be fatal in as little as ten days: generally by perforation; sometimes by exhaustion, caused or hastened by vomiting; very rarely by hæmorrhage. But, in the majority of instances, a period of several weeks or months precedes the fatal event. And an extension of this period to years seems not by any means uncommon. Among cases of this kind—possibly relapses, but more probably continuous open ulcerations—I find in my notes one of 35 years, two of 30 years, three or four of 20, four or five of 15, and several of 10, 7, 5, and 4 years' duration.

The healing of such ulcers by a process of cicatrization appears to be much more frequent than is generally supposed. The examinations of Dittrich, Jaksch, Willigk, and Dahlerup, reveal a total of 147 scars and 156 ulcers, making the proportion of the former nearly equal to that of the latter. Against such results it can hardly be alleged that the supposed scars have really been mere local hypertrophies or thickenings of the mucous membrane, or fibrous deposits in its sub-mucous areolar tissue. While in their favour we may point out how easily cicatrices of small size might escape discovery, in less careful scrutinies of the mucous membrane of the stomach than those which appear to have been made by these observers.

The cicatrix by which the ulcer heals is therefore, on the whole, about as frequently met with as the ulcer itself. In other words, half the instances of this disease undergo what is probably a spontaneous cure. The precise details of the process of cicatrization differ with the amount of destruction that has preceded it. Where the ulcerative process has not extended deeper than the mucous membrane, the scar is sometimes little more than a mere condensation and thickening of the sub-mucous areolar tissue; and in shape (like the typhoid cicatrix) closely resembles the ulcer that preceded it. But in the majority of instances, it has a more characteristic shape. The gradual contraction of the lymph deposited at the base of the ulcer converts it into a hard, and often thick, central mass; which gives off cord-like processes, that seem to radiate into the surrounding healthy tissues. The latter are themselves thrown into folds, as the result of the tension which this contraction produces. Where the previous loss of substance has been considerable, this process often seriously affects the shape and capacity of the stomach. In such instances the cicatrix corresponds to a constriction of the organ, which gives it more or less of an hour-glass shape. And in extreme cases, the contraction amounts to an absolute stricture, which impedes the transit of food, and thus gradually causes dilatation and hypertrophy of the over-distended cardiac segment of the stomach. Such examples are however rare.[*]

The cicatrices which thus affect the calibre and shape of the stomach are generally those of large ulcers, that have remained open for a long period before undergoing the healing process. Hence they are usually found associated with adhesion of the stomach to some of the neighbouring organs:—a circumstance which itself often aggravates the perils of the gastric constriction, by still further embarrassing the muscular contractions of the stomach, and aiding its changes of shape. In most of these cases, the substance of the adhesion is so inseparably united with that of the cicatrix itself, that it is impossible to distinguish one from the other. Both are indeed composed of the same substance:—a fibrous structure, the elements of which gradually approach, but never fully attain, the development of the ordinary white fibrous tissue. There are three or four very interesting cases[†] on record, in which the ulcer has given rise to a peculiar dilatation and thickening of the pyloric end of the stomach, so as to convert this part into a kind of pouch, which could be distinguished through the anterior wall of the belly during life. The details of these cases scarcely render them susceptible of a common description, far less of a single explanation, but it seems not improbable that in all of them the accumulation of the gastric contents, which formed the immediate cause of the dilatation and thickening of the coats of the stomach, was due to a local failure of muscular contraction, itself the result of that destruction of tissue which the ravages of the ulcer had brought about. Any fuller consideration of their origin would lead us too far from our present subject, from which we must exclude these cases of "ampliation" of the stomach, even though they are caused by gastric ulcer.

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[*] From the cases I have been enabled to collect, I should conjecture them to be scarcely one in 200 instances of ulcer; or one per cent. of the cicatrices by which they are conditionated.

[†] Cruveilhier (whose description of the gastric ulcer, twenty years ago, still forms the most valuable part of our knowledge respecting it) gives two instances of this kind. Another will be found (exceedingly well reported) in the Dublin Medical Journal (vol. ii. p. 494).

There are other complications of adhesion and cicatrization which we may dismiss with a very brief notice. In some instances the surface of a broad ulcer becomes completely skinned over, while its firm and extensive adhesion to the neighbouring wall of the belly seems to prevent the complete contraction of the cicatrix. Here (just as in adherent wounds of the stomach, attended with much destruction of its walls) the mucous membrane around the margin of the depression or fossa formed by the cicatrix, becomes prolapsed and protruded into it, and is thus maintained in perpetual contact with the smooth base of the ulcer.* Where the adhesion is smaller and less substantial, it is sometimes drawn out by the constant traction the stomach exercises, so as to form a hollow and funnel-shaped tube, which is lined by the smooth quasi-serous surface of the cicatrix.

Perforation.—We have already alluded to the simplest and most frequent variety of perforation as being a mere extension of the ulcerative process to the peritoneum, which is followed by the sloughing or rupture of this delicate membrane, and the effusion of the contents of the stomach into the peritoneal cavity, with the result of a fatal peritonitis. And before passing on to consider those modifications of this process which have sufficient pathological interest to deserve a brief notice, we may point out a few general considerations respecting the event to which the term "perforation" is generally applied.

The history of a large number of such cases has been nearly as follows. —A person, often a young and apparently healthy female, in other instances dyspeptic or chlorotic during a variable time, has been suddenly attacked, soon after a meal, with excruciating pain in the belly, followed by all the symptoms of peritonitis, speedily ending in death. Such a rapid transition from apparent health to agonizing pain and death has naturally excited much attention; and has sometimes led to the suspicion of poison. But though the interest that has thus been attracted to these cases has given rise to much speculation respecting their nature and origin, still it has not hitherto led to their being collected and sifted in such a manner as to admit of any valid conclusions respecting many of their details. This defect the author has been anxious to supply, and has therefore brought together from various sources 234 instances of such perforation. And the information derived from these cases has suggested the following conclusions.

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* It is not impossible that the friction of such an abnormal surface may favour that recurrence of the ulcerative process which appears often to obtain in such cases.

Firstly, as regards the *frequency* with which perforation occurs in the course of the gastric ulcer, it is evident that our conclusions can only be based upon a number of careful examinations of the ulcerous stomach, made quite irrespectively of this event.

From the results of such inquiries by Duval, Rokitansky, Dittrich, Jaksch, Chambers, Habershon, Gairdner, and myself, I have collected a total of 257 cases of open ulcer, out of which 69 had perforated the organ. This is a proportion of about 1 perforation to 4 ulcers: more exactly, 1 to 3·725. And if we assume (as we have found good reason for doing) that these 257 open ulcers represent almost an equal number of scars, we should be led to conclude that not more than one in every 7 or 8 cases of gastric ulcer (1 to 7·45) terminated by perforating the walls of the organ:—a proportion which is equivalent to about 13·4 per cent.

Those of the observations I have collected, which include scars and ulcers, are less numerous and trustworthy than the data of the above calculations. Their more direct results, however, so far correspond with the preceding indirect estimate, as to afford us 137 cases of gastric ulcer, out of which 21 perforated. This is a proportion of about 1 to 6½, or 15⅓ per cent.

It is therefore evident that perforation is an exceptional occurrence in gastric ulcer; and that we have no right to infer anything as to the malady in general from this its occasional termination.

The *sex* of these cases of perforation offers nearly the same proportion as that which we have deduced for the ulcer generally. The 234 instances collected consist of 160 females and 74 males;—a ratio of about 2 to 1.

In respect of *age*, however, there is a remarkable contrast between the perforating ulcer and the ulcer generally. The latter we have found especially to affect the periods of middle and advancing life, with a frequency that gradually increases up to the extreme age allotted to Man. But the perforating ulcer seems not only to select another period of life, but to exhibit a marked contrast of age in the different sexes: the period of life in which it is most liable to occur being quite a different epoch in the male and in the female.

Of the cases of perforation which I have collected, 199 specify the exact age. This number is composed of 139 females and 60 males.

The most accurate contrast of these 199 cases, in respect of their age, is afforded by grouping them in epochs of seven years. Such an arrangement gives us the following table:

	Ages up to										
	7	14	21	28	35	42	49	56	63	70	,,
Number of females affected with perforation . . .	0	1	63	35	12	9	5	5	5	1	3
Number of males similarly affected.	0	2	3	13	8	4	9	7	9	3	2

The liability to perforation at the various ages in the two sexes, can only be determined by calculating the numbers of these cases, relatively to the number of persons living at such ages. And, for the sake of comparing them with our previous conclusions respecting the ulcer generally, we had better reduce them to similar epochs of ten years. The following table is the result of such a procedure, taking the maximum female liability to perforation as 100.

	At ages up to								
	10	20	30	40	50	60	70	80	beyond.
Comparative liability to perforation in the female . . .	1	*100	62	15	14	11	2⅓	13	"
In the male . . .	1	2⅓	20	13	22	23	13	5	15

The comparative liability of the ulcer to perforate in the two sexes at these different epochs, cannot be directly determined from the foregoing table. But we have already noticed that the proportion of the cases of perforation in the two sexes, is precisely that of the ulcer generally: two females to one male. And since the liability of the ulcer to perforation would consist in the number of instances in which this event occurs, divided by the number of ulcers generally, the comparative liability of the two sexes to perforation (as deduced by the preceding table), will not represent that of the ulcers, until the number of these lesions have been made to correspond to each other. In other words, since the ulcer is only half as frequent in the male, by multiplying the lower row of this table by two, we obtain a plausible inference as to the comparative frequency with which the ulcer perforates in the male and female at the above ages.

	At ages up to								
	10	20	30	40	50	60	70	80	beyond.
Comparative liability of the gastric ulcer to perforation in the female	1	62	62	15	14	11	2⅓	12	"
The same in the male .	1	4⅔	40	26	44	46	26	10	30

Although we can lay little stress on the details of such a comparison, it seems to leave no doubt of one important conclusion, which is in striking accordance with information derived from other sources: namely, that the greater liability of the gastric ulcer to perforate in the female during the 16 years that intervene between the age of 14 and 30, is so exactly compensated by a diminished risk of this event after that period, that the total risk of the two sexes during the whole of life remains nearly equal.

Finally, by adding together the two columns of the last table, we obtain an average liability of both sexes, which it will be interesting to compare with the similar table that we constructed for the ulcers generally. Throwing two decades of years together, for the sake of simplifying the contrast, we get the following numbers.

	At ages up to				
	10	30	50	70	90
Comparative liability to gastric ulcer . .	0	71	96	136	175
Comparative liability of gastric ulcer to perforate in the same periods	0	169	99	85	53

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* The liability for the whole of this decade is not more than 62. But as I have not been able to find a single case of perforation in the female between the years of 8 and 15, and only one prior to the former age, I have judged it better to prevent this important fact from being buried in figures, which, after all, are but intended to suggest and illustrate our conclusions. The reader has therefore to remember that the above maximum refers to the six years of female life which intervene between the ages of 14 and 20.

Here it is evident, that the general liability to perforation undergoes a constant decrease as life advances, even although the liability to the ulcer itself is just as constantly on the increase. The amount of these two converse alternations of risk we may regard as equal: and as being such that, from the age of 30 to that of 70, the risk of gastric ulcer gradually rises to double, while the risk of perforation from gastric ulcer gradually sinks to one half, its former amount.

The average age of the male and female subject of perforation affords an equal contrast. In the female it amounts to not more than $27\frac{1}{2}$ years, while in the male this age is raised to $42\frac{1}{7}$ years. Comparing these averages with the numbers of each sex respectively living at these two ages, we may estimate the general liability to perforation as having the proportion of 11 in the male to 18 in the female.

Again, a careful inquiry shows that the whole of the excess of cases of perforation in the female ((F) 160 — (M) 74 = 86), falls on the 16 years of life which intervene between the ages of 14 and 30 ((F) 108 — (M) 17 = 91): while nearly two-thirds of that excess belongs to the six years between 14 and 20. ((F) 54 — (M) 1 = 53).

Such a remarkable increase (or rather commencement) of liability to perforation at this particular[*] epoch of female life, naturally suggests the physiological events of this period as a more or less immediate cause of the occurrence. A further discussion of this topic would lead us too far into a consideration of the symptoms and causes of the gastric ulcer, to be attempted in the present article. At present it may suffice to point out, that not only do the proportions of older females, and of males, who succumb to this accident afford abundant evidence that it is essentially independent of any such cause, but that the circumstances of many of these cases themselves are such as inculcate great caution in coming to any definite conclusion on their causation. Some of them are expressly mentioned as not having arrived at puberty: others are recorded to have menstruated regularly, and even profusely: and finally, one of the most characteristic instances occurred in a person who, though supposed to be a female, was proved by a careful necropsy to be devoid of ovaries; and therefore, physiologically speaking, alike incapable of menstruation, or of any conceivable disorder of this function.

And whatever the relations which the various symptoms of such cases bear to each other, or however expedient it may be to regard these, with their age and sex, and their liability to perforation, as constituting them a special group, there can be no doubt that we know nothing at present which would justify us in regarding them as pathologically distinct from others. On the contrary, there is every reason to affirm, that in a large number of the females who become the subject of the lesion at this epoch of life, it has precisely the same origin, course, termination, and appearances as the gastric ulcer of any other person, old or young, male or female.

The maximum and minimum ages scarcely deserve notice. The oldest case I am acquainted with is that of a man of 82. The youngest instances in my notes are a girl of 8 and a boy of 9 years.

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[*] Dr. Crisp has the merit of first establishing this important fact, as the result of some interesting cases that have fallen under his own observation, and of a large number which he has collected, in the Lancet for 1843.

As regards the situation of the perforating ulcer, 191 of the cases just alluded to specify the part of the stomach affected. Of these 191, in 69 the ulcer occupied the lesser curvature; in 55, the anterior surface; in 11, the posterior surface; in 19, the pyloric extremity; in 10, the cardiac extremity; in 4, the middle of the organ;* and in no less than 24 there were two ulcers,† opposite to each other, on the anterior and posterior surfaces of the organ:—the former being the site of the perforation, while the latter was in most instances firmly adherent to the pancreas. In order more exactly to compare the situation of the perforating ulcer with that of the ulcer generally, we may increase all the above numbers by a proportion of $\frac{3}{19}$ths, which will convert their total, 191, into 220, the number of ulcers brought together in a preceding table. Such a procedure affords the following contrast of the two:

Situation in the stomach . .	Lesser curvature.	Anterior surface.	Posterior surface.	Anterior & posterior surfaces.	Pyloric extremity.	Middle of organ.	Cardiac extremity*
Number of gastric ulcers . .	60‡	10	96‡	13	32	5	4
Number of perforating ulcers occupying the same situations . . .	80	64	12	28	22	5	12

We thus deduce that, though the posterior surface of the stomach is the part most frequently the seat of ulcer, yet that it is one of those least liable to perforation: while conversely, the anterior surface, though much more rarely occupied by the ulcer, is yet one of the most frequent sites of perforation.

But it will be useful to place these two sets of figures in a more natural relation to each other. Assuming that our previous estimate of the general proportion between perforations and ulcers was a tolerably correct one, it will not only enable us to do this, but will even afford us a tolerable clue to the proportionate danger of perforation in the above situations. For example, we estimated the frequency of perforation as 13·4 per cent., or two in every fifteen cases of ulcer. Hence we may regard these 220 perforations as the result of (220 + $7\frac{1}{2}$ =) 1650 ulcers: and by grouping these under the same proportions for the different parts of the organ, we may re-arrange the preceding contrast as follows:

Situation in the stomach . .	Lesser curvature.	Anterior surface.	Posterior surface.	Anterior & posterior.	Pylorus.	Middle.	Cardiac extremity.	Total.
Number of examples of ulcer generally . .	450	75	720	$97\frac{1}{2}$	240	$37\frac{1}{2}$	80	1650
Number of instances of perforating ulcer.	80	64	12	28	22	5	12	220
Per-centage of perforations to ulcers, in these situations respectively . .	18	85	$1\frac{2}{3}$	28	10	$13\frac{1}{2}$	40	

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* In the two latter groups I have included one or two perforating ulcers equally referable to the larger curvature.

† Dr. Crisp (*loc. cit.*) has noticed several cases of this kind.

‡ Here I have divided the 15 cases in which Jaksch states the ulcer to have occupied both these situations, into the proportions (10 and 5) of 86 and 55, and added them to the latter numbers.

Of course, we are not entitled to lay much stress on the details of this contrast, in which the regions are too vaguely assigned, the numbers too small, and the probable inaccuracies of one element multiplied seven times. Making every allowance, however, for all these sources of error, it still affords us a remarkable numerical confirmation of what we might, *à priori*, expect to be the effect of situation on the ulcer of the stomach. Seated on the anterior surface of the organ, the lesion is very likely to perforate its coats; placed on the posterior surface, it is very unlikely to do so. In other words, could we rely on the accuracy of the above figures, it would appear that, in the former of these two cases, the probabilities are nearly 6 to 1 in favour of the occurrence of perforation; in the latter they are 60 to 1 against it. The relation of these surfaces to the occurrence of adhesion and to the nature of the tissue which effects it, have been already pointed out.

One feature of the above table deserves, however, a passing allusion.

The reader can hardly have failed to notice the anomaly implied in the statement, that while the ulcer of the anterior surface perforates about 85 times in 100 cases, the double ulcer of the anterior and posterior surfaces only does so about 28 times in 100, or less than one-third of that proportion. Now, we can scarcely suppose that the usual tendency of the anterior ulcer to undergo perforation, is directly diminished to such a great extent by the presence of another ulcer on the posterior surface. We are therefore left to conjecture some original or specific difference in the ulcerative process by which this anterior ulcer is produced. The curiously exact apposition of the two ulcerated surfaces in many of these cases appears to strengthen this suspicion: or rather, let us say, justifies our condensing it into the question,—Whether, in some of these instances, the anterior ulcer may not have been preceded and caused by the posterior one, the perpetual contact of which with this gastric surface has thus resulted in its secondary ulceration, just as the same irritating contact with the original ulcer often leads to ulceration or suppuration of the liver, pancreas, and spleen? That such a secondary ulceration should be less active, less intense, and therefore less likely to perforate, would not be very surprising.

The age at which perforation occurs would seem to be no way influenced by the site of the ulcer. Thus the average age of the subjects of perforating ulcer of the anterior surface of the stomach was 43 years in the male, 28 in the female: or the same as the average age of the subjects of perforating ulcer in general. These numbers were deduced from 13 and 54 cases respectively.

It is scarcely more possible to substantiate any influence exercised by the sex of the patient on the situation of the perforating ulcer. Remembering, however, the small (and therefore unsafe) numbers with which we are here concerned, we may mention that in the female sex the perforation seems more liable to occupy the anterior surface (54 female to 13 male cases); the opposite aspects of the anterior and posterior surfaces (19 female to 5 male cases); the cardiac extremity of the organ (10 female cases to 1 male); while the pylorus is more frequently perforated in the male than in the female (12 male to 7 female cases).

And even if we reduce these numbers to a more natural proportion by doubling the rarer male cases, we shall find that these contrasts remain still sufficiently distinct. The perforating ulcer of the anterior surface, and the double ulcer of the anterior and posterior surfaces, would seem to be twice as frequent in the female; and the ulcer of the cardiac end of the organ, five times as frequent. While the perforating ulcer of the pyloric extremity appears to be between three and four times more common in the male than in the female.

There can be no doubt that mechanical tension of some kind generally forms the immediate cause of perforation, by rupturing the thin film of tissue to which the ulcer has already reduced the coats of the stomach. Indeed, in a great majority of instances, the occurrence will be found to have taken place immediately after a meal; or in other words, with a distended stomach. And in less frequent cases we may find evidence of other mechanical agencies. Thus, in two of the above instances, the perforation appears to have been immediately brought about by vomiting;* in one instance, by a similar compression of the stomach by the abdominal muscles in the act of defæcation; in one instance, by the rupture of a delicate adhesion of the omentum to the anterior wall of the stomach, in the act of sneezing; in one instance (a girl of 20), by the sudden constriction of the waist by a tight belt; and in one instance (a man of 54), by the violent displacement of a kind of plug formed by the adherent omentum in contact with the exterior of the ulcer.

Complete perforation of the walls of the stomach is generally accompanied by the sudden effusion of more or less of its contents into the cavity of the belly. But the degree and extent of this effusion is liable to great variety. One or two instances are recorded, in which the accident has given rise to none of its ordinary symptoms, and has been followed by no appearances of peritonitis in the dead body. In some of these rare and anomalous cases it seems very doubtful whether the perforation was really complete during life, or whether the aperture observed may not have been caused by the solvent action of the gastric juice after death upon the film of peritoneum forming the base of the ulcer. In others, the anomaly appears to have been due to the state of the patient having prevented the access of all symptoms; the perforation having occurred during the approach of death, from the exhaustion produced by the ulcer, or by some independent disease. In equally rare instances the stomach appears to have been retained in such close apposition to the wall of the belly by the abdominal pressure, that scarcely any of its contents have escaped, save a small quantity of a clear fluid, which has (as it were) filtered between the surfaces of contact, and lit up the fatal peritonitis.

In other instances, the effusion of the gastric contents is confined to the immediate neighbourhood of the perforated spot; and the inflammation which they excite being equally limited, may be distinguished as circumscribed peritonitis. These cases are of course far less likely to be immediately fatal than those in which a wider extent of the serous surface is involved in the inflammation. Hence the patient often survives the first shock of the accident, only to succumb to the combined effects of peritonitis and gastric exhaustion. In other instances, however, a different result obtains: the portion of the peritoneal cavity circumscribed by the inflammation continues to suppurate, and is thus gradually converted into a chronic abscess, that finally discharges its contents at some point or other of its exterior. There are about twenty cases of this kind on record. Their age and sex give me precisely those averages which we have already deduced for the accident of perforation generally. Their other features are almost as easily summed up.

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* I am obliged to quote from memory another case that I think I have met with in some journal, and in which an emetic, incautiously administered for the relief of certain symptoms regarded as dyspeptic, gave rise to the rupture of a gastric ulcer, and thus caused the death of the patient.

As implied above, the circumscribed character of the inflammation appears due to the limited diffusion of the gastric contents; which, so far as they reach, seem always to excite this process. What restrains them in such narrow bounds it is not always easy to specify. Sometimes, however, it is evidently a deposit of lymph, caused by extensive adhesive inflammation around the ulcer prior to its perforation. Sometimes the delicate omentum forms a septum that bounds the lower surface of the sac. Sometimes the transverse colon lends a more or less temporary aid to the process: or a casual coil of some other part of the intestinal canal affords a similar assistance. Sometimes the aperture in the peritoneum seems too narrow to allow of more than an inconsiderable leakage, such as spreads very slowly on all sides of it. In any case, the rapid effusion of lymph has a strong tendency to render such localizations permanent, and thus to seal up the mischief within the limits to which such (almost fortuitous) mechanical causes have at the time confined it.

The opening of a gastric ulcer into the chest is generally accomplished by the mediation of such an abscess: less frequently by a recurrence or extension of the ulcerative process destroying an adhesion between the stomach and the diaphragm. The penetration of this septum has been known to be followed by instantaneous suffocation. In most instances, however, the fatal event is preceded by an interval, during which gangrene of the lung or other pulmonary lesions arise. The pericardium is very rarely opened.

The communication of the stomach with the exterior of the belly by a fistulous aperture seems, in most of the instances* recorded, to have been the result of a similar abscess, which has pointed and burst like an abscess of the liver.† The gastric fistula, once established, either kills by exhaustion, or (what seems more usual) gradually closes, just like the artificial fistula established in animals for the purpose of physiological experiment. Subsequently to its closure, the adhesion of the stomach to the anterior wall of the belly is sometimes drawn out into a cord, which is occasionally excavated by a funnel-shaped cavity, that is itself continuous with the inner surface of the stomach at its broad base, and is lined by a smooth membrane of a serous aspect.

The communication of the stomach with other parts of the alimentary canal, as the result of gastric ulcer, is generally independent of any such abscesses. The ulcerated part of the stomach becomes attached by lymph

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* About six are all I can recollect to have met with.

† In one case of this kind, which was fatal by hæmatemesis, the abscess in front of the stomach communicated with a suppurating cavity, that occupied the areolar tissue of the rectus abdominis muscle.

to some portion of intestine in contact with it: and a mere extension of the ulcerative process successively removes the parietes of the stomach, the lymph, and the coats of intestine where they are united to each other. As regards the situation of such abnormal apertures, there are one or two cases recorded in which the stomach has opened into the neighbouring segment of the duodenum; and about ten in which a similar communication has been brought about between the the stomach and colon. The comparative frequency with which this segment of the canal is selected as the site of the communication, is of course referable chiefly to its situation and size.

The frequency with which ulceration implicates the liver and pancreas cannot be exactly estimated. But from the large proportion of ulcers that occupy the posterior surface and lesser curvature of the organ, these viscera must be very often attacked. The cardiac extremity of the stomach is so much more rarely the seat of the ulcer, that it is not surprising penetration of the spleen should belong to the rarer sequelæ of the malady. And as none of these viscera can be excavated by the ulcer unless they have been previously attached to the stomach by adhesive inflammation, the perforation which their excavation really implies has a much less dangerous character than where it opens the peritoneal sac. The chief danger, indeed, seems to be that of hæmorrhage; either from the larger vessels that occupy the upper border of the pancreas, or from those smaller ones that ramify in the substance of the liver and spleen for their supply. Gangrene of the two latter viscera is, however, by no means unfrequent. And of course, the adhesions alluded to may themselves at any time become the seat of further ulceration; which, without causing any new perforation of the coats of the stomach, can open directly into the cavity of the belly, and cause a fatal peritonitis.

Hæmorrhage is another of the accidents connected with ulcer of the stomach which deserves a special inquiry. We shall perhaps hereafter see that the discharge of blood from the mouth or anus, which generally follows a considerable gastric hæmorrhage, constitutes one of the most frequent and important symptoms of the ulcer. At present we shall limit ourselves to a brief notice of its production, and shall especially treat of its significance as a termination of the malady:—in other words, as a cause of death.

In respect to the sources of such hæmorrhage, we may distinguish four: which, speaking generally, come into operation at different dates of the malady; and which certainly have a very different influence on its course.

In the first place, analogy and observation coincide to indicate that the congestion which often attends the commencement of ulceration of the stomach may give rise to a hæmorrhage from the vessels of the mucous membrane. But since, without any existing breach of surface, we cannot define the case as one of ulcer, while, with it, we can rarely exclude the possibility of its having been the source of the bleeding, such a cause of hæmorrhage is rather to be admitted as a probability, than stated as a fact.

The progress of the ulceration itself determines the three following varieties of hæmorrhage. As the breach of surface gradually involves the vascular mucous membrane, it successively erodes a vast number of vessels: —at first mere capillaries; then the minute arteries and veins from which these capillaries ramify; and lastly, the small vessels of the arterial and venous plexuses that occupy the sub-mucous areolar tissue. The hæmorrhage determined by these numerous solutions of continuity is probably often arrested at once, by a coagulation of the blood within the open extremities of the eroded vessels. More frequently, however, it gives rise to a slow drain of blood in very moderate quantity. This, as it flows, mingles with the secretions and contents of the stomach: and gradually undergoes the usual changes of blood when exposed to the digestive action of the alimentary canal; exchanging its crimson colour for one which is almost black, and exhibiting (if in sufficient quantity for such a change to be visible) a viscid or tarry consistence. In rare instances the quantity of blood thus set free is much more considerable, and closely imitates the more important hæmorrhage which forms the third variety. In such cases we may conjecture the hæmorrhage to be increased by a sudden congestion of the ulcerous stomach.

The third and most serious class of hæmorrhages is one in which the bleeding comes from a large artery of the stomach. Consistently with this source, it will generally be found to occur at that later period of the gastric ulcer when, after penetrating the mucous and muscular coats, it reaches that interval between the latter and the peritoneal coat in which these vessels run; or when, in the case of an ulcer of the posterior surface, it has eaten into the adhesion fixing it to the pancreas, so as to erode the splenic artery that courses along the upper border of this gland.

The blood poured out by such hæmorrhage often exhibits the characteristic marks of its arterial source, even after it has been expelled by vomiting. In other instances, it possesses a colour and coagulation that vary with the amount poured out, the rate of its flow, the gastric contents with which it has been mixed, and a variety of other circumstances. In some cases it is rapidly effused in such vast quantity, that death ensues almost instantaneously; and it is only at the necropsy that its cause is revealed, in the shape of an enormous mass of clotted blood, that distends the stomach and a variable extent of the intestinal canal.

Such hæmorrhages have one feature in common with perforation—viz., that they generally occur soon after a full meal. The mechanical influence of distension of the stomach in disturbing the eroded segment of the vessel is too obvious to require any comment. It seems to be assisted by that afflux of blood to the organ which attends its digestive act. This view is confirmed by one or two cases on record, in which the hæmorrhage appears to have been excited by violent mental emotion.

I have not been able to bring together satisfactory data for any estimate of the frequency with which these larger bleedings occur. But in many cases—probably of the majority—they are altogether absent during the whole progress of the ulcer. In many cases, again, they do not cause death. And even when they are fatal, it is rarely by only one attack.

In one or two instances the necropsy has shown a peculiar condition of the vessel, such as quite explains the intermittent, though repeated, character of these hæmorrhages. The ulcer has cicatrized over its whole extent with the single exception of a point in the centre, which is occupied by the eroded artery. And the calibre of this tube has been found filled by a clot, the detachment of which from time to time has evidently allowed the hæmorrhage to take place, with long intermissions to its flow. Why the tissues of the artery present this contrast with the neighbouring cicatrix, we must for the present forbear to inquire.

The fourth kind of hæmorrhage has already been alluded to, as forming what is strictly a sequela of perforation, and consisting in the erosion of vessels that occupy the substance of the liver, pancreas, or spleen. These vessels are generally the small arteries and veins that supply the proper substance or parenchyma of the above glands. And the moderate hæmorrhage to which their breach of continuity usually gives rise, undergoes changes similar to that of the second variety.

If we now proceed to examine those cases of ulcer of the stomach which have been fatal by hæmorrhage, we may glean some interesting information:

Firstly, as regards the frequency of death from this cause, its proportion to the ulcer in general can only be determined directly from the statements of Willigk and Jaksch; which, put together, amount to a total of 261 ulcers, that include 13 fatal by hæmorrhage. This is a ratio of about 1 in 20, or 5 per cent.

Turning to more indirect (and therefore uncertain) methods, the cases recorded by Sangalli, Rokitansky, Dittrich, Jaksch, Willigk, and Duval, afford results that precisely correspond with this. They show a total of 316 open ulcers, of which 32 were fatal by hæmorrhage. And assuming these 316 open ulcers to represent 316 scars, this would afford us a proportion of 32 to 632, or about 5 per cent.

On the other hand, the cases which I have collected, chiefly from British sources, seem to indicate that the ulcers fatal by hæmorrhage bear a somewhat smaller proportion to the number of ulcers generally. The various records I have looked over have afforded me 57 instances of this kind of death; while the same search has given me 235 instances of fatal perforation. And hence, if we assume* these numbers to represent the relative frequency of the two events, and further suppose our previous estimate of the frequency of perforation a correct one, we may conjecture that these 57 instances of hæmorrhage correspond to ($235 \times 7{\cdot}45 =$) 1751 cases of ulcer; which is a proportion of about 1 in 31, or 3¼ (3·26) per cent.

In 52 of these cases the *sex* is mentioned—34 being male and 18 female.— The preponderant number of males renders these instances of hæmorrhage a remarkable contrast with the cases of perforation already adduced. Indeed, since we have found reason to suppose that the ulcer occurs twice as frequently in the female as in the male, it would seem that the liability of a given ulcer to be fatal by hæmorrhage must be nearly 4 times ($34 \div \frac{18}{2} = 3{\cdot}8$) greater in the male than in the female sex.

The average *age* of these cases in the two sexes renders them an equally marked contrast with the cases of perforation. Out of 44 instances which specify the age, there are 30 males, with an average of 43½ years (43·6), and 14 females with an equal average of 43¼ (43·2). The maximum and minimum ages are, in the male, 78 and 13; in the female, 70 and 22, respectively.

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* It is true we have no right to assume that such casual and independent records as those from which many of these instances were derived, would include the two varieties of ulcer in exactly their natural proportion to each other. Still, it seems by no means improbable that something approximating to this has really happened. Many of these instances are from groups of cases by pathologists whose researches (like those of Abercrombie, Cruveilhier, and others,) were evidently directed equally toward all such lesions of the stomach. And of the scattered cases contributed by others, we may at least say, that the symptoms of a fatal gastric hæmorrhage are as likely to arrest attention, and thus to receive investigation, as those of the more frequent accident of perforation. While the errors of such numerous observations would in some sense correct each other.

In 52 of these cases the situation of the ulcer is mentioned. In 24 it occupied the small curvature; in 17, the posterior surface; in 6, the pyloric extremity; in 2, the anterior surface; in 2, the cardiac extremity; and in 1, the middle of the organ. But a comparison of these sites with those of the ulcer generally* does not afford a contrast sufficiently marked to justify any further remark.

The exact source of the hæmorrhage is specified in 34 of the 57 cases. In 3 instances it was the substance of the liver; in one instance, the substance of the spleen; in the remaining 30 cases, a large vessel. And in 29 of these 30 cases the vessel itself can be named:—or to quote more exactly, 23 of the number specify the vessel; while in 6, the description is such that a practical anatomist could scarcely doubt of its identity.

In 16 of the 29 instances, hæmorrhage was the result of an ulcer which eroded the splenic artery in its course along the upper border of the pancreas. And, as we might expect, the majority (11) of these cases are described as ulcers of the posterior surface of the stomach; while 2 are stated to be ulcers of the lesser curvature, 1 of the pyloric extremity, and 1 of the middle of the organ. In the remaining 13 of the 29, the vessel which gave rise to the fatal bleeding was the superior pyloric or coronary artery. Which of these two vessels, however, it seems often impossible to determine. And considering the complete continuity with each other, which they usually offer, any such distinction would generally be arbitrary and useless. Indeed, here, as in the case of the splenic artery, it is not unlikely that one of the larger branches of the vessel may sometimes have been mistaken for the trunk. Of these 13 cases, 11 were the result of ulcers on the smaller curvature of the stomach in the ordinary course of the eroded vessel; while 1 case is referred to the pyloric extremity, and 1 to the anterior surface of the organ.

The 3 ulcers causing excavation of the liver were all seated on the smaller curvature of the organ in contact with the gland; that penetrating the spleen on the cardiac extremity of the stomach, adjacent to this viscus.

Concerning the *exhaustion* or *starvation* which forms another of the fatal terminations of the gastric ulcer, it is much to be regretted that we have scarcely any numerical data to offer:—the more so that there can be little

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* For the convenience of the reader, I subjoin the following comparison, in which the above figures are multiplied by 4¼, so as to bring their total to an equality (52 × 4¼ = 221) with the 220 cases of ulcer generally, with which we previously compared our 191 cases of perforation.

Situation in the stomach	Lesser curvature.	Posterior surface.	Anterior & posterior.	Pyloric extremity.	Anterior surface.	Cardiac extremity.	Middle of organ.
Number of ulcers in general	60	96	13	32	10	4	5
Number of ulcers fatal by hæmorrhage	102	72	0	25	8	8	4

It is true that the numbers of ulcers present in the two first situations seem to differ considerably from each other. But we must remember, that not only do these two parts of the stomach merge into each other by gradations which it is easy to confound, but also that they are liable to be affected by the distortion which a large and adherent ulcer can produce. While if we add together the cases belonging to both these situations, and include (as we ought) the instances of double lesion in the third column, we shall find the proportionate numbers of the general and special ulcer about equal to each other (60 + 96 + 13 = 169, 102 + 72 = 174).

doubt, both of its frequency, and of its being sometimes preventable by suitable medical treatment. I have, however, collected 14 cases, in which it seems probable that the ulcer caused death in this way.

In about 7 of these 14, it seems probable that the exhaustion was produced, not so much by any direct influence of the ulcer on the digestive powers of the stomach, as by the vomiting of food to which it had given rise. And in one or two other cases it appears to be possible (though not probable) that the patient's powers were materially enfeebled by moderate hæmorrhage from the lesion.

Do these numbers give us any clue whatever to the average frequency of this mode of death? I think not. Their number (14), compared with that of the cases (234) of perforation, and the frequency (1 in 7·45 cases) of this termination (as already adduced), would assign them a proportion of not more than 1 per cent. But Dittrich, after carefully excluding 34 cases in which the marasmus they caused appeared partially referable to the old age of the patient, still finds 3 per cent. (3 in 103 instances) in which death was caused by tabes referable solely to the ulcer. And in the last few weeks I have myself witnessed two fatal cases of this kind; besides seeing two or three in which the patient's life has appeared in considerable danger from this cause, although death has not resulted.

As respects the combinations of gastric ulcer with cancer of the stomach, they seem to be chiefly limited to a cancerous degeneration or deposit, that involves the hard brawny mass which we have already noticed as generally present, in variable quantity, in the base or periphery of an ulcer of long standing. In some rare instances, in which the whole of the substance around the ulcer has been converted into a cancerous excrescence, it is chiefly by the shape and other characters of the ulcerated depression that (in the absence of any history of the case during life) we should discriminate between the cancerous degeneration of the hard margin of an ulcer, and the ulceration of a growth originally cancerous. But in the majority of such cases the decision is less difficult. Indeed, the most frequent form of such a combination appears to be that in which a fungus (generally a bleeding one) shoots up from the basis of a gastric ulcer, the characters of which are in all other respects those usually seen in the ordinary lesion.

Lastly, as regards the complications of the gastric ulcer with diseases of other organs, the best information which I have been able to collect is derived from the writings of Jaksch, Dittrich, and Engel.* Comparing the statements of these observers, which refer to a total of some hundreds of cases of the lesion, we find them all agreeing as to the frequency with which the ulcer is associated with pulmonary tubercle. This complication appears to be present in about 19 or 20 per cent. of the whole number of ulcers. Jaksch and Engel also correspond in stating the frequency of pneumonia and pleurisy at about 27 per cent. Dittrich and Jaksch, again, agree in representing 10 per cent. of the ulcers as associated with cancer of other organs. Engel finds 10 per cent. to be connected with previous syphilis.

To these I may add the following cases of my own collection, which probably specify the chief cause of death, rather than the full results of a sedulous examination of all the organs. Diarrhœa and dysentery, 4 cases; renal disease, 2; ovarian disease, 1; pneumonia, 2; bronchitis, 1; apoplexy, 1; fever, 3; phthisis, 4; other independent disorders, 2 instances.

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* Schmidt's Jahrbucher, pp. 82, 237. 1854.

Even as regards what is said above respecting the exhaustion as a cause of death, it is hardly necessary to remind the reader how little exactness many of our conjectures must necessarily possess:—how difficult, for instance, it would often be, after the most careful study of the history of a given case, to say whether death had been chiefly caused by the exhaustion or impaired nutrition that had been for years the result of the presence of the ulcer, or by the moderate hæmorrhage that had once or twice occurred in its course, or by the (apparently casual) diarrhœa that had immediately preceded the fatal event.

And an equal uncertainty applies to all these remarks concerning the complications of gastric ulcer. They seem to indicate—what indeed there is little difficulty in supposing—that this long and exhausting malady, which is itself the expression of a serious lesion in one of the most important organs of the body, predisposes the constitution to a variety of other diseases; and renders unusually fatal many of those attacks of illness which, in the course of years, very few persons altogether escape.

But when we turn from this probable (though vague) relation of the gastric ulcer to disease generally, and proceed to inquire what are the special maladies to which it is peculiarly calculated to predispose the constitution, we find how little information is contained in such statements as the foregoing. For example, the per-centage of phthisis above-mentioned renders it one of the most frequent complications of the ulcer. While we have seen that it is that about which there is most agreement in the observations hitherto on record. But the significance of such a proportion must evidently depend, not so much on its absolute amount, as upon a comparison of this with the average share taken by phthisis in the mortality from all causes. In other words, if the gastric ulcer really had any very direct or marked influence as a predisposing cause of phthisis, we should expect to find, not merely a large absolute number of ulcerous cases dying of this latter malady, but such a proportion as would considerably exceed the average ratio of the deaths by phthisis to those from all causes indifferently. But the deaths by phthisis, in persons of both sexes above the age of 20,* amount to rather more than 18 per cent. of the deaths from all other diseases. Hence the statement that 20 per cent. of the cases of gastric ulcer die of this malady, is one which, even if confirmed by a wider series of observations, will not by any means justify us in assuming a direct causative influence.

We may end this brief sketch by a summary, which well illustrates how much we have yet to learn respecting even the more obvious pathological relations of this important malady. Let us assume (what, however, it would be very rash to assert), the accuracy of all the conjectures to which the preceding statements have led us. Let us suppose that, of every 100 ulcers of the stomach, 50 cicatrize, 13½ perforate its walls, 3½ erode its larger vessels, and 2 or 3 kill by the sheer exhaustion and inanition they involve. We have still a proportion of about 30 ulcers in every 100 left quite unaccounted for. In other words, we have yet to determine the termination of nearly one-third of all the instances of this lesion:—and are ignorant whether the presence of an ulcer in the stomach heightens the liability to disease in general, or to certain maladies in particular; or finally, whether the persons who are the subjects of such a lesion have merely the ordinary liability to most other maladies, failing the access of which, the ulcer does but anticipate, hasten, or increase, that gradual failure of the nutritive functions which is one of the most essential elements of death by old age.

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* An age that we have seen may be taken as the commencement of that epoch of life during which the gastric ulcer chiefly occurs.

ART. II.

Annual Report of Cases Admitted into the Medical Wards of St. George's Hospital during the Year ending December 31st, 1854. By Dr. BARCLAY, late Medical Registrar of the Hospital.

THE present forms the fourth of the series of annual reports of medical cases at St. George's Hospital which have been arranged on a uniform plan, and the second which has appeared in the pages of this Journal.

Last year some remarks were made on the classification adopted, which need not be here repeated, as they are by no means essential to a right understanding of its forms and uses. Their chief purport was to explain why the system of the Registrar-General had not been adopted, unchanged, as might have seemed the more natural course, showing the practical difficulties that arose in applying a return of mortality to a scheme of disease, and the theoretical difficulties which especially applied to the theoretical class of zymotic diseases.

On the present occasion, any facts that seemed to deserve especial notice have been placed in a series of remarks at the end of the table, numbered in accordance with the principal divisions of disease; and it is hoped that in this form they will be more intelligible and more easily referred to than when placed, as in the Report of last year, at the bottom of each page.

Cases admitted during the year 1854.						Admitted during four years.	
Nature of Disease.	Admitted.	Died.	Per-centage of mortality.	Complicated with other diseases.	Deaths among complicated cases.	Admissions.	Per-centage of mortality.
1. Fevers:							
Continued fevers	185	23	12·4	69	11	559	12·2
Influenza	16	...	...	4	...	87	...
Asiatic Cholera	139	68	48·9	33	16	140	49·3
2. Eruptive fevers:							
Measles	9	...	...	3	...	17	...
Scarlatina	24	3	12·5	4	2	43	18·6
Varioloid	6	...	...	3	...	16	...
Erysipelas	30	3	10·0	10	3	94	14·9
3. Intermittent fevers:							
Quotidian	5	...	...	...	...	18	...
Tertian	12	1	?	1	1	32	...
Quartan	...	...	...	...	...	5	...
Irregular	3	...	...	...	...	7	...
4. Rheumatism:							
Acute	90	5	5·5	35	5	255	4·3
Subacute and slight	105	1	?	24	1	406	...
Chronic	139	2	?	28	2	550	...
5. Gout (including rheumatic gout)	37	...	...	11	...	106	...

	Cases admitted during the year 1854.					Admitted during four years.	
Nature of Disease.	Admitted.	Died.	Per-centage of mortality.	Complicated with other diseases.	Deaths among complicated cases.	Admissions.	Per-centage of mortality.
6. Poisoning:							
Irritant	1	...	...	...	...	9	22·2
Narcotic	1	...	...	...	...	7	14·3
Syphilitic and gonorrhœal	7	...	...	5	...	36	...
Hydrophobia	...	...	...	...	...	...	...
7. Colica pictonum	5	...	...	2	...	30	...
8. Entozoa:							
Intestinal worms	2	...	...	...	...	19	...
Echinococcus hominis	...	...	...	...	...	2	100·0
9. Dropsy:							
Anasarca	104	41	39·1	101	41	401	32·9
Ascites	18	8	44·4	18?	8	70	51·4
10. Hæmorrhages:							
Epistaxis	4	1	?	4	1	15	13·3
Hæmoptysis	23	9	39·1	23	9	79	25·2
Hæmatemesis	3	2	66·6	2	2	21	19·0
Hæmaturia	6	1	16·7	5	1	28	7·2
Intestinal	10	1	10·0	7	1	34	17·1
Uterine	5	...	...	5	...	23	...
11. Purpura and scurvy:							
Purpura	6	1	16·7	6	1	24	25·0
Scurvy	2	...	....	...	...	5	...
12. Anæmia	83	3	3·6	52	3	254	2·0
13. Chlorosis	14	1	?	8	1	79	...
14. Cachæmia	8	5	62·5	7	5	35	40·0
15. Scrofula	14	...	...	6	...	39	10·3
16. Tubercular diseases:							
Phthisis	128	49	38·3	77	36	511	39·0
Tubercles in peritoneum	5	4	80·0	4	4	20	85·0
Tubercles in brain	1	1	100·0	1	1	9	88·8
17. Morbid growths:							
Hydatids	1	1	100·0	1	1	7	71·4
Encephaloid	16	10	62·5	10	8	36	66·9
Scirrhus	18	4	22·2	6	1	85	16·5
Colloid	...	...	...	...	...	1	100·0
Growths from bone	...	...	...	...	...	2	...
18. Hysteria	52	...	...	17	...	215	...
19. Chorea	20	...	...	8	...	73	...
20. Delirium tremens	17	4	23·5	7	2	57	12·3
21. Tetanus	1	...	...	1	...	2	50·0
22. Diseases of brain and spinal cord:							
Cephalitis	9	8	88·9	7	7	37	83·8
Chronic disease	6	1	16·7	1	1	31	25·8
Apoplexy	9	6	66·6	9	6	24	58·3
Epilepsy	27	3	11·1	11	3	117	16·2
Functional disturbances	58	3	5·2	22	3	172	3·5
Inflammation of cord	5	4	80·0	5	4	10	70·0
23. Paralysis:							
Hemiplegia	33	4	12·1	9	4	117	8·4
Paraplegia	25	6	24·0	7	6	85	14·1
Local paralysis	2	...	...	2	...	23	4·4
24. Neuralgia:							
Tic douloureux	2	...	...	...	...	13	...
Sciatica	11	1	?	2	1	48	...
Hemicrania	1	...	...	...	...	5	...
Angina	...	...	...	...	...	2	...
25. Diseases of the heart:							
Pericarditis	12	5	41·7	12	5	57	36·8
Endocarditis	15	1	6·6	15	1	53	7·5
Hypertrophy	38	19	50·0	35	19	113	60·2
Dilatation	15	8	53·3	14	8	56	50·0
Valvular disease	60	15	25·0	50	15	224	24·5
26. Diseases of arteries and veins:							
Aneurism	6	1	16·7	1	...	17	23·5
Phlebitis	5	1	20·0	2	1	25	28·0
27. Diseases of the respiratory organs:							
Laryngitis	10	3	30·0	6	3	30	36·7
Tracheitis	1	...	...	...	...	3	...

Cases admitted during the year 1854.						Admitted during four years.	
Nature of Disease.	Admitted.	Died.	Per-centage of mortality.	Complicated with other diseases.	Deaths among complicated cases.	Admissions.	Per-centage of mortality.
Bronchitis	117	13	11·1	77	12	488	13·1
Emphysema	16	5	31·3	16	5	48	35·4
Asthma	1	...	...	...	...	3	...
Pneumonia	57	20	34·8	26	14	165	29·6
Pleurisy	60	22	36·7	41	22	208	29·8
Pneumo-thorax	...	...	...	...	...	5	80·0
Pertussis	3	...	...	1	...	5	...
28. Diseases of the mouth and pharynx:							
Glossitis	...	...	...	...	...	...	...
Quinsy	14	...	...	4	...	43	...
Enlarged tonsils	2	...	...	1	...	15	...
Ulceration	5	...	...	1	...	15	?
Mumps	...	...	...	...	...	8	...
29. Diseases of the stomach and œsophagus:							
Dyspepsia	77	...	...	40	...	243	?
Ulceration	2	2	100·0	2	2	6	?
Stricture	4	1	25·0	4	1	7	14·3
30. Diseases of the intestinal canal:							
Constipation	44	...	...	21	...	198	...
Obstruction	...	...	...	...	...	4	50·0
Diarrhœa	70	5	7·1	38	5	145	7·6
Dysentery	5	2	40·0	2	...	16	37·5
Tympanitis	...	...	...	...	...	10	...
Ulceration	2	...	...	...	...	10	70·0
31. Diseases of the peritoneum:							
Acute peritonitis	27	10	37·0	15	9	90	40·0
Chronic peritonitis	19	7	36·8	15	7	55	50·9
32. Diseases of the liver and gall bladder:							
Inflammation and congestion	8	2	25·0	5	2	25	24·0
Cirrhosis	12	9	75·0	12	9	49	73·5
Jaundice	19	8	42·1	14	8	69	29·4
Enlargement	15	5	33·3	10	5	42	26·2
Gall-stones	...	...	...	...	...	2	...
33. Diseases of the spleen:							
Enlargement	5	2	40·0	4	2	14	28·6
34. Diseases of the pancreas	...	...	...	...	...	...	...
35. Diseases of the urinary organs:							
Nephritis	9	...	...	8	...	19	...
Abscess	2	1	50·0	1	1	8	25·0
Albuminuria	94	41	43·6	89	40	322	48·8
Ischuria	1	1	100·0	1	1	1	100·0
Diuresis	...	...	...	...	...	1	...
Cystitis	6	2	33·3	4	2	23	13·0
36. Diabetes	4	1	25·0	...	...	17	17·6
37. Diseases of the ovaries:							
Dropsy	3	2	66·6	1	1	19	31·6
Tumours	10	1	10·0	2	1	29	?
38. Diseases of the uterus and vagina:							
Amenorrhœa	12	...	...	11	...	50	...
Menorrhagia	16	...	...	11	...	54	...
Leucorrhœa	8	...	...	6	...	45	...
Tumours	8	...	...	4	...	16	?
Prolapsus	6	...	...	1	...	21	...
Ulceration	...	...	...	...	...	...	...
Congestion	2	...	...	2	...	7	...
Diseases of external organs	5	...	...	...	...	5	...
39. Diseases of bones and joints	4	1	25·0	4	1	34	20·6
40. Diseases of skin and cellular tissue:							
Erythema	13	...	...	5	...	52	...
Urticaria and roseola	6	...	...	5	...	13	15·4
Lichen and prurigo	3	...	...	1	...	9	...
Squamous eruptions	7	...	...	2	...	37	...
Vesicular eruptions	17	...	...	15	...	68	...
Pustular eruptions	6	...	...	6	...	24	...
Pompholyx and rupia	2	...	...	1	...	12	?
Cellular inflammation and abscess	20	9	45·0	16	9	38	50·0
41. Diseases of muscles	...	...	...	...	...	1	100·0
42. Anomalous and accidental cases	7	3	...	3	2	21	?

Remarks.

1. *Fevers.*—No attempt has been made further to subdivide continued fever, because, though from earliest times there has been a great tendency to discriminate varieties, no arrangement has as yet met with universal approbation and assent. There was no evidence from analogy of symptoms during last year of the existence of epidemic influence assuming any special form or characterized by any particular lesion. Of the 23 fatal cases, 5 were not examined post-mortem, 11 presented various stages of ulceration of the lower end of the ileum, 6 showed no ulceration, but in a few there were patches of elevated glands more prominent than in health.

The complications in the fatal cases are distributed as follows—2 with cholera, in one of these the cholera succeeded the fever, in the other a distinct attack of fever, with miscarriage, supervened on convalescence from cholera. In one case death was caused by erysipelas occurring after a severe attack of fever with ulcerated bowels, and the complication of pleuro-pneumonia.

2 deaths resulted from peritonitis,
1 case was complicated with hæmorrhage from the bowels,
2 „ „ „ inflammation of brain,
1 „ „ „ inflammation of spinal cord and subsequent paraplegia,
1 „ „ „ pneumonia,
1 „ „ „ bronchitis and sloughing bed sores,
1 „ „ „ disease of kidney.

Among the cases enumerated as cholera, are included the in-patients treated for choleraic diarrhœa. It is very difficult to assign names which shall convey exactly what is meant in the nice discrimination of gradations of disease. The cases now referred to, which are indeed only 16 in number, were so closely allied to the fully developed cholera, that by some they were called "mild cholera." They have been kept distinct from another class of cases, which seemed equally to have a "zymotic" origin, if one may so say, and were offshoots from the cholera epidemic, although they could not properly be called "choleraic." These last have been entered in the Patients' Register as fever and diarrhœa, and swell the number of complicated cases, under the head of diarrhœa, for the past year somewhat beyond the average. (See 30. Intestinal Canal.)

Of the 139 cases classed under cholera, 31 occurred among patients already in the hospital, servants, and nurses, 22 being cases of decided cholera, and 9 of mild cholera, or choleraic diarrhœa: 108 cases were therefore admitted with symptoms of cholera. Of these 101 were decided, and only 7 of a milder character. If the 123 severer cases be alone taken as cholera, the mortality rises from 48·9 to 55·3 per cent.

The patients attacked by cholera in the hospital were the following:—

Fatal Cases.

1 case of fever.
1 „ erysipelas from chronic ulcer.
1 „ ague.
2 „ acute rheumatism.
1 „ cancer.
2 „ phthisis.
1 „ pleurisy.
1 „ ulceration of stomach.
1 „ disease of kidney.

Cases of Recovery.

4 cases of fever.
1 „ acute rheumatism.
2 „ chronic rheumatism.
1 „ dropsy, with disease of the heart.
1 „ scrofula.
1 „ phthisis.
2 „ pneumonia.
2 „ dyspepsia.
2 „ jaundice.

No statistics can be given of the complications in the remaining fatal cases, as the deaths were at one time so numerous that it became impossible to make post-mortem examinations.

2. *Eruptive Fevers.*—One case of measles occurred eleven days after admission, when there was no other patient in the hospital with measles, nor any evident source of infection. Scarlatina appeared in a child suffering from congenital paralysis five days after she had been admitted—this case proved fatal. There was no other instance of any of the infectious exanthemata occurring in the hospital last year, and probably both cases just noticed came within the period usually allotted to the incubation of their respective fevers, so that they may be considered due to infection caught before admission. A second case of scarlatina terminated fatally, which was complicated with delirium tremens. A third was fatal in which there was no complication.

The number of cases admitted with scarlatina during the year 1854 is unusually large, but the mortality is below the average of four years. It will also be found, on reference to another part of the table, that the admissions with nephritis, the majority of which were scarlatinal, exceed the average. (See 35. Kidneys, &c.)

The cases of erysipelas, as well those admitted with the disease as those occurring in the hospital, are more numerous than the average of the three preceding years. In 1853 only 3 cases commenced in the hospital; while in 1854 there were 9. Careful records have been kept, in conformity with the scheme issued by the Epidemiological Society; but the results have been entirely negative: neither in the medical nor in the surgical wards has any evidence been obtained of the spread of the disease by contagion, or of any other circumstances with which its existence is intimately or constantly connected in this hospital. In their tables, diffuse cellular inflammation is placed as one of the congeners of erysipelas; it is deficient, however, in many of those characters which alone could warrant its being classed among fevers, and it is placed in this Report as a local malady under the head of Diseases of the Skin and Cellular Tissues. (See 40.)

There were only 3 deaths among the patients attacked by erysipelas, giving a mortality of ten per cent. All these were among the complicated cases. One occurred in a patient who had passed through a very severe attack of continued fever, with ulceration of the bowels and pretty extensive pleuro-pneumonia; a second in a case of anæmia, with flabby heart; a third patient had phthisis.

3. *Intermittent Fevers.*—The death recorded under tertian ague resulted from an attack of cholera which occurred in the hospital.

4. *Rheumatism.*—In the subdivision of this class, the absence of fever, as well as of redness and tenderness, or their limitation in such circumstances to one joint or a portion of one extremity, has been considered a sufficiently distinct guide in excluding any particular case from the class of acute rheumatism, which has been limited to cases such as are appropriately designated by the corresponding term of rheumatic fever.

In 26 cases of acute rheumatism, some form of disease of the heart was also noted; 17 being believed to be cases of recent inflammation, and 9 being regarded as diseases of old standing. These numbers would represent very nearly 29·0, 19·0, and 10·0 per cent. of the cases admitted; and probably are rather below the average.

Of the fatal cases,

2 died of cholera,
2 had pericarditis and inflammation of the lungs,
1 had inflammation of the spinal cord.

Two deaths occurred among patients admitted with chronic rheumatism,

1 from general tubercular disease,
1 from old disease of the heart and kidney.

5. *Gout.*—The larger proportion of patients included in this class belong more properly to what is called rheumatic gout. Characterized by thickening and distortion of the joints, and separated from true gout by the absence of chalky deposit. It would probably be better that these two classes should be kept quite distinct, but the rules of diagnosis are by no means clear, and the division will not be always accurate.

6. *Poisoning.*—The poisons in the two cases recorded were nitric acid and opium.

Syphilis and gonorrhœa can only be found casually among the cases in the medical wards.

9. *Dropsy.*—Anasarca is here used as the generic name for general dropsy, because, wherever else effusion takes place, some amount of fluid always finds its way into the areolar tissue, and generally appears there sooner than elsewhere. Ascites is the only independent form of dropsy recognised, other effusions being either the result of general anasarca or of some specific local disease. In discriminating the cases belonging to each class, an error is sometimes first detected by post-mortem examination, which proves that the organic lesions were such as were much more likely to have produced ascites, and yet anasarca has been reported as the first observed symptom of disease. It is probable that the cases referred to ascites are therefore too few, and the mortality relatively too great.

Organic disease of some sort or other is recorded in 96 out of 104 cases of anasarca. In 8 cases the effusion seemed to be the result of mere debility, and 3 of these presented only such trifling complications that nothing is recorded concerning them.

In two cases of ascites, the cause of the existence of fluid in the peritoneal cavity was not quite clear.

Of the fatal cases,
4 had disease of the liver, and
4 had chronic peritonitis,
as the chief cause of effusion.

10. *Hæmorrhages.*—The death recorded under epistaxis resulted from cholera. It is impossible to record all the instances in which the expectoration is only slightly tinged with blood, nor would these be properly regarded as hæmorrhagic. In one instance only was the hæmoptysis so severe as to prove directly fatal. Among those recorded, it was associated,

in 17 cases with phthisis, of which 5 were fatal,
in 5 cases with disease of the heart, of which 3 were fatal,
in 1 case with chronic peritonitis and ascites, also fatal.

In both patients who died, among those entered under hæmatemesis, disease of the liver was found after death, and in one of them disease of the spleen was also present.

Among the cases of hæmaturia, 4 had dropsy. The fatal case was associated with purpura, and dependent on alteration of blood produced by disease of the liver.

Intestinal hæmorrhage was found to have depended in the case which terminated in death on ulceration of the bowels. It occurred in a patient suffering from continued fever.

Uterine hæmorrhage has been limited to those cases in which it could be recognised as an accidental occurrence, and thus discriminated from menorrhagia, the name which has been reserved for such cases as were merely an increase of the natural function, either in extent of duration or in frequency of return. There were 16 of this class (see 38. Uterus), while there are only 5 considered as hæmorrhage.

11. *Purpura.* (See 10. Remarks on Hæmaturia.)—The remaining complications were,

in 3 instances with disease of kidney, in two of which dropsy was also present,
in 1 „ with dropsy alone,
in 1 „ with confluent small-pox, which was immediately removed to Small-pox Hospital.

12. *Anæmia.*—Here used to express all forms of general weakness and exhaustion which were not characterized by the actual existence of specific morbid action; the term is not absolutely restricted to cases in which the proportion of red particles is reduced below the standard of health. Among the cases recorded as anæmic,

1 death resulted from erysipelas,
1 „ from disease of liver,
1 „ from disease of spleen.

13. *Chlorosis.*—Especially indicating cases in which the absence of menstruation is either the cause or the aggravation of that form of anæmia in which the colouring matter is deficient in amount. Menstruation, suspended or rendered scanty by previously existing anæmia and exhaustion, is not classed under this head. The only death resulted from the supervention of phlebitis and peritonitis during the stay of the patient in the hospital. There are 12 cases referred to the head of amenorrhœa (see 38. Uterus) which were not chlorotic.

14. *Cachæmia.*—The limits of this class are rather indefinite; it has been taken as including, 1, pyæmia, or the supposed existence of pus in the blood associated with phlebitis, secondary depôts, &c.; 2, cachexies having no specific character of scrofula, tubercle, cancer, &c., especially as recognised by unhealthy suppurations, "festering" wounds, &c.; 3, "poisoned wounds," when some substance in a peculiar state of chemical change is casually introduced under the skin, and induces an altered condition of blood.

All the deaths in these cases were confined to the first division or pyæmia, which was generally associated with diffuse cellular inflammation or abscess. (See 40. Skin, &c.)

16. *Tubercular Diseases.*—Three out of five cases in which tubercles existed in the peritoneum, presented the same deposit in the lungs: only one of these was so far relieved as to be able to leave the hospital. The existence of tubercles in the brain can very rarely be determined during life. It was only found once on post-mortem examination during last year, in a patient admitted with concomitant chronic disease of the brain. Tubercles were also found in this case in the lungs.

17. *Morbid Growths.*—One serous cyst only was noted during last year. It was found in the brain after death, which seemed to be caused by inflammation of the spinal cord; the echinococcus was not present.

20. *Delirium Tremens.*—This disease was alone the cause of death in 2 cases, in the other 2 it was complicated,
in 1 by the co-existence of scarlatina,
in 1 by disease of the liver.

21. *Tetanus.*—The only case presenting symptoms of idiopathic tetanus in the hospital last year, was that of a child, in which tetanic spasms came on very frequently after slight movement or any other cause of irritation, lasting occasionally for some hours. The other symptoms seemed to indicate the existence of inflammation of the spinal cord (q. v.): the child recovered.

22. *Diseases of the Brain and Cord.*—Cephalitis embraces all acute inflammations, whether called meningitis or phrenitis, while chronic disease includes all organic changes of slow development. Functional disturbance is the name assigned to those undefined conditions in which it is impossible to say that actual change of structure has occurred, its range extending from persistent headache to insanity.

Of the fatal cases of cephalitis,
3 occurred in persons of a tubercular diathesis,
2 „ in cases of fever,
1 „ in a person of broken-down health, where pleuro-pneumonia, associated with pericarditis, supervened on disease of liver,
in 1 case a fibrous tumour existed in the brain.

In the remaining cases, one of which terminated in death, and one in recovery, no other morbid condition was discovered.

One of the cases of chronic disease still remained under treatment on the 31st of March. The only patient that died was found, on post-mortem examination, to have tubercular deposit. (See 16. Tubercular Diseases.) The hydatid cyst found in the brain (see 17. Morbid Growths) is not again enumerated, because there was no evidence of further disease set up by it.

In 3 out of the 6 fatal cases of apoplexy, disease of both heart and kidneys were found associated together.

Among the deaths in epileptic patients,
2 resulted from disease of kidney,
1 from disease of heart.
In none was death caused by the epileptic seizures.

The cases in which coma and convulsions supervened on old standing disease, are referred to functional disturbance, and include the only deaths under this head.
2 were dependent on disease of kidney,
1 „ on disease of heart.
Exclusive of hypochondriasis, 17 were distinctly instances of insanity, generally in its earlier stage, and comparatively amenable to treatment.

Only one patient recovered of those believed to have inflammation of the cord: it was accompanied by tetanus (q. v.), and was probably meningeal. Of the remaining four,
1 seemed to arise from exposure,
1 occurred in the course of an attack of continued fever,
1 was associated with disease of kidney,
1 was chronic, and of long standing.
All of these were accompanied by paraplegia.

23. *Paralysis.*—Among the patients dying hemiplegic, the paralysis was found,
in 3 cases dependent on apoplectic clot,
in 1 „ on chronic disease of brain.

The paraplegic cases terminating fatally were dependent,
4 on inflammation of spinal cord,
1 on disease of vertebræ,
in 1 child it was congenital, and death was caused by scarlatina.

24. *Neuralgia.* (See 2. Eruptive Fevers.)—The only death among neuralgic cases was that of a patient admitted with sciatica, who died of phthisis.

25. *Diseases of the Heart.*—Pericarditis and endocarditis were noted six times coinciding in the same patient.

Pericarditis was associated,
in 9 cases with acute rheumatism,
in 2 „ with pleurisy,
in 1 „ with dropsy and hæmoptysis, and a very early stage of disease of the kidney.

Endocarditis was associated,
in 14 cases with acute rheumatism,
in 1 with disease of kidney.
The only death was one in which pericarditis was also present.

It is almost impossible that all cases of hypertrophy or of dilatation should be recognised during life, and the mortality under each is consequently too high. Cases of fatty degeneration, from their symptoms, naturally fall under the class of dilatation, even when no enlargement of the cavities is observed.

In 21 cases of hypertrophy, dropsy was present.
Exclusive of other less important complications,
10 cases were associated with valvular disease,
13 „ „ with disease of kidney,
2 „ „ with both these diseases together.
In 10 cases of dilatation, dropsy was present.
Exclusive of other less important complications,
2 cases were associated with valvular disease,
4 „ „ with disease of kidney.
In 21 cases of valvular lesion, dropsy was present;
11 cases were admitted with some form of rheumatism.
In addition to other less important complications,
15 cases were associated with disease of kidney,
12 „ „ with disease of lungs,
5 „ „ with disease of liver.

26. *Blood-vessels.*—The fatal case of phlebitis occurred in a case of chlorosis, and was associated with peritonitis.

27. *Respiratory Organs.*—In all the instances of laryngitis, the disease was more or less chronic, and in 4 it was associated with phthisis.

In the case of tracheitis, tracheotomy was performed, and a most perfect recovery followed.

Among the patients suffering from bronchitis, there were 12 deaths; of these the disease was associated,
in 1 case with continued fever,
in 1 „ pneumonia,
in 2 „ emphysema and tubercles,
in 1 „ emphysema alone,
in 7 „ diseases of heart and kidneys, 2 having emphysema.
All the cases of emphysema were associated with bronchitis.

In 17 examples of pleuro-pneumonia, both affections were of such importance as to lead to the cases being classed under each head separately, though probably among so large a number of individuals they must have co-existed in minor degrees much more frequently. In 5 instances other complications also existed; of the remaining 12, 5 were fatal.

Low pneumonia supervened on bronchitis in 2 cases, of which 1 was fatal. The other complications of pneumonia were chiefly the following,
3 were associated with secondary suppuration,
6 „ „ with fever,
3 „ „ with phthisis,
4 „ „ with disease of liver,
5 „ „ with disease of kidney.

Pleurisy existed in one instance along with bronchitis as its only complication. The other complications were chiefly the following,
3 were associated with fever,
4 „ „ with peritonitis,
12 „ „ with phthisis,
9 „ „ disease of heart, kidneys, or liver.

29. *Stomach and Œsophagus.*—The chief complications of dyspepsia were of anæmia and constipation. No case is recorded as ulceration

which was not ascertained to be such by post-mortem examination; one patient died of peritonitis, the other of cholera.

All the cases of stricture of the œsophagus were believed to be cancerous.

30. *Intestinal Canal.*—The only patient with obstruction was one in which slow occlusion of the canal was produced by the contraction of a band of lymph stretching across it.

Among the deaths recorded under diarrhœa,

3 were due to phthisis,
1 „ „ disease of heart and dropsy,
1 „ „ disease of kidney and peritonitis.

(For Choleraic Diarrhœa, see 1. Fevers.)

Epidemic cases which were not choleraic, are entered as diarrhœa and fever; there are 13 such among the complicated cases in the table for the year 1854.

It has been found impossible to reckon the forms of ulceration occurring in the course of fever and phthisis.

31. *Peritoneum.*—Among the fatal cases, acute peritonitis was associated,

in 2 cases with fever and ulcerated bowels,
in 1 „ ulceration of stomach,
in 2 „ pleurisy,
in 1 „ phlebitis,
in 1 „ chronic peritonitis,
in 2 „ disease of kidney.

Chronic peritonitis was associated,

in 1 case with recent peritonitis,
in 1 „ tubercular deposit,
in 2 „ morbid growths,
in 2 „ ovarian tumours,
in 1 no complication beyond the presence of ascites existed.

Of the whole number of cases of chronic peritonitis, fluid existed in the cavity twelve times.

32. *Liver.*—Inflammation terminated in 3 instances in abscess, 2 of which terminated in death, and 1, after very long duration, in apparently complete recovery.

All the cases of cirrhosis recorded were associated with dropsical effusion. The disease is one extremely difficult to recognise during life, and even after death its limits are by no means accurately defined. The presence of ascites is one of those circumstances which, taken together, afford some clue to the knowledge of its existence, but not unfrequently it is only by post-mortem examinations that its presence becomes known. Hence the fact of its being accompanied in every case by dropsy, and the high rate of mortality, are not points of much value. In 4 of those examined after death, disease of the heart was found, and in 2 of these disease of the kidney also co-existed.

Among the fatal cases of jaundice,

3 were associated with disease of heart,
2 „ „ pleuro-pneumonia,
2 accompanied the secondary fever in deaths by cholera,
1 was attacked by cholera in the hospital, and died; another, similarly attacked, recovered.

In 7 instances enlargement was believed to be dependent on the presence of deposit of malignant character; of these, only 2 died in the hospital. In 3 other cases enlargement of the spleen co-existed; 1 of them died, and the other 2 were not much benefited by treatment.

33. *Spleen.*—Besides those just mentioned under disease of the liver, enlargement of the spleen co-existed in one fatal case with disease of the kidney.

No very exact limit can be assigned to enlargement of the spleen as a disease; it may be three or four times as large in one person as another, without giving rise to any symptom, or being recognised during life, and with no evidence that it ought to be regarded abnormal after death. Great enlargement is here necessarily the condition alone referred to.

35. *Kidneys and Bladder.*—The subdivisions of kidney-disease are at present by no means accurate. Nephritis has been limited in this Report to cases of acute inflammation, and of scarlatinal dropsy with bloody albuminous urine. All but one during last year belonged to the latter class; all of them were associated with dropsy; in 1 case endocarditis co-existed, and in 1 case bronchitis.

Dropsy was present in 58 of the cases admitted with albuminuria. In 9 instances sympathetic affections of the brain were present; in 15, affections of the lungs; and in 6, rheumatism and gout.

Among the fatal cases, 20 were associated with diseases of the heart.

The only case of suppression last year occurred in one of those of albuminuria, which terminated fatally.

36. *Diabetes.*—The only death was that of a woman, admitted in a state of extreme exhaustion, who died in a few hours. No complication existed. One patient who was improving remained in the hospital at the 31st of March.

37. *Ovaries.*—The division into dropsy and tumours is probably scarcely tenable; it merely discriminates those having a solid feeling from those manifestly containing fluid. The only patient dying in the hospital with a solid enlargement was one in whom obstruction had gradually come on from constriction of the intestines, in consequence of chronic peritonitis. (See 30. Intestinal Canal.)

38. *Uterus, &c.*—Twelve cases are classed as amenorrhœa, in addition to the 14 already enumerated under chlorosis (q. v.), because there was no concurrent anæmia.

Under menorrhagia are recorded all examples of simply increased menstrual discharge, whether in frequency or amount; the accidental cases, of which 5 are enumerated, are referred to uterine hæmorrhage. (See 10. Hæmorrhages.)

39. *Bones and Joints.*—The case terminating in death was one of disease of the spine, and consequent paraplegia.

40. *Cellular Inflammation and Abscess.*—Under this class there are 7 cases which partook of the character of diffuse cellular inflammation, or of secondary suppuration and pyæmia, following abscess. In the tables issued by the Epidemiological Society, these are all classed along with erysipelas. No results indicating any relationship have been obtained,

nor any facts pointing to a specific origin, either endemic or contagious. (See 2. Eruptive Fevers.) There were 3 cases recorded as examples of diffuse inflammation, of which 2 died; and 4 of secondary suppuration, all of which died.

ART. III.

Algiers: its Climate and Merits as a Resort for the Invalid. By ARTHUR MITCHELL, A.M., M.D.

HAVING had occasion lately to spend some months in Algiers, I occupied myself in acquiring information concerning its climate, and its merits as a place of resort for invalids; and I think I was sufficiently successful to warrant my bringing the facts I collected, and the deductions which flow from them, before the profession.

I have to acknowledge with thanks the aid received from Dr. Foley, whose high position and attainments, with his long residence in the country, give great value to his opinion on such a subject as the one I propose to discuss; he not only furnished me himself with many important data, but pointed out to me the channels through which I should obtain others. And scarcely in different terms have I to express the thanks which I owe to Dr. Bertherand, the enlightened head of the medical staff of the military hospital; and to his colleagues, Drs. Leclerc and Lavereau. To Captain Humbert also, and to M. Bourget, amateur but able meteorologists, I owe my thanks for the most liberal manner in which they placed all their observations at my disposal; and I cannot omit the acknowledgment of the assistance, direct and indirect, which was so readily rendered to me on all occasions by our respected consul-general, Mr. Bell, and by Mr. Ellmore, the vice-consul.

French Algeria is a wide possession, stretching as it does eastward from the empire of Morocco to the kingdom of Tunis, washed by the Mediterranean on the north, traversed by the chains of the Atlas in its central regions, and with its southern limits ill defined as it merges into the Great Desert.

When we regard it as divided into the district stretching between the feet of the Atlas and the shore, sometimes hilly and sometimes in extensive plains; with plateaus more or less elevated, forming great steps, up the northern slopes of the Atlas; with similar plateaus on the southern slopes; the hilly Sahara, and the Great Sahara itself—passing between the extremes of a rich perennial verdure, and lifeless, barren wastes of sand, between sea levels and altitudes on the confines of eternal snow—when we regard it thus, and remember its position on the meteorological chart of the world, between the temperate and torrid zones, we are prepared for the fact, that its climatology is as varied as the features of its surface.

It must not be imagined, then, that what is true of one spot is true of the whole. It is never so in reference to any country, and still less so in reference to this region, where topographical accidents have such powerful influences. My object is, in the first instance, to establish, as far as

I can, the meteorology of the capital of the country, the El Jesair of the Arabs, corrupted by the Franks into Algiers.

This metropolis stands in 36° 49′ 30″ north latitude, and 3° 28′ east longitude of Greenwich, being nearly in the same parallel as many points of the northern shore of the Mediterranean, such as Malaga and the south of Spain generally, Sicily, Greece, and part of Asia Minor. It is somewhat further north than Malta, Egypt, and Madeira, and further south than Nice, Florence, Leghorn, Rome, &c.

About thirty miles inland from the sea, the Little Atlas forms the boundary of a vast marshy plain, which, less than thirty years ago, was the home of some 100,000 Arabs, whom it fed and made wealthy by the excess of its produce; by whose villas it was studded, and who sang of it as the "Mother of the Poor." Now, when looked upon from the heights, it wears the aspect of desertion, and its green treeless area looks like an inland sea, with the houses of its former inhabitants floating like wrecks on its surface. Yet the smiling prosperity of some of its French settlements, such as Bouffarick and Bené Mered, when more closely seen, give hope that this sad picture of desolation may yet be reversed, although the pestiferous air of its swamps will, as heretofore, demand a heavy tribute of those who first attempt to subjugate them.

Between this rich plain and the sea extends the range of the Sahel hills, varying from 500 to 1300 feet in height, and of a different and more recent data than the Atlas.

The rocks of this range are of the tertiary lime formation, resting upon talcose mica-schist, with veins of quartz—the schist passing at certain points into feldspar and gneiss; and the fossils most generally found in them are of shell fish which exist in the Mediterranean at the present day.[*]

These hills are cut across by winding valleys of the most charming beauty, which for a quiet, picturesque loveliness I believe are scarcely anywhere surpassed. Their flora is luxuriant and varied, differing, but not widely, from that of the south of France and Spain. Their sides are ornamented with Moorish villas, whose whitewashed walls and paradisial gardens tell they are peopled; yet, being windowless, they break not the solitude of the spot. Orange and lemon groves are met at every turn; the pomegranate, the olive, the almond, the carob, the mulberry, and the fig-tree, are in every garden; while more rarely the stately date-palm, the flowering aloe, and the rich banana, are brought into contrast with the dark gloomy cypress, and perhaps with some solitary stone-pine, which, when recognised, is affectionately hailed by an Englishman, as speaking of his own less lovely but not less happy country. A stroll along the old Arab walks which creep up these valleys is certain to please the most fastidious taste. They extend for miles, and you may vary them every day of a winter's residence, and each one will be found to have something new—some beauty peculiarly its own. They wind along the sides of the slopes between hedges of the Barbary fig (*cactus opuntia*), and shrubs and wild olives, garlanded with the most chaste and tender creepers, and peopled with nightingales. You meet with no interruption, beyond the occasional salute of a stately Moor returning from the city with his morning's purchases, or the ghost-like form of some veiled Mauresque gliding past with her swarthy attendant.

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[*] Pulszky's Wagner, page 40.

[†] Munby.

It is on the northern slope of this range of hills that the city of Algiers stands.

A half-circle indentation, with Cape Matifou and the Pointe Pescade for its seaward extremities, forms the Bay of Algiers, whose waters wash the feet of the Sahel or Bousaria hills at many points, while at others small fertile plains intervene. Where Algiers stands, the slope is prolonged almost to the water's edge, so that as you approach the town from the sea, it seems to cling to the side of the hill, and is not unlike a great chalk quarry, the houses being all white-washed, and disposed in irregular terraces. It expands along the shore from the gates of Bab-el-oued to Bab-azoun for about a mile, and becomes narrower as you ascend to the old fort of the Casbah, which is the culminating point, and is about 350 or 400 feet above the level of the sea. It thus presents a triangular appearance, with its base taking the curve of the bay.

The attention is scarcely less forcibly or pleasingly arrested by the cheerful-looking suburbs of St. Eugene on the one side and Mustapha on the other; and one cannot but be struck with the countless Moorish villas with which the faces of the hills are dotted, half concealed in the luxuriance of the vegetation which surrounds them, so much at variance with what one is too apt to associate with "sterile Africa," and so marked a contrast with the arid, burnt-up look of the Marseilles coast so recently quitted.

The steamer has scarcely dropped her anchor, when one feels that everything around is thoroughly new and un-European, and that objects of interest, amusement, and instruction have not here to be sought out, but are to be encountered on all sides: the boats that crowd around the vessel with their motley crews and Babel tongues—the Spaniard and Maltese, the Negro, and the grey-bearded Moor, who may have pulled a pirate's oar, where now, for a fixed and small tariff, he rows the Christian to the shore. Once on land, the objects of wonder multiply, and when you reach the "Place du Gouvernement" in quest of the large and commodious Hotels "*de la Régence*" or "*d'Orient*," they have attained their climax.

With difficulty, if at all, will the European traveller find a spot on earth where natural beauties so combine with those of man's creation to please and interest him. One of the long sides of the oblong of which the "Place" is formed, is open to the sea, and commands a view of the bay, the harbour, the site of the ancient Rusginium, the peaks of the distant Atlas, and the verdure of the Sahel slopes. The "Place" itself is filled with a strange mixture of races, in their striking and varied costumes;—the stately Arab of the desert walks up and down with the handsome Moor, his brother of the town; the industrious hard-featured Kabyle passes on with his burden to or from the port—the turbaned Jew of Africa talks of gain to his jewelled brother of Europe—the fever-marked colonist from the plain tells his troubles to a bloused mechanic—the dandy civilian promenades with the dashing captain of Chasseurs—the Maltese fisherman fraternizes with the Spanish fruit-seller—veiled women of Moslem glide among the crowd, and the picturesque Jewess, and pretty Spaniard with her graceful mantilla, are seen, not with but among the gay ladies of France—detachments of soldiery are constantly marching past to the strains of martial music; and troops of donkeys, with their negro drivers, scramble out of the way of the omnibus and *diligence* so constantly passing and repassing. To this strange concourse of nationalities, this strange mixture of races, costumes, and professions, add the effect of the Moorish architecture, with its mosques and minarets standing beside the modern erections of the French—look at the paletot walking with the burnous—civilization touching barbarity—Christianity, Paganism—the remote, the present.

And it is not of small moment to the invalid that pleasure and interest meet him thus at every step; that he has neither fatigue nor risk in seeking them; that they are of a nature to amuse without exciting him, adapted to all tastes and to all capabilities.

In this point of view, Algiers contrasts most favourably with other places of resort, where the objects of interest are chilly cathedrals, cold picture galleries, and such like, which fashion demands that the stranger shall visit, be he an invalid or in health, and for which he must assume a spurious enthusiasm, if, from defect of nature or education, he lacks the taste. It is of real importance that the invalid shall leave his room, not full of some excursion to a cathedral or ruin, but simply to be in the open air, to wander about where his fancy may lead him, sure of finding himself gratified and amused. He returns ere he feels fatigue,—for he has no prescribed mission or task which he must accomplish,—in good spirits, and refreshed, with as much to talk of and think of as he can desire.

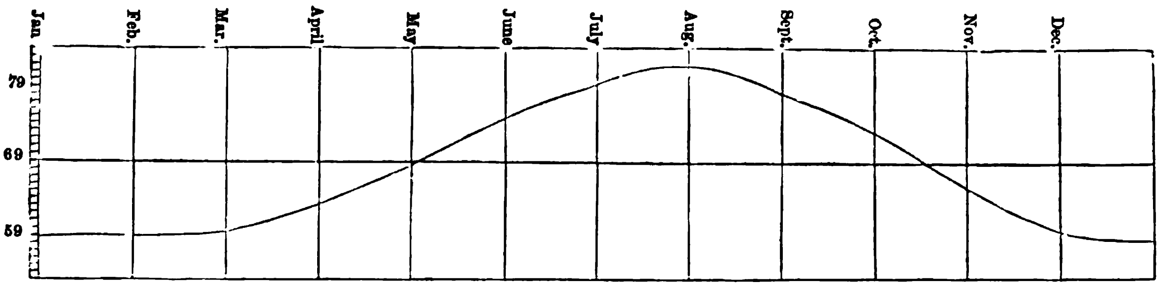
Believing it of importance that the resort of the invalid should be beautiful and full of interest, as well as possessed of a good climate, I have given the preceding sketch of the general features of Algiers, which at the same time has served the purpose of making more clear its geographical bearings. The examination of its climate, however, is at present my chief object, and to that I now proceed.

More general information I shall have an opportunity of giving on a future occasion.

METEOROLOGY OF ALGIERS.

Temperature.—The observations which form the basis of the following table I obtained from different sources. Those for several of the earlier years were made by the "Direction du Port d'Alger," and were furnished to me by Dr. Foley and others. Those for the later years were made by M. Bourgét and Captain Humbert, and were placed at my disposal by those gentlemen themselves. I have reduced them to the scale of Fahrenheit, calculated the averages, and so arranged them as best to suit my object.

The means for some of the years are calculated from the mean monthly maxima and minima; and the hours of observation from which the averages of other years are calculated, although generally three a day, vary frequently and considerably, even in the hands of the same observer. Such a variety of elements, however, increases the accuracy of the final results arrived at in the following table, and gives a most trustworthy statement of the mean temperature of the place.



Mean Monthly and Annual Temperature of Algiers, from Observations of Thirteen Years.

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Annual.
Mean temperature, calculated for the period of five years, from 1837 to 1841	63·12	62·06	62·20	65·86	71·24	76·28	81·32	83·84	81·50	76·28	69·64	64·94	71·51
Mean temperature for the period of five years, including 1842-43-44-46 and '47 .	57·74	58·82	61·38	63·51	70·07	75·77	81·87	81·50	78·27	74·84	65·57	60·67	69·17
Mean temperature for the period of three years, from 1852 to 1854, inclusive . .	56·69	56·15	56·58	62·82	67·95	73·35	78·05	80·87	76·83	70·43	63·99	56·85	66·71
Mean temperature for the period of thirteen years above indicated	59·18	59·01	60·05	64·06	69·75	75·13	80·41	82·07	78·86	73·85	66·40	60·82	69·13

In every case, as will be afterwards seen, where such a mixture has been objectionable, I have either only associated the observations of such years together as were identical in the required aspects, or, failing to find this, I have selected some single year. In all instances, however, I have stated the period of time over which the observations extend that form the ground of any conclusions.

The table exhibiting the yearly averages shows a progressively diminishing temperature, which has its explanation, I think, in the preceding remarks, although at first it struck me as singular and interesting.

The fourth item of this table is calculated from upwards of thirteen thousand observations, and may be interpreted into the following statements:

1. The mean temperature for the whole year: 69·13.
2. The mean for each month—see lowest series of table.
3. The mean temperature of each season, viz.—

Winter	62·13	Summer	75·09
Spring	61·04	Autumn	78·26

4. The difference of the mean temperature of summer and winter, 12·86.
5. The difference between the means of the successive seasons, viz.—

Difference between winter and spring	1·09
„ „ spring and summer	14·05
„ „ summer and autumn	3·15
„ „ autumn and winter	16.13

6. The difference between the mean coldest and hottest months—

Coldest, February	59·01
Hottest, August	82·07
Difference	23·06

7. The difference between the mean temperature of the successive months, viz.—

Between Jan. and Feb.	— 0·17	Between July and Aug.	+ 1·66
„ Feb. and March	+ 1·04	„ Aug. and Sept.	— 3·21
„ March and April	+ 4·01	„ Sept. and Oct.	— 5·01
„ April and May	+ 5·69	„ Oct. and Nov.	— 7·45
„ May and June	+ 5·38	„ Nov. and Dec.	— 5·58
„ June and July	+ 5·28	„ Dec. and Jan.	— 1·64

8. The mean difference of the successive months, 3·84.

As regards the phenomena of temperature, therefore, we should rather divide the year into two seasons than into four. Winter scarcely differs from spring, and together they form what may be called "*the temperate or warm season*," while they are separated by a range of some fourteen or fifteen degrees from summer, between which and autumn again there is but a slight range, and these may be said to constitute "*the hot season*."

The mean annual temperature more nearly approaches that of Malta than of any other of the more ordinary resorts of the invalid. It exceeds it, however, by 2°, while it exceeds Malaga by 3°, Madeira by 4°, Rome by 9°, Nice by 10°, and Pau by 13°. The mean annual temperature of Cairo, however, is 3° higher, yet its winter is 4° colder, than that of Algiers.

This excess of the annual mean over that of Madeira depends upon the greater summer heat at Algiers, since, as regards the seasons of spring and winter, the two places are almost identical, there being only a difference of a tenth of a degree between their means for this half of the year. It follows, therefore, that the difference between the hottest and coldest months, and between spring and summer, will be less in the case of Madeira; but the difference between winter and spring is less at Algiers, and is indeed less than in any place with the meteorology of which I am acquainted.

Spring is about equally warm at Malta, Madeira, Malaga, and Algiers —the last, however, being one degree below the others. But the Algerian winter is 2° warmer than that of Madeira, 5° warmer than Malta, 8° warmer than Malaga, 13° warmer than Rome, 14° warmer than Nice, and 15° warmer than Pau; while the spring at Cairo, which is 15° warmer than its own winter, is 11° above that of Algiers.

The hottest month at all these places is August, and the coldest either January or February. The difference between these at Algiers is about the same as at Malta, less than at Cairo, Nice, or Pau, but about double that at Madeira, which for equability of temperature over the whole year is unsurpassed. It is, however, to escape the winter and spring of our country that the invalid seeks another, and during these seasons the climate of Algiers contrasts, as regards steady warmth, even with Madeira, and excels most others. The summer and autumn are hot, being much like Malta, but exceeding Madeira, Pau, Rome, and Nice, and inferior to Cairo. Compared with Malaga, its summer temperature is 4° cooler, but its autumn 10° hotter.

The mean difference of the successive months is greater than at Madeira or Teneriffe, but considerably less than at Malta, Nice, Pau, Cairo, or any of the other places given in Sir James Clark's tables, which have served as the basis of the foregoing comparisons. It is less also than that of Malaga as recorded by Dr. Francis, to whose writings on the subject I am indebted for my information about this place.

In looking at the table of differences between one month and its successor, it will be observed that the temperature steadily rises during one half of the year, from February to August, and steadily falls from August to February.

In addition to the foregoing, I have the mean maxima and minima during five years, from observations made by the "Direction du Port," from which the *approximative* mean temperature of the place may be calculated.

For Five Years, from 1837 to 1841.	Jan.	Feb.	March	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Annual.
Mean max. temp.	68·18	66·74	66·92	70·88	77·36	83·30	87·62	87·98	87·44	81·86	75·20	70·88	77·03
Mean min. temp.	54·32	54·68	56·84	61·34	68·54	72·32	76·46	78·98	73·94	66·02	65·12	58·82	65·62
Mean difference .	13·86	12·06	10·08	9·54	8·82	10·98	11·16	9·00	13·50	15·84	10·08	12·06	11·41

This table embodies between 3000 and 4000 observations, and may be interpreted as giving—

9. The mean daily range for each month—see third line of figures in table.

10. The mean daily range for the year, 11·41.

11. The mean daily range for each season, viz.—

Winter	12·00	Summer	10·32
Spring	10·56	Autumn	12·78

The mean annual daily range, therefore, is much the same as at Rome and Montpellier, while it is less than at Malta, and greater than at Madeira by 1·6°, than at Nice by 3°, than at Pau by 4°, and than at Malaga by 7°, which last has only a daily range of 4·1°. Dr. Francis, however, does not state whether the observations from which this is calculated were made by self-registering thermometers. This I understand to be essential, since it is not the difference between the maximum and minimum temperature observed, but the difference between the absolute maximum and minimum of the twenty-four hours, which constitutes the daily range. If I calculate this, for instance, from the observations which I possess for 1853, made by M. Bourgét at 7 A.M., midday, 4 P.M., and 7 P.M., I have a result even below that for Malaga, the mean daily range being reduced to 3·3°. And certainly the period of twelve hours thus embraced is that of most importance to the invalid, since during the other half of the day he is or ought to be in his room, where he experiences in a modified manner the external variations of temperature. But for purposes of comparison it is well to know the nature of the observations from which this result flows.

Dr. Casimir Broussais,* in writing of Algiers, says that the temperature of morning and evening is almost equal, and that the "*full day*" at the hour of its maximum is only from 3° to 5° C. warmer; viz. 5° C. in summer and 3° or 4° C. in winter. While, on the other hand, the nights are only 2° or 3° colder than the evenings. The foregoing table speaks in rather less favourable terms, and so do my own observations there, but it is probably owing to the cause to which allusion has just been made. Dr. Broussais and many other French writers think the hour of maximum temperature is at 11 A.M., but I think I am correct in placing it between 2 and 3 P.M.

The following additional facts in reference to the temperature of Algiers are drawn from the observations of M. Bourget for 1852 and 1853, those for the first year being made at 7 A.M., mid-day, and 7 P.M., and those for the second at 7 A.M., midday, and 4 P.M.

12. Extreme annual range of temperature observed—

1852.	Min., February,	44·6	and	Max., August,	88·7—	Difference		44·1
1853.	„ „	46·4	„	„ Sept.,	91·4	„	„	45·0
	Mean . . .	45·50			90·05			44·55

13. Extreme range of temperature observed during each month, viz.—

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* Mém. de Méd. Milit. vol. lx. seconde éd.

	1852.	1853.		1852.	1853.
January	11·7	18·9	July	10·8	22·5
February	14·4	16·2	August	11·7	18·9
March	18·0	18·0	September	12·6	19·8
April	10·8	14·4	October	18·0	13·5
May	14·4	16·2	November	16·2	21·6
June	10·8	19·8	December	12·6	14·4

14. Extreme range observed on any one day during each month—

	1852.	1853.		1852.	1853.
January	5·4	10·8	July	3·6	12·6
February	5·4	11·7	August	9·0	9·0
March	8·1	7·2	September	9·0	5·4
April	3·6	8·1	October	7·2	7·2
May	9·0	10·8	November	5·4	9·0
June	5·4	14·4	December	5·4	5·4

15. Extreme range of temperature observed during each season—

	1852.	1853.
Winter	27·0	25·2
Spring	25·2	23·4
Summer	22·5	33·3
Autumn	20·7	26·1

The mean annual range is therefore nearly 5° below that of Malaga, which Dr. Francis[*] says is "many degrees less than that of any other place on the Continent of which we possess records, the range at Pau being 68°, at Rome 62°, and at Nice 60°." Madeira, however, surpasses both places in this respect, its range being only 31°.

On comparing the figures under 1852 and 1853, the influence of the hours at which the observations are taken becomes very evident, those for 1853 being more nearly the hours of the maxima and minima than those for 1852.

I have only calculated the mean successive daily ranges for 1853, and these I now subjoin.

16. Mean successive daily range for each month, viz.—

	Degrees Far.		Degrees Far.
January	1·68	July	2·34
February	2·52	August	1·75
March	1·90	September	1·62
April	1·70	October	1·48
May	1·85	November	1·44
June	2·79	December	1·26

17. Mean successive daily range for the seasons, viz.—

Winter	1·46	Summer	2·66
Spring	2·04	Autumn	1·61

18. Mean successive daily range for the year, 1·94° F.

Under this aspect, Algiers seems to be superior to every other place whose meteorology we know, except Madeira, whose mean successive daily range for the year is 1·11.[†]

The maximum range between any two succeeding days is 7·2, and this occurs three times during the year, while it very frequently happens that many days in succession have the same mean temperature.

Compared, then, with other points on the Mediterranean, Algiers has a warmer and a less varying climate than Marseilles,* Nice,* Genoa,* and Naples,* while it more nearly approaches, but is still superior to, Malta,† Corfu,† and Gibraltar.*

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[*] Francis on Climate, 174.

[†] Sir J. Clark on Climate.

Better to compare it with other points in Algeria itself, on the coast and in the interior, I annex a table. This table suggests many considerations of interest, but I shall confine my remarks to Oran, which is held in high repute by many, and I think deservedly.

Mean Temperature of different Points of Algeria.

	On the Coast.					In the Interior.								
	Algiers.[1]	Oran.[2]	Mostaganem.[3]	Bone.[4]	Bougie.[5]	Blidah.[6]	Medeah.[7]	Milianah.[8]	Orleansville.[9]	Tlemcen.[10]	Maskara.[11]	Constantine.[12]	Setif.[13]	Laghouat.[14]
Jan.	59·18	49·64	57·56	52·16		53·60			50·72	57·2	48·2	51·26		50·72
Feb.	59·01	52·16	59·90	60·44		48·20			47·48	62·6	51·8	49·46		60·08
Mar.	60·05	57·86	63·50	62·06		55.94			54·86	62·6	53·6	45·14		
April	64·06	59·54	69·08	65·30		60·26			60·08	69·8	64·4	51·26		
May	69·75	65·30	73·22	75·20		67·46			71·60	78·8	71·6	65·84		70·34
June	75·13	70·70	78·62	83·12		75·74			80·06	84·2	73·4	76·46		83·30
July	80·41	76·10	83·66	86·18		81·14			87·44	98·6	78·8	83·30		93·20
Aug.	82·07	77·36	85·10	86·18		81·14			89·24	95·0	83·3	79·70		91·04
Sept.	78·86	72·14	81·88	79·88		72·86			75·56	84·2	69·8	76·28		78·08
Oct.	73·85	68·00	75·56	79·34		66·20			67·46	78·8	69·3	65·84		68·90
Nov.	66·40	56·39	62·42	63·12		52·34			50·90	75·2	68·9	60·44		57·92
Dec.	60·82	50·54	56·48	60·80		51·80			51·20	59·0	64·9	50·36		49·10
Annual.	69·13	62·98	70·58	71·14	62·6	63·87	57·2	59·0	65·55	75·5	66·5	62·94	62·6	...
Feet above level of sea.	...	164	...	...	...	...	3017	2952	...	984	1312	2132	3608	...

AUTHORITY, AND OTHER REMARKS.

[1] Calculated by Dr. Mitchell from twelve years' observations.

[2] L'Echo d'Oran. For eight years, from 1841 to 1848.

[3] Moniteur Algérien, for 1854.

[4] Annal. d'Hygiène, vol. xli. Boudin, for 1841.

[5] Boudin: Carte Météorol.

[6] Observations made at the Civil and Military Hospital, 1851-2, furnished by Dr. Laveran to Dr. Mitchell, by whom arranged.

[7] Boudin: Carte Météorol.

[8] Boudin. Carte Météorol.

[9] Bérby: Mém. de Méd. Mil. vol. xii. 1851-2.

[10] Catteloup: Mém. de Méd. Mil. vol. xii. 187.

[11] Haspel: Mém. de Méd. Mil. vol. viii. 93, for 1849.

[12] Boudin: Annal. d'Hygiène, vol. xli. for 1838.

[13] Boudin: Carte Météorol.

[14] Given to Dr. Mitchell by Dr. Bertherand, for 1854-5. Laghouat is an oasis in the desert.

Like Algiers, Oran is very beautifully situated, and has excellent hotel accommodation.

The mean monthly and annual temperatures will be seen in the foregoing table, calculated from observations made during eight years. They are uniformly somewhat below those for Algiers.

The mean temperature for the seasons is as follows:—

Winter	52·19	Summer	70·70
Spring	56·52	Autumn	72·50

The mean daily range for the same period is 14·04, and this is about equal in the different seasons.

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* Sir J. Clark, op. cit.

† Col. James, Abs. of Met. Obs.

Hail has been observed to fall four times a-year at Oran, and snow once every two years; while at Algiers snow has only been observed to fall once in seven years.

The extreme annual range is much higher than at Algiers, being for 1847 and 1848, from the records of self-registering thermometers, 63·0°.

The range of temperature, however, for some places in the interior is much greater. For instance, at Laghouat, situated on an oasis in the desert, the range between the mid-day *mean* for July and the nine A.M. *mean* for December, is no less than 73·8°. The observations for this place are now for the first time published.

Here, as along the whole coast of Algeria, the heat of summer is tempered by the sea breeze, which rises every day about eleven A.M.*

As bearing still further on the question of temperature, I have extracted the following facts from M. G. Aimé's work in the "Exploration Scientifique d'Algérie."

The Temperature of the Mediterranean at a distance of 100 *to* 2000 *Metres outside the port of Algiers, compared with the Temperature of the Air, from Observations made between* 1840 *and* 1845; *Averages calculated for the Seasons.*

	Sea.		Air.		Difference.
Winter	57·92	...	54·32	...	+ 3·60
Spring	59·90	...	61·34	...	— 1·44
Summer	71·96	...	73·40	...	— 1·44
Autumn	69·08	...	68·00	...	+ 1·08
Annual Averages . .	64·71		64·26		

In winter and autumn, therefore, the surface temperature of the Mediterranean off Algiers is higher than the temperature of the atmosphere, but lower in spring and summer.

In the waters of the Mediterranean outside the port of Algiers, the diurnal variations cease at a depth of about 60 feet, and the annual variations at a depth of 380 to 440 yards. Observations made in France and Belgium show that the diurnal variations of temperature of the crust of the earth cease at a depth of rather more than 4 feet, and the annual variations about 80 feet.

Near the shores of the Mediterranean, the surface temperature is higher than out in the "*open sea*" during the day, but sometimes lower during night; whereas on the shores of the ocean the surface temperature is lower than "*out at sea.*"

All the observations made while the sea was rough were excluded, and the instruments employed were most perfect in their construction.

Barometer.—For the information which I possess on this point, I am indebted to nearly the same sources as in the case of the thermometer. When otherwise, I shall state it.

The following table is calculated from upwards of 11,000 observations, all of which are reduced to 32°, but no correction for altitude is made, as the cisterns of the various instruments employed were at so small an elevation above the level of the sea.

The slight variations in the pressure of the atmosphere at Algiers, and the absence of either abrupt or great changes, give indication of the tropical feature which the climate possesses.

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* Obs. published in the Echo d'Oran, No. 382.

Mean Barometric Altitudes.

Months.	Averages for years 1839-40-41.	Averages for years 1842-3-4.	Averages for years 1845-6-7.	Averages for years 1852-3-4.	Averages for twelve years, being upwards of 11,000 observations.	Difference of successive months.
January .	30·039	30·078	30·066	29·998	30·045	— ·043
February .	30·019	30·199	30·070	29·866	30·039	— ·006
March. .	30·039	29·976	30·080	29·975	30·018	— ·021
April . .	29·905	30·012	30·169	29·958	30·011	— ·007
May . .	30·051	29·984	30·160	29·840	30·009	— ·002
June . .	30·039	29·984	29·892	29·983	29·975	— ·034
July . .	30·062	30·023	29·960	29·939	29·996	+ ·021
August .	30·090	29·865	29·858	29·983	29·949	— ·047
September	30·078	29·973	30·046	30·008	30·026	+ ·077
October .	30·157	30·024	30·204	29·997	30·095	+ ·069
November	30·078	29·973	30·059	29·912	30·005	— ·090
December.	30·078	30·157	30·180	29·939	30·088	+ ·083
Triennial Averages	30·053	30·021	30·062	29·950	30·021	

The extreme annual ranges are small, as might be expected from an examination of the monthly averages. The mean difference of the successive months is only $\frac{1}{25}$th of an inch, while three times out of twelve it has to be reckoned in thousandths. Again, between the highest monthly mean and the lowest, there is only a difference of ·12, or a little more than $\frac{1}{10}$th of an inch.

During 1853, from the observations of M. Bourgét, the maximum height of the barometer observed occurred on the 1st of January, and was 30·237; while the minimum, observed on the 7th of February, was 29·331, giving a difference as the extreme annual range of 0·906.

The preceding year gives almost the same result. Its maximum was observed on the 15th and 16th of January, and was 30·394, and its minimum on the 1st of December was 29·331, showing an extreme range of 1·063. In other words, the difference between the highest observation recorded and the lowest during the year is, on average, less than an inch. And these instances of comparatively high pressure are exceptional, and very rare. In the two years examined, the minimum occurred only once each year, and the maximum once in 1853 and twice in 1852. For the latter year, 29·606 precedes that for the 1st of December among low observations, and is recorded six times during the year—twice in February and March, and once in January, November, and December; while 30·315 follows the maximum, but only occurs once in the year—on the 5th of February.

The extreme ranges observed during the month are of course also limited. I subjoin those for 1853, which, it will be observed, are greater in winter and spring than in summer and autumn.

January. . . .	·86	July	·29
February . . .	·59	August	·16
March	·70	September . . .	·39
April	·67	October	·39
May	·74	November . . .	·48
June	·43	December . . .	·51

It is generally believed that north winds determine the greatest elevation in Algiers of the mercurial column, and west winds the greatest depression.

The mean successive daily range is extremely low. Indeed, it frequently happens, and especially in summer, that the mean atmospheric pressure for eight days consecutively is represented by the same figure.

It has to be observed, however, that although the mercurial column rises and falls here within very restricted limits, yet there are changes, represented, it is true, by small measurements, which occur with a wonderful regularity and certainty—diurnal movements at fixed hours, and annual ones having reference to the position of the sun in the ecliptic. The first are more steady and unfailing than the second, but both unmistakeably show themselves.

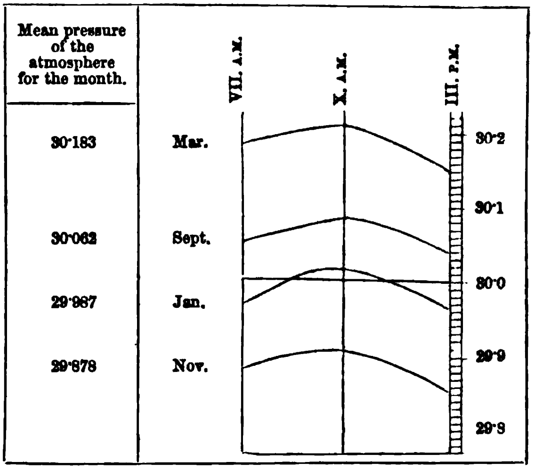
The existence of these diurnal tides at Nice and Genoa, on the opposite shore of the Mediterranean, has been established by Colonel Sykes, and the phenomenon is not less evident at Algiers. Thus, on examining the observations for 1854, hereafter given in full, which were made with extreme care and with an excellent instrument, by my friend Captain Humbert, we have the

Mean Descent of the Barometer, from 10 A.M. *to* 3 P.M.

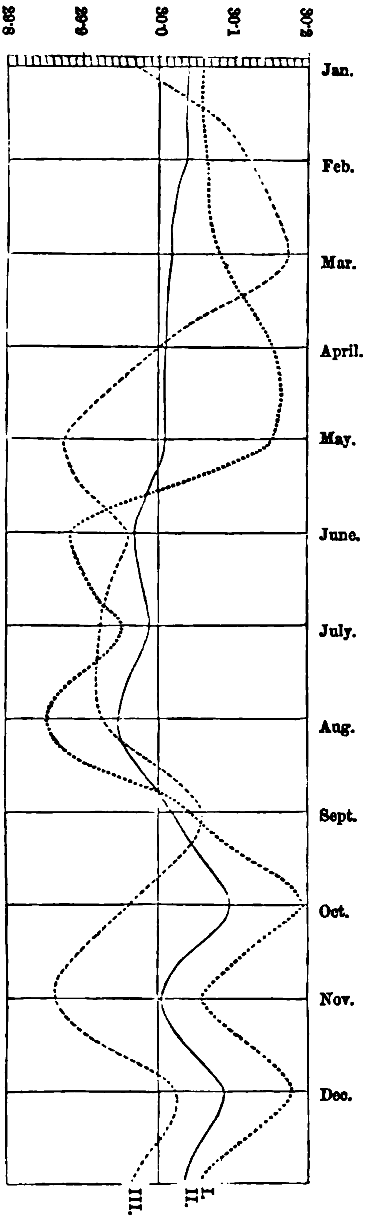
January . . .	— 0·028	July	— 0·033
February . .	— 0·043	August . . .	— 0·022
March . . .	— 0·056	September . .	— 0·042
April	— 0·017	October . .	— 0·064
May	— 0·044	November . .	— 0·052
June	— 0·009	December . .	— 0·044

I am not able to give the ascent to 9 from 3 P.M., but I have not a doubt about its being equally regular, from my own observations while there. Without the exception of a month, and I may add almost without the exception of a day, the pressure of the atmosphere is less at 3 o'clock in the afternoon than at 10 in the forenoon, and this has no reference to whether the column stands high, as in the cold, or low, as in the hot season. Thus, we have the

Mean Diurnal Barometric Curve between 7 A.M. *and* 3 P.M.



Annual Barometric Curves for Algiers.



From some observations which I obtained from Dr. Foley (by whom made I know not) during 1843, at the hours of 8, 9, 10, 12, 1, 2, 3, 4, and 5, which are reduced to 32°, carried to the fifth decimal place, and have every evidence of having been conducted with care by competent persons, I am led to conclude that the maximum occurs a little after 10, and the minimum a little after 4. From the averages of every month except one, 10 o'clock is above 9, and the minimum occurs nine times out of the twelve at 4, but the depression is carried on twice to 5.

When the sun is at the southern tropic, or during the winter months, we have the highest atmospheric pressure; and when in the northern, or during the summer months, we have the lowest. This annual tide is more imperfectly marked here than in India, but sufficiently so to make the general statement correct, as will be seen from the annexed diagram.

The curve No. 1 is constructed on the averages of the three years, 1845, 1846, and 1847, from observations very carefully made, and which give a mean annual barometric altitude of 30·062.

No. 2 is constructed on the averages of twelve years, including upwards of 11,000 observations, of which the mean annual is 30·020.

No. 3 is constructed on the averages of a single year (1854), from the very accurate observations of Captain Humbert.

It will at once be observed that while each curve varies, there is still, in general terms, a very apparent similarity—the evidence of some regular moving influence—going and coming, present at one season, absent at another, returning again, and so on.

The peculiar and sudden dip of

the three curves which takes place in November, has also been noticed as occurring at Genoa, by Colonel Sykes.

The barometric phenomena at Oran* have the same general features as at Algiers. The mean annual atmospheric pressure is 30·070, while the maximum altitude of the column recorded during eight years was 30·636, and the minimum 28·976, giving as the extreme range for that period 1·660.

Rain.—On this point I have obtained very full and satisfactory information, as the table on p. 209 will show.

The data for the first eight years are from observations by Monsieur Don, l'Ingénieur en chef du service des desséchements, and were published, I think, in the "Moniteur Algérien," No. 731; those for the next three are from the records of the "Direction du Port," the next four from the observations of M. Bourgét, and the last from those of Captain Humbert. The orifices of the instruments used by the different observers were from 70 to 150 feet above the level of the sea, but Captain Humbert has this year (1855) erected one on the top of the Casbah, about 400 feet above the sea level, which I have no doubt will give a different result from that at the Arsenal d'Artillerie, which is 330 feet lower.

There is a considerable range in the results for the different years, yet the average for any four consecutive years does not vary much from that for any other four; and the first eight years give almost the same mean as the last eight. 1849 and 1852 were unusually small, and 1848 unusually great; indeed, more than 2½ inches fell in 1848 for every inch that fell in 1849, but this great excess occurred in January and February, which together gave more than 30 inches, while the same months of the other year only gave 3. Indeed, a striking difference between the corresponding months of different years is often observed. Thus, 14 inches of rain fall in one December, while less than an inch falls in another; 10 inches in one November, and not 1 in another; and 15 in the February of one year, while scarcely more than one-tenth falls in that of another. Very little rain falls in June, and July may be called rainless, and August nearly so. The regularity observed in the barometric and thermometric phenomena is not equally observed here, nor in fact is it ever so with those of the rain. Yet enough of uniformity prevails in the results to justify a general expectation, and this may be entertained with more confidence when it refers to seasons than when it refers to months, and to the year than to seasons.

It will be observed that about the same quantity of rain falls during the first, as during the last, three months of the year, and that it is more than double that which falls in the intervening six months.

Monsieur Don's method of illustrating the manner in which the rain is divided over the year is more striking, and I have arranged the means of the sixteen years accordingly, as below:—

Arranged in trimestrial periods, &c., according to M. Don's plan.

			Three-monthly.	Six-monthly.	Annual.
1st	„	Dec. to Feb. . .	17·34	25·13	36·18
2nd	„	Mar. to May . .	7·79		
3rd	„	June to Aug. . .	0·87	11·05	
4th	„	Sept. to Nov. . .	10·18		

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* L'Echo d'Oran, 382.

Table of the Quantity of Rain that has fallen in Algiers between 1838 *and* 1854, *or for a Period of Sixteen Years, with Monthly, Three Monthly, and Half Yearly Averages.*

The quantities are in inches and tenths of an inch.	1838.	1839.	1840.	1841.	1842.	1843.	1844.	1845.	1847.	1848.	1849.	1850.	1851.	1852.	1853.	1854.	Averages from 17 years' observations.			
																	Monthly.	3 monthly.	½ yearly.	Annual.
January	3·32	3·64	4·79	6·19	7·42	3·56	6·54	7·87	3·70	20·23	1·91	7·19	7·58	3·26	3·27	5·70	6·01	14·51	19·64	36·18
February	4·72	4·08	3·39	4·23	1·61	6·02	7·28	15·63	5·98	10·49	1·26	0·16	4·07	3·04	11·20	1·27	5·27			
March	2·86	2·71	3·96	0·53	2·83	3·51	3·43	4·33	6·37	6·07	3·86	0·70	3·13	0·78	3·44	3·09	3·23			
April	2·52	5·07	2·97	3·18	3·87	1·73	6·71	1·71	5·58	0·92	3·23	0·51	1·08	2·89	0·33	2·59	2·80	5·13		
May	0·16	0·86	0·57	0·66	3·50	0·39	3·49	4·37	2·79	2·77	0·59	1·83	0·02	0·89	2·33	2·99	1·76			
June	,,	,,	0·68	0·34	,,	0·15	,,	0·66	1·65	0·11	0·28	1·33	2·38	0·06	0·79	0·64	0·57			
July	,,	,,	,,	,,	,,	,,	0·04	,,	,,	,,	0·02	,,	,,	0·64	,,	0·02	0·05	1·50	16·54	
August	0·08	0·04	0·02	,,	1·71	,,	0·38	,,	1·37	,,	0·02	0·11	,,	,,	,,	0·26	0·25			
September......	1·69	0·39	1·96	,,	2·04	0·34	1·63	1·28	0·39	1·31	1·38	2·38	1·04	1·88	0·35	1·20	1·20			
October	2·72	3·30	1·86	3·05	3·90	1·54	3·19	1·02	4·40	5·01	1·22	8·76	7·63	1·78	1·88	6·48	3·61	15·04		
November......	1·79	6·94	2·48	5·47	6·94	10·97	0·87	0·81	7·90	8·69	0·43	10·03	9·78	2·29	5·93	4·64	5·37			
December	14·07	1·28	8·91	11·44	1·55	1·89	7·59	3·52	6·09	0·96	7·07	4·15	4·23	7·69	10·21	6·25	6·06			
Total	33·93	28·31	31·59	35·09	35·37	30·10	41·15	41·20	46·22	56·56	21·27	37·15	40·94	25·20	39·73	35·13				

Annual average...36·18

It would appear from this, as he remarks himself, that there is one period of 3 months very rainy, and one very dry, between two others of an intermediate character.

Or the year might be divided thus into a rainy and a dry season, each of six months.

November, December, January, February, March, April .	28·74	36·18 annual.
May, June, July, August, Sept., October	7·44	

And if we include October in the first period, and make it one of seven months, we shall have 8·9 of the rain of the year falling between October and April.

Having thus ascertained the mean annual fall of rain at Algiers, with the manner in which it is divided over the year, it becomes of interest to ascertain the number of days on which the rain falls. The following table answers the inquiry.

Number of the days of the year on which Rain falls at Algiers, from the observations made by M. Bourget, considering the day as a period of 24 hours, with the manner in which these are distributed over the different months.

Months.	1851.	1852.	1853.			
	No. of days.	No. of days.	No. of days.	Average year.		
January	16	9	14	13·0		72·0
February	14	13	23	16·7		
March	11	6	17	11·3		
April	5	9	2	5·3		
May	2	8	7	5·7	23·7	
June	4	1	6	3·7		
July	...	4	...	1·3		
August	...	...	...	...		
September	3	6	4	4·3		
October	14	4	8	8·7		
November	17	4	18	13·0		
December	11	7	20	12·6		
Average	95·6					

I regret that I had not the data for calculating these averages for a longer series of years.

If we arrange them in four seasons of three months, as Monsieur Don has done, or group them into two periods of six months as I have done, the number of rainy days for each season bears an approximative proportion to the number of inches of rain which falls. Before analysing further or commenting upon the facts involved in the preceding tables, I shall subjoin another, which is extracted from M. Don's observations, and shows the comparative frequency of rainy days and rainy nights during the year. From this table it follows, according to the distinguished observer himself, that the number of rainy nights and rainy days are in the proportion of 100 to 117, while the quantities of rain that fell during night and day have the proportion of 110 to 100.

On examining the table, I observe that the excess of rainy nights over rainy days occurs almost entirely between May and October, being in nearly equal proportions during the rest of the year.

Table showing the Number of Rainy Days and Rainy Nights during eight years, from the observations made by M. Don, with the Quantity of Rain that fell during Day and Night.

AVERAGE OF EIGHT YEARS, FROM 1838 TO 1845.

Months.	No. of days.	Quantity of rain in inches.	No. of nights.	Quantity of rain in inches.
January	9·50	2·43	8·12	2·99
February	7·25	2·54	7·50	3·33
March	6·50	1·62	4·75	1·52
April	6·00	1·70	4·87	1·77
May	4·00	0·74	3·00	1·00
June	1·75	0·15	0·50	0·08
July	0·12	0·01	...	...
August	1·50	0·27	0·25	0·01
September	2·87	0·47	2·25	0·69
October	4·62	1·55	3·12	0·88
November	4·37	2·17	5·12	2·36
December	7·62	2·81	8·62	3·47
Totals	56·10	16·46	48·10	18·10

It would appear, therefore, that the mean annual fall of rain at Algiers is high; exceeding that at Rome,[*] Florence,[*] and Constantinople[*] by 5 inches, that at Madeira[*] by 7 inches, that at Malta[†] and Nice[‡] by 12, and by the same quantity even that at London[*] and Undercliff,[*] while it is nearly equal to that at Manchester[*] and Glasgow.[*] The fall of rain, however, at Gibraltar[†] exceeds it by 10 inches, at Pau by 6, and at Penzance[*] by 8 inches. Comparing it, however, with other points on the same shore, and these not far removed, we observe a more singular fact:

Quantity of Rain which falls at different points on the South Shore of the Mediterranean.

At Oran	average of 9 years	—18·49 inches	on 56·05 days.	'Echo d'Oran.'
Algiers	average of 16 years	—36·18 ,,	,, 95·60 ,,	Don and others.
Mostaganem for 1854	...	19·77 ,,	,, 56·00 ,,	'Moniteur Algérien.'

We have thus the fall of rain at Algiers doubling that at two places situated westward on the same coast, and with nothing in their position or the physical configuration of their vicinities to account for such a marked difference. It has been observed, however (I do not remember by whom), that less rain falls in the province of Oran generally than in that of Algiers, and less in Algiers than in that of Constantine; or, in other words, that it decreases as you go from the east to the west of Algeria.

The rain is divided over the year very much in the same manner as at Madeira, Malta, Gibraltar, and Nice. In the resorts of the invalid in our own country, however, it is distributed very differently, being more equally spread over all the months; the year, therefore, has no such marked division into a rainy and a dry season. Rome, Montpellier, and Florence are intermediate, but rather approach, in this tropical feature, the climate of Algiers.

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[*] Sir J. Clark.

[†] Colonel James.

[‡] Watkins.

In pursuing the comparative analysis of these tables, one cannot but be struck with the fact that the quantity of rain which falls in the various places of which we have records, bears no proportion to the number of days on which it falls; and this involves the consideration of another aspect of the question, which has a very direct bearing on the interests of the invalid.

Every day was reckoned a rainy one at Algiers on which the smallest indication of a rise was observed in the pluviometer. A comparison, therefore, with other places whose records give a larger number can only err by being in their favour. Better to appreciate the comparison, I subjoin it in a tabular form.

Place.	Mean annual quantity of rain which falls.	Mean annual number of days on which it falls.
Algiers	36·18 inches.	95·6 inches.
Oran	18·49 "	36 "
Mostaganem	19·77 "	56 "
Malta†	24·44 "	75 "
Madeira*	29·23 "	70 "
Rome*	31·17 "	117 "
Pau‡	42·00 "	119 "
London*	24·80 "	178 "
Torquay*	28·20 "	132 "
Undercliff*	23·48 "	146 "

In other words, if Algiers had followed the proportion of London, rain would have fallen on 267 instead of 95 days. It must happen, therefore, that frequent small showers or drizzling rains fall in the one place more frequently than the other. Such must also be the case in the north of France,§ where 26 inches of rain are spread over 144 days. At any rate, in Algiers it is very much otherwise. I speak from my own experience and from the testimony of all whom I met there who had given attention to the subject. The rain falls in short, heavy showers, and appears, from considerations to which I shall afterwards allude, to result from the condensation of vapour in an upper stratum of air, probably from the meeting of currents of different temperatures saturated, or nearly so, with vapour. If they are saturated, condensation and precipitation must take place, since "the resulting tension of the vapour will always exceed the tension belonging to the resulting mean temperature." But even when such is not the case, at the first meeting of two such currents there is a temporary condensation—and clouds are formed—before the equilibrium is established, which are then re-dissolved, if they have not fallen as rain. That currents under such conditions do meet each other will be afterwards shown, when we are considering the phenomena of the wind.

Dr. Casimir Broussais‖ talks of the year opening at Algiers with "a sky pure and serene, and a mild temperature; clouds come from time to time to obscure the sun for some minutes, rarely for some hours, and more rarely still for some days; rains show themselves for *moments*, and are sometimes abundant and prolonged." Other writers talk of it in almost the same language.

The rain then generally falls in large drops—in heavy showers—which, if they last for any length of time, convert the streamlets into impassable torrents, which, a few hours after the rain has ceased, lapse again into their original smallness.

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* Sir J. Clark. † Colonel James. ‡ Dr. Taylor.

§ Hardy, Traités d'Agriculture, &c., de l'Algérie, 47.

‖ Mém. de Med. Mil. vol. lx.

The shower is scarcely over, when the invalid can leave his rooms for exercise in the open air. The streets and roads are already dry, and the sky cloudless, and the sun bright and cheering. This arises partly from the city and its suburbs being built on a slope, and partly from the nature of the soil, from the temporary cause of the shower, and from the dryness of the lower stratum of air. Indeed, I believe that no invalid who should go to Algiers will be confined to his apartments by the rain half-a-dozen days of a season's (six or seven months) residence there. For although, properly speaking, rain falls on an average on 95 days every year, yet speaking of the day as opposed to night, and so considered it most directly bears upon the invalid's comfort, it only falls on 56 days, and on by far the greater proportion of these for only an hour or two.

As a further proof that the falling of the rain has this feature, I have to notice the following instances of large falls of rain in short periods. Less marked ones are exceedingly frequent.

Large Falls of Rain in short periods.

	Date.	Time: hours.	Quantity in inches.	Authority.
ALGIERS.	1841.—1st and 2nd November	48	5·47	Mons. Don.
	1848.—10 A.M. 22nd to 1 A.M. 23rd Jan.	15	5·51	Mons. Bourget.
	1850.—28th to 30th October	48	8·85	Do.
	1852.—2nd December	24	4·51	Do.
	1853.—31st December	24	2·35	Do.
ORAN.	1842.—November	12	2·00	'L'Echo d'Oran.'
	1844.—April	16	2·24	Do.
	1845.—January	12	1·58	Do.
	1847.—November	3	1·00	Do.
	1847.—December	1	0·71	Do.
	1848.—October	2½	3·15	Do.

Wind.—Next in importance to the careful study of the temperature of a climate which is to be recommended to invalids, stands an inquiry into the direction of the prevailing winds, with the general characters imparted to them by local or other accidents. I have omitted no opportunity, therefore, of obtaining information on the subject as full and satisfactory as possible, and I am in possession of daily observations on this point, in reference to Algiers, over a period of nine years and a half, for which I have to thank my friends Dr. Foley and Captain Humbert. Those furnished to me by the former were made by the managers of the Port of Algiers, with a wind gauge about fifty feet above the level of the sea, and those by the latter were conducted under his own inspection, at the Arsenal d'Artillerie, the gauge being eighty feet above the level of the sea. I have condensed and arranged these observations in the way I thought would best aid the analysis of the question. I now proceed briefly to discuss the results, comparing them with similar observations obtained from official documents for the towns of Oran and Mostaganem.

As regards the order of their frequency, we observe that those winds which prevail most at Algiers are from the north-west, which indeed form nearly 30 per cent. of the whole observations made, while the sum of those from the northerly points constitutes nearly one-half. The subjoined table better illustrates this statement than any worded account.

TABLE I.

Table showing the Direction of the Wind at Algiers, from observations made once a-day, during eight years, from 1837 *to* 1844, *by the "Direction of the Port of Algiers."*

		Total number of Observations.	Proportion in 1000.		
6.	N.	156	54	North or Northerly	465
1.	NW.	832	285		
3.	NE.	369	126		
7.	S.	62	21	South or Southerly	126
5.	SW.	277	95		
8.	SE.	30	10		
4.	E.	298	102	East	102
2.	W.	485	166	West	166
	Calm or variable	413	141	Calm or variable	141

Those from the southerly points are much less frequent, not exceeding 12 per cent. of the whole, but of these, that from the south-west is much the more prevalent. South and south-east winds are rare, only occurring twice in every hundred observations. West winds blow oftener than east winds, and together they make about 25 per cent. of the observations which form the basis of the foregoing table; but I am inclined to think this proportion high, from a comparison with the results obtained by Captain Humbert at Algiers itself, and with the observations made at Oran and Mostaganem. As in all meteorological observations, there is a difficulty in recording those of the wind, and one observer may be easily understood to mark that as an east wind, which another would enter as east-north-east, and this the more readily if the tables are constructed, as is generally the case, only for the eight leading points.

It is important to know that, as a general statement of proportions, the above is not only true of the sum of the eight years, but, with slight variations, of the individual ones. And moreover, not only is it so of Algiers, but, as the following table will show, it holds good with other towns situated on the same coast under nearly identical conditions. Instead of repeating Table I. for the purposes of comparison, I give here another series of most careful observations for Algiers, by different observers and for another epoch.

TABLE II.

Direction of the Wind at Different Points of the Coast of Algeria, from official and other Documents.

	ALGIERS.		ORAN.		MOSTAGANEM.	
	Observations by Captain Humbert for the year 1854.		Published in the 'Echo d'Oran,' for eight years, 1841 to 1848.		Published in the 'Moniteur Algérien,' for 1854.	
	Proportion in 1000.		Proportion in 1000.		Proportion in 1000.	
N.	112	566	155	679	138	769
N.W.	404		315		386	
N.E.	50		209		245	
S.	56	330	57	262	14	87
S.W.	106		166		45	
S.E.	168		39		28	
E.	64	64	19	19	34	34
W.	40	40	40	40	110	110

It follows, from this comparison, that the north-west wind is invariably and by far the most frequent, and that those winds from the northerly points are always equal to, or surpass, the sum of all the others. The smaller proportion of winds directly east and west, given in Table II., I am inclined to think more nearly approaches the truth than that in Table I.

Some reliance may be placed on these results, as they are drawn from nearly 7000 observations, stretching over a period of 13½ years. And besides, they derive strong confirmation from the fact that they lead always to the same inference, however examined—in part or in whole—for any year or any aggregate of years.

But we are here led naturally to the important query—how are these winds divided over the year, and do they observe the same proportions to each other in all months?

A passing glance at the following table will show that they do not observe the same relations, but are unequally divided over the year; for, while northerly winds always have the absolute ascendancy, there are periods when their relative superiority is greatly reduced. On running the eye over the columns, it will be perceived that the north, north-west, north-east, and east winds are represented by smaller figures in the commencing and ending months of the year than they are in the central ones, while exactly the opposite holds with reference to winds from the south, south-east, south-west, and west, which show much lower figures in the central months than in the others.

Calms are more steadily prevalent in the central months, but the difference here is not very great.

Like the conclusions which preceded, these also are arrived at with equal certainty by the examination separately of any one of the years which enter into the following table. A uniformity is observed, which makes it evident that this results from an influence which is steady and persistent, and that probably it springs from something of a more general nature than local accident.

TABLE III., *a.*

The observations contained in TABLE I., *and those made by Captain Humbert, contained in* TABLE II., *arranged so as to show the winds that prevail in the different months.*

		N.	S.	E.	W.	N.E.	N.W.	S.E.	S.W.	Calm.
For a period of 9½ years.	January	11	6	4	79	11	78	7	48	35
	February	7	16	14	68	27	56	7	37	22
	March	12	5	33	53	34	91	12	26	13
	April	28	1	28	49	28	75	13	22	26
	May	12	1	23	43	39	98	6	21	36
	June	31	1	38	26	35	86	1	13	39
	July	27	2	56	16	53	79	...	12	34
	August	26	3	54	7	67	82	2	11	27
	September	8	3	22	17	43	101	8	21	46
	October	19	8	23	17	33	105	5	35	34
	November	10	13	17	57	15	67	12	36	43
	December	12	15	16	66	7	67	15	23	58

If we arrange the same table in a different form, the fact becomes still more evident.

TABLE III., *b*.

The same observations grouped differently.

	Northern points.	Southern points.	East.	West.	Calm.
January	99	51	4	79	35
February	90	61	14	68	22
March	137	43	33	53	13
April	131	36	32	49	26
May	149	28	23	43	36
June	152	15	38	26	39
July	159	14	56	16	34
August	175	16	54	7	27
September	153	32	22	17	46
October	157	48	23	17	34
November	92	61	17	57	43
December	88	53	16	66	58

TABLE III., *c*.

Or, otherwise arranged.

		Northerly.	Southerly.	East.	West.	Calm.
The observations of 9½ years grouped in periods of six months.	November, December, January, February, March, April	638	305	106	372	197
	May, June, July, August, September, October	944	153	216	126	216

It results from this, therefore, that southerly winds are twice, and west winds three times as frequent from November to April, as from May to October; while, on the other hand, east winds double their number from May to October, and the northerly add 50 per cent.

The importance of this as affecting the merits of Algiers as the resort of a class or classes of invalids, will be apparent when the physical properties of these different winds are being inquired into. In the meantime, I desire rather to elucidate the point in a meteorological aspect.

At Madeira, according to Heinecken, the winds have much the same order of frequency and the same distribution over the year. North winds are predominant throughout the year, but especially in summer.*

In Egypt, too, according to M. Martins, from May to October, "the winds constantly are from the north or north-west. In the winter, their direction is less constant; but the predominance of north winds is still very marked."†

Having observed in the elaborate account of Nice, by Colonel Sykes, that the winds from the northerly points of the compass also prevailed there greatly in excess of all others, I at first felt disposed to think that they crossed the Mediterranean and impinged against the shores of Northern Africa. On examining, however, the manner in which they are disposed over the different months of the year, I find that in this respect Nice is exactly in the position of Algiers reversed—northerly winds being more frequent from November to January than from May to October, and southerly more prevalent from May to October than from November to January. It is probable, therefore, that Colonel Sykes is correct in attributing these prevailing winds to the influence of local accidents, the cold dense air flowing from the top of the Alps into the warmer and lighter air of the basin of the Mediterranean.

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* Kæmitz's Meteorology, page 47.

† Kæmitz, page 46.

At Malta, however, which lies about 1° further south than Algiers, and about 8° further south than Nice, from the observations* made under the direction of Colonel Thompson, R.E., it appears that the winds are nearly in the same proportion to each other as at Algiers, and are distributed over the year much in the same manner.

At Gibraltar, again, which lies almost in the same latitude as Algiers, but about which there are many circumstances in respect of its locality to modify and impart peculiarities to its meteorology, I find† that the winds from the southerly points of the compass almost equal those from the northerly. But on examining their relation as to periods of the year, that place at once assumes a parallel with Nice. These facts I shall now subjoin in a tabular form, that they may become more palpable and more readily understood:

TABLE IV., *a*.

Direction of the Wind at Various Places.

	ALGIERS.		MALTA.		GIBRALTAR.		NICE.		
	Proportion in 1000.		Proportion in 1000.		Proportion in 1000.		Proportion in 1000.		
N. ...	112	566	64	533	37	476	205	366	269 calms.
N.W.	404		289		402		85		
N.E..	50		180		37		76		
S. ...	56	330	53	331	63	474	31	238	
S.W..	106		157		282		145		
S.E. .	168		121		129		62		
E. ...	64	64	63	63	44	44	104	104	
W....	40	40	73	73	6	6	23	23	

I shall now show, in a tabular form, how these winds are distributed over the two seasons, from May to October and from November to April, as in Table III., *c*.

TABLE IV., *b*.

		Northerly.	Southerly.	East.	West.	Calm.
Algiers.	November, December, January, February, March, April	195	93	32	114	60
	May, June, July, August, September, October	288	47	66	39	66
Malta.	November, December, January, February, March, April	219	214	20	37	—
	May, June, July, August, September, October	314	117	53	26	—
Gibraltar.	November, December, January, February, March, April	275	192	1	28	—
	May, June, July, August, September, October	202	282	4	16	—

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* Abstract of Met. Obs., 1853-4, by Col. James.

† Ibid.

		Northerly.	Southerly.	East.	West.	Calm.
Nice.	November, December, January, February, March, April	219	104	60	15	100
	May, June, July, August, September, October	147	135	43	8	169
Mostaganem.	November, December, January, February, March, April	336	78	23	48	—
	May, June, July, August, September, October	428	14	11	62	—

The Mediterranean and its shores, therefore, have their monsoons. The dependance of these winds upon the seasons was known to the ancients, as is proved by their calling them *Etesian*[*] and *Ornithian*.[†]

The northerly winds of summer are probably nothing more than the lower trade winds, wanting, of course, in the steadiness and regularity which these winds have when observed on the ocean. Thin white clouds, floating rapidly from the south, often show the existence of an upper and opposed current—the S.W. trades. Bruce in his 'Travels' observes this, and it is a common phenomenon. Thomson talks of this upper stratum as being charged with moisture; and he is probably correct, if it be, as I suppose, the S.W. trades.

As a general statement, the zone of the N.E. trades is said to extend between 7° and 29° north latitude; but as the whole system of zones moves with the sun, the belt of these N.E. trades will in July and August lie between 12° and 34°, and in March and April between 2° and 24° north latitude.[‡] This would place Algiers on the southern confines of the calm belt of Cancer in summer, and a little beyond the northern in winter. In other words, this belt would oscillate over Algiers, and this, if correct, would never bring it within the region of N.E. trades. But it appears to me that these zones are rather related to the thermal equator than to the terrestrial, and ought therefore to follow its bendings. For a considerable distance westward of the longitude of Greenwich, this would push the whole system northward some 10° or 15°; and we should then have Algiers during summer fairly within the zone of the N.E. trades, which at this longitude would now lie between 20° and 44° north latitude during July and August. In winter, again, it would lie on the edge of the belt of calms—where irregular winds and rain prevail—in consequence of the zone of trades retreating some 10° towards the equator.

"All navigators know," says Kæmitz, "that the passage from Europe to Africa is much quicker in summer than the return,"[§]—the difference being, according to Martins, one-fourth for a sailing vessel, and one-tenth for a steamer.

A great proportion of the northerly winds of winter may have the same origin, and it is probable that the southerly winds of this season belong to the S.W. trades. It is in the latitude of Algiers that during winter the upper S.W. current meets the upper N.E.; and both dip towards the earth, and become lower currents,[||] pursuing each its original direction, the one to the pole and the other to the equator. When such a meeting takes place a great uncertainty must prevail.

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[*] Kæmitz's Meteorology, 45.

[†] Thomson's Meteorology, 338.

[‡] Lieut. Maury.

[§] Kæmitz's Meteorology, 46.

[||] Lieut. Maury.

Although the great aerial currents, taken as a whole, flow as manifestly and steadily in one direction as rivers in their courses, yet there are constantly recurring local causes which produce eddies and bendings in the course of the one as in the course of the other. And perhaps in Algeria these are as apparent as anywhere. The relation of the great basin of the desert to that of the Mediterranean, is sufficient to produce serious disturbances in the general current; and such, without doubt, it does. And both north and south winds at times have this more local origin.

The *cold* and *dry* northerly wind, of which M. Hardy speaks,* is the N.E. trade, which in fact is nothing but a returning S.W. trade, from which the last particles of moisture have been expressed by the colds of the Polar regions. It usually reaches Algiers, however, after having taken up some moisture from the Mediterranean. When it blows with violence or for any length of time, M. Hardy* says that it injures vegetable life, paralysing the side of the tree against which it directly and usually strikes.

There is another dry wind, but this is a hot one—the sirocco. This has probably the local origin to which reference has just been made. Its lethal effects on vegetable life are well known, and can hardly be overstated. But fortunately it is rarely felt at Algiers, even in summer, and very rarely indeed in winter and spring.† When it does occur in winter it is cooled down somewhat in its passage over the snows of the Atlas, and at the same time receives an amount of moisture which greatly modifies its unpleasant effects.

The winds which are thought to bring rain are the S.W.—probably the trades, which are charged with moisture which they have drawn from the waters of the southern hemisphere. At Malta and Gibraltar, 721 northerly winds give 93 rainy days, while 588 southerly give 95. But even when there is a N.E. or N.W. current indicated on the surface, there may be a condensation in the upper southerly current, producing the rain.

Humidity.—I have no observations which directly bear upon the hygrometry of Algiers beyond those which I made myself during the months of March, April, May, and June, 1855. I employed a Regnault's hygrometer, and made the observations at 10 A.M., 4 P.M., and 10 P.M., under circumstances to secure trustworthy results, and at the same hours I noted also the readings of the wet and dry ball thermometers.

Monsieur M. E. Millon had records of a number of observations made with Saussure's hygrometer, but I did not obtain these, partly from the unsatisfactory character of the instrument used, and partly, because being conducted with a special object, their results did not truthfully apply to the climate generally.

The atmosphere, during the time alluded to, was drier or farther removed from saturation by 1° at 4 P.M., than at 10 o'clock in the forenoon or at 10 in the evening. Between these last hours there was almost no difference. Over the three observations for the whole period, the average depression of temperature which occurred before the deposition of dew was 8·39° F. On all occasions the temperature of the air was *at least* 4° above the dew point, so that at the hours of observation dew was never falling. The depression may be said to have ranged between 4° and 15°. On one occasion, when the sirocco was blowing, it was as high as 22°.

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* Recueil de Traités d'Agriculture, 42.

† During 1845, it was only observed twelve times, according to Casimir Broussais, Mém. de Méd. Mil. vol. lx.

The climate of Algiers must be considered, I think, dry and bracing. Certainly during the months of the above observations it was so; but other facts establish the same conclusion.

Thus at Oran, which has a climate so similar to that of Algiers, the quantity of water evaporated during 1854 was 57·63 inches, being more than three times that which fell as rain.* In England, according to Thomson, the rain is in excess.

Again, I frequently observed myself that evaporation went on almost till the commencement of the shower, and the rain had no sooner ceased to fall, than it was resumed with briskness. And yet in taking exercise, although from the warmth of the atmosphere perspiration was free, it was so rapidly and freely removed that no oppression was felt. Indeed, the climate was always spoken of as *bracing*, and never as *relaxing* in its properties. It was sometimes called moist by those who argued from the quantity of rain which falls annually, but this rain is seldom the condensation of vapour in the stratum of air next the earth but in one above, and occurs in short heavy showers. Thus, quoting again from Le Docteur Martin, "En Algérie un nuage vient, il se juge de suite. Le soleil le dissipe, on bien il tombe *comme en masse;*" and again, "Sitot que les larges gouttes de cette pluie touchent le sol, elles sont renvoyées en vapeur dans l'air."

The effect of these showers is not altogether refreshing. They certainly cool the air by abstracting its heat for their reconversion into vapour; but in thus lowering the temperature and at the same time adding vapour, they bring it nearer the point of saturation, and so render it, if anything, more oppressive.

As a general rule the merits of a dry atmosphere are superior to those of a moist one. It is obvious that man was not intended to live in an atmosphere *saturated* with vapour, since, if so constituted, it would be unable to carry off the aqueous exhalations for which it is palpably the intended medium. A perfectly dry atmosphere—whether cold and dry, or hot and dry—would only be less objectionable. We have seen that it is prejudicial to vegetable life. It would be to the skin what a purgative is to the mucous membrane of the intestines, and in its stimulating action would subject it to an increasing and exhausting drain. Nor would this "simply involve the drinking of a pint or more of extra fluid," according to Dr. Forbes Watson, since the function of the skin being complex or manifold, it would not be stimulated to hyperaction in one direction and remain unaffected in the others.

Dews fall very rarely in the evening at Algiers itself during winter and spring. They are much more frequent, however, in the hot season. In the valleys they occur at all periods of the year.

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* Moniteur Algérien.

Fogs are of more frequent occurrence also in the hot season than in the cold. They are never dense, and those that involve Algiers come from the Mediterranean. At eight or nine o'clock in the morning the plain of the Metidja is very often to be seen covered with a thin haze, which, however, is soon dissipated.

State of the Sky, Light, &c.—The sky of North Africa rivals, if it does not excel, that of Italy, and stands beyond comparison with that of the North of Europe or our own country. "Pouvons nous," asks Dr. Martin, "comparer celui du Nord de l'Europe avec le bleu intense, et si admirablement limpide, du ciel d'Afrique?" There is a health-giving influence in a bright atmosphere and a cloudless sky which is not fully appreciated. Light has a higher power on the functions of the animal economy than we are apt to think; and proofs are not wanting.

Deprive the tadpole of its influence, and nourish it as you like, it remains a tadpole still.* This agent is essential to its development, and it is arrested when deprived of it. Again, disease among the soldiers who lived on the dark side of an extensive barracks at St. Petersburg,† was uniformly in the proportion of three to one, compared with that on the side exposed to a strong light.

But it is in the vegetable kingdom that we have the clearest manifestations of its workings. In plants we find the secretions developed "in greater perfection according to its intensity."‡ Deprived of it, we find them flowerless, fruitless, and with small and stunted leaves, while on branches of the same plant "which grow towards the light we have full-sized leaves, and perfect flowers and fruit."§ Had we no other proof, we should be authorised in inferring that that which is so potent on vegetable is not inert on animal life. The physiology of the two kingdoms is ever more or less closely related. And that which stimulates the flower to expand its petals—giving a welcome as it were to the vivifying influence—is also, though perhaps more obscurely, a stimulus to man. But the operation of no stimulus must be continued or uninterrupted. And that of light is no exception. Hence darkness and light alternate. The state of protracted day in the polar regions is described as telling injuriously on health, and as being painful and exhausting to those who endure it. During night the plant is not exactly what it is during the day—it does not perform the same functions, or if it does, it is with less vigour; or, perhaps, one set of functions alternates with and relieves another—but in any case, it is to it a period of repose. And is it not also probable that man and animals enjoy the most perfect and natural repose during darkness?

Every man has experienced the gayness and brightness of spirits which a clear sunny day produced, and no man who has known the horrors of a London fog will be unable to paint the reverse picture. But it is a query if this bright mental atmosphere, which comes with a bright physical one, is not the direct result of its stimulating action on us simply as animals. Life within us is intensified, and the mens sana is tinged with the impressions of the corpus sanum. In other words, it would appear as if some actual physical change is induced, or that the powers of life are exalted by light, which surely is the case when it "protects the plant from the effects of low temperatures,"* as it has been proved to do.

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* Milne Edwards in Ward on Close Cases, p. 112.

† Ward op. cit. p. 113.

‡ Ward op. cit. p. 6.

§ Ward op. cit. p. 6.

The intensity of light at different places differs greatly, and it is a matter of regret that there is no proper means of measuring it, as the results would certainly prove interesting. According to Sir J. W. Herschel,† that of the Cape of Good Hope compared with that of a bright summer's day in England, is as 44° to 17°.

I shall conclude the consideration of this question with the following table, which I extracted from the records of what may be called the log-book of the port of Algiers. I believe that the days marked "serene and clear" were almost absolutely so.

The columns giving the state of the sea in the bay are not without interest.

1844.	SKY.			SEA, In the Bay of Algiers.		
Observations made three times daily.	Serene and clear.	Cloudy and overcast, with or without rain.	Foggy.	Calm.	Rough.	Stormy.
January..............	48	45	...	35	39	19
February	57	30	...	34	32	21
March	68	21	4	34	48	11
April...............	38	48	4	31	50	9
May	61	26	6	65	28	...
June	83	6	1	64	26	...
July	88	5	...	63	30	...
August	71	18	4	45	41	7
September.........	47	40	3	81	9	...
October............	68	25	....	42	51	...
November.........	60	29	1	69	21	...
December	45	48	...	32	45	16
Total	734	341	23	595	420	83
1000 observations give...............	668·5	310·6	20·9	541·9	382·5	75·6

Ozone.—During the greater part of my residence in Algiers, I recorded three ozonometric observations daily, and for two months Captain Humbert also recorded two. The combined results of these I shall briefly discuss.

The total number of observations was 209, and the average indication 5·5° of Schönbein's scale. The papers exposed during the night, gave a higher average by 1·5° than those exposed during the day; or, since the night exposures were on an average five hours longer than those by day, perhaps the more legitimate deduction is, that the length of time during which the paper was exposed to the air influenced the degree of the indication.

The direction of the wind did not very obviously affect the result. The averages, however, from a due east or due west wind were 1° higher than when the wind blew from any of the northerly or southerly points, between which there was no difference.

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* Ward op. cit. 104.

Between Captain Humbert's observations and mine for the same day and the same hour there was often all the difference that could exist—the difference between 10° and 0°; and they seldom exactly agreed. Our places of observation were not far separated, but mine was 80 feet higher than his.

Light did not affect the papers, since some of them, sealed in a glass tube and exposed for months to its influence, were unchanged. The same result followed a similar exposure of some of the solution of the starch and iodide of potassium.

On the whole, however, I found ozonometric observations, as at present conducted by Schönbein's papers and scale, unsatisfactory.

It often happened that on comparing the tints, I myself assigned to them one place, while another person placed them a degree above, and a third a degree below; and occasionally a greater diversity even than this occurred, amounting to 30 per cent. One might almost have anticipated this; yet where such can happen there must be an absence of precision that practically nullifies the results.

But there is another aspect under which the uncertainty of the present mode becomes still more apparent.

It is clear that if the paper be exposed during a perfect calm, it will be acted on only by the ozone in the little atmosphere which immediately surrounds it, and this may only be in quantity to give a very feeble indication, or perhaps none at all. If the same air be put in motion—in other words, if there be wind, the ozone of many times that quantity of air will impinge upon the paper, and exert its characteristic action; a higher tint will thus be produced. Increase the motion still further, or suppose a strong wind blowing, and, as the consequence, the quantity of ozone brought into contact with the paper is greater, and the indication is increased. Yet, as concerns the proportion of ozone contained in it, in each case the air is the same.

This fallacy I proved in many ways. During a walk along the shore, I frequently attached a piece of the paper to the edge of my hat, so as to be freely exposed to the wind, and if this were strong I often had in the course of an hour a high indication; while another paper, so attached as to be simply protected by the body of the hat from the direct action of the wind, remained unaffected. In travelling by diligence I often made a similar experiment, and, for some time I made them regularly on the terrace of the hotel. The result was uniform—the paper sheltered from the wind was always below that exposed to its direct action, and the difference was in proportion to the completeness of the shelter.

An ozonometer, therefore, appears to be still a desideratum; and we must have an instrument which, besides having a scale more definite than the tints of Schönbein, will cause the paper to be acted on by a fixed quantity of air during a fixed period of time.

I constructed, while there, a small chamber with a diaphragm, in the aperture of which was fitted for each experiment a bit of the prepared paper, and the air was drawn through it by means of the aspirator used for Regnault's hygrometer. As this was only capable of drawing through a couple of cubic feet of air, which never sufficed to produce any effect, the process became tedious. This difficulty, however, might be overcome

Dr. Humbert's *Meteorological Observations, made at Algiers during the year* 1854, *with means for every ten days, for each month, and for the year.*

Months.	Dates.	Barometer.					Thermometer.					Rain.
		Mean height of the barometer at the temperature of 32°, the cistern being 54 feet above the level of the sea.			Mean range from 10 A.M. to 3 P.M.	Means of the three observations for each month.	Mean temperature of the air, the thermometer being in the shade, and looking to the north.				Mean difference between temperature at 7 A.M. and 3 P.M.	Quantity of rain which fell each month, in inches.
		7 A.M.	10 A.M.	3 P.M.			7 A.M.	10 A.M.	3 P.M.	Means of 3 obs.		
Jan. ...	1 to 10	29·795	29·811	29·764			52·34	57·20	60·44			
	11 to 20	29·787	29·820	29·814			53·24	55·94	56·66			
	21 to 31	30·318	30·350	30·291			52·52	57·56	58·82			
	1 to 31	**29·979**	**30·005**	**29·977**	**0·028**	**29·987**	**52·70**	**56·84**	**58·64**	**56·06**	**5·94**	5·700
Feb. ...	1 to 10	30·176	30·198	30·142			52·70	59·18	60·98			
	11 to 20	29·990	29·997	29·933			44·96	48·74	50·00			
	21 to 28	30·241	30·259	30·231			49·82	54·14	56·66			
	1 to 28	**30·136**	**30·139**	**30·096**	**0·043**	**30·124**	**49·10**	**53·56**	**56·12**	**53·06**	**7·02**	1·267
March.	1 to 10	30·422	30·442	30·384			48·74	53·24	55·94			
	11 to 20	30·127	30·136	30·104			53·96	58·64	60·62			
	21 to 31	30·040	30·060	30·010			54·86	56·48	60·26			
	1 to 31	**30·191**	**30·207**	**30·151**	**0·056**	**30·183**	**52·52**	**56·12**	**59·00**	**55·88**	**6·48**	3·094
April .	1 to 10	30·223	30·223	30·206			55·22	59·54	62·78			
	11 to 20	29·966	29·966	29·912			62·78	66·92	67·10			
	21 to 30	29·826	29·845	29·836			59·90	64·40	65·66			
	1 to 30	**30·005**	**30·005**	**29·988**	**0·017**	**29·999**	**59·36**	**63·68**	**65·12**	**62·72**	**5·76**	2·594
May ...	1 to 10	29·888	29·910	29·859			65·12	69·62	70·34			
	11 to 20	29·856	29·871	29·851			61·52	65·30	67·82			
	21 to 31	29·908	29·928	29·864			68·00	72·14	74·66			
	1 to 31	**29·886**	**29·903**	**29·859**	**0·044**	**29·883**	**64·94**	**68·90**	**70·88**	**68·24**	**5·94**	2·995

June...	1 to 10	29·915	29·920	29·914			68·14	70·16	72·32			
	11 to 20	29·953	29·970	29·963			71·06	76·28	77·54			
	21 to 30	30·031	30·030	30·013			71·96	75·56	78·98			
	1 to 30	**29·966**	**29·973**	**29·964**	**0·009**	**29·988**	**70·34**	**73·94**	**76·10**	**73·46**	**5·76**	0·637
July ...	1 to 10	29·924	29·922	29·907			74·30	75·92	79·52			
	11 to 20	29·952	29·964	29·916			74·84	76·82	81·86			
	21 to 31	29·984	29·994	29·963			76·28	79·70	85·10			
	1 to 31	**29·954**	**29·962**	**29·929**	**0·033**	**29·948**	**75·02**	**77·54**	**82·22**	**78·26**	**7·20**	0·027
August	1 to 10	29·887	29·851	29·867			80·78	82·76	84·92			
	11 to 20	29·961	29·987	29·972			77·00	81·14	83·12			
	21 to 31	30·042	30·054	29·995			74·66	78·62	82·58			
	1 to 31	**29·940**	**29·966**	**29·944**	**0·022**	**29·948**	**77·36**	**81·14**	**83·48**	**80·66**	**6·12**	0·259
Sept. .	1 to 10	30·000	30·038	30·003			73·94	78·08	80·60			
	11 to 20	30·072	30·104	30·075			73·40	77 18	80·42			
	21 to 30	30·083	30·112	30·053			68·18	72·86	75·56			
	1 to 30	**30·055**	**30·086**	**30·044**	**0·042**	**30·062**	**71·78**	**75·92**	**79·16**	**75·62**	**7·38**	1·204
Oct. ...	1 to 10	30·025	30·046	29·992			70·52	75·74	75·74			
	11 to 20	29·933	29·954	29·903			62·60	66·74	68·72			
	21 to 30	30·040	30·051	29·976			60·26	65·84	66·74			
	1 to 31	**30·002**	**30·018**	**29·954**	**0·064**	**29·991**	**64·40**	**69·44**	**70·52**	**68·12**	**6·12**	6·479
Nov....	1 to 10	30·131	30·144	30·076			56·66	60·80	61·34			
	11 to 20	29·809	29·822	29·752			53·78	58·46	59·18			
	21 to 30	29·711	29·736	29·733			52·34	56·30	57·38			
	1 to 30	**29·884**	**29·901**	**29·849**	**0·052**	**29·878**	**54·32**	**58·46**	**59·18**	**57·32**	**4·86**	4·641
Dec. ...	1 to 10	29·951	29·971	29·863			50·90	54·54	55·04			
	11 to 20	30·031	30·071	30·036			49·10	52·52	53·60			
	21 to 31	30·175	30·197	30·172			51·26	52·52	53·42			
	1 to 31	**30·056**	**30·083**	**30·039**	**0·044**	**30·062**	**50·36**	**53·24**	**53·96**	**52·52**	**3·60**	6·251
Means for the whole year		30·005	30·021	29·983	0·038	30·004	61·85	65·73	67·86	65·16	6·01	35·148 Total.

by using a small fan, with which the air could be drawn through at a fixed velocity, and the quantity might be determined by interposing a "dry gas meter" between the little chamber and the fan. Instead of prepared paper in such an instrument, I would suggest the use of prepared "tarlatan" muslin. A little management in the preparation would easily keep the meshes open, so as to permit the air to be drawn through it.

This still leaves the difficulty of the scale unsolved in any degree. As an attempt towards this, I thought of and tried the drawing a fixed amount of air through a solution of the starch and iodide of potassium, and having ten or twelve test tubes filled with different shades of blue with which to compare the tint produced, but this I found liable to many objections, and it proved unsatisfactory in my hands.

I give these details, because I think that ozone has recently acquired fresh importance. If it be a fact, as I understand it to be, that all oxygen which is set free without the agency of high temperatures, exists in the state of ozone, is it not probable that the oxygen set free by plants is in the same state?

In the above analysis of the meteorology of Algiers we have not a few indications of tropical features in its climate—the limited oscillations of the barometric column, with its annual and diurnal tides—the small range of the thermometer—the periodicity of the winds and rain, the short twilights, and the cloudless sky. These are but *indications*, however, for in the aggregate of its constituents it has much more of a temperate than a tropical character. The climate of Algiers, then, during winter and spring, may be said to vie with that of Madeira—being as warm and steady in temperature, but drier and more bracing. No climate, however, is perfect, and the invalid who seeks Algiers expecting to find nothing but uninterrupted serenity, will be disappointed. Bad weather occurs there, as everywhere; but, on the whole, figures and experience justify me in asserting that few climates are superior, and more likely to benefit that class of patients who seek for health in a more genial temperature and less cloudy atmosphere than our own.

I have been as full as possible in the meteorological details, and have endeavoured merely to be the interpreter of the facts and figures. There was nothing to bias my mind, and the observations were my *medical* bearing. To this part of the subject I propose to advert on a future occasion. With the exception of the first eight years of the rain table, so far as I know, none of these observations have yet been published. The arrangement and reduction of such a miscellaneous mass has proved somewhat tedious, but the deductions are more valuable than if they had been drawn from the records of more limited periods.

I conclude the subject with a complete Meteorological Table for 1854, from the observations of my friend, Captain Humbert.*

* This table is inserted at pp. 224 and 225.

PART FOURTH.

Chronicle of Medical Science.

ANNALS OF PHYSIOLOGY.

By **Hermann Weber**, M.D.,
Licentiate of the Royal College of Physicians, Physician to the German Hospital.

I. Food and Digestion.

1. **Von Ancum**: *On the presence of Iodine in the Drinking Waters of the Netherlands.* (Erdmann's Journ., vol. lxiii. p. 251; and Canst. Jahresber. f. 1854; Physiol. vl. p. 103.)
2. **Chatin**: *On the Iodine in the Air and Waters of Germany, Belgium, &c.* (Compt. Rend. vol. xxxviii. p. 83; and Canst. Jahresb. l. c.)
3. **Schmidt**: *On the Gastric Juice of Man.* (Lieb. Ann. vol. xcii. pp. 42 ss.)
4. **Schmidt**: *On the Secretion of the Pancreas.* (Lieb. Ann. vol. xcii. pp. 33 ss.; and Canst. Jahresb. l. c. p. 129.)
5. **Kölliker** and **H. Müller**: *Report of the Physiol. Institut. at Wurzburg.* (Transact. of the Wurzb. Soc. 1854, pp. 215 ss.)

Von Ancum detected *iodine* in all the waters used for drinking in Holland, in the proportion of from $\frac{1}{100}$ to 1 milligramme in a kilogramme of water. Alkaline waters from soil rich in clay or peat, contain iodine in the largest quantity. Chatin also found iodine in almost all the waters of Germany and Belgium; the absence of this substance in the various aliments—air and water included—he considers to be one of the principal causes of goitre and cretinism.

The main results of Schmidt's observations on the *gastric juice* we communicated in the number for January, 1855 (pp. 261 ss.), when analysing the dissertations of Grunewald and Schroeder.

Schmidt's newest experiments on the secretion of the pancreatic juice lead to different results from those described in Bidder and Schmidt's 'Verdauung und Stoffwechsel.' The older observations were made on pancreatic fluid obtained *soon* after the establishment of the fistula; Schmidt has since succeeded in producing a *permanent* fistula in a dog (after Ludwig's method), and was thus enabled to examine the secretion at different periods after the operation. While the fluid collected immediately after the operation, contains 10 to 11 per cent. of solid matter, with 9 to 10 per cent. of organic substances, that from the permanent fistula yields only 1·5 to 3·6 per cent. of the former, with 1 to 2·3 of the latter. The quantity of secretion furnished in the former experiments was in the proportion of 0·1 to 0·2 grammes, in the latter of 3·0 to 5·0 grammes within an hour, to 1 kilogramme of the animal's weight. Schmidt further found that the relative quantity of this secretion, like that of bile, is in a direct proportion to the relative amount of food and oxygen required. The larger the animal, the smaller the consumption by respiration; the smaller, therefore, the quantity of food required, the smaller the amount of the secretion in question. The fluid obtained from the permanent fistula is transparent, colourless, of alkaline reaction, specific gravity = 1·010 to 1·011; at the temperature of 98·6° it instantaneously transforms amylum into gum and sugar. Schmidt attributes the difference between the secretion from the transitory and the permanent fistula, to the influence of the nerves, as exhibited in the experiment of Ludwig. Schmidt repeats that 2 per cent. of the whole quantity secreted would be sufficient to transform the whole amount of starch consumed within twenty-four hours: the principal function of this secretion appears to be connected with the intermediate circulation of fluids, and with the saturation of the hydrochloric acid of the chyme separated from its base in the process of secretion of the gastric fluid.

Kölliker and H. Müller confirm the fact, that the secretion of the *parotid gland* alone is not sufficient to transform starch into sugar. The quantity secreted was considerably increased by chewing. Ludwig's statement is likewise corroborated, which is, that galvanic irritation of the nerve of the *submaxillary gland* is immediately followed by increased flow of saliva, which ceases almost simultaneously with the cessation of the stimulus. The secretion of this gland by itself likewise possesses no power of saccharification. The rapid action of human saliva on starch is confirmed; the fluid secretion during salivation effected by calomel possesses even greater power than the normal saliva. The active principle is contained in the watery, not in the alcoholic, extract. A mixture of equal parts of saliva and gastric juice has not the power in question, but a mixture with superabundance of saliva transforms starch into sugar.

II. BLOOD; CIRCULATION; RESPIRATION; VOICE; ANIMAL HEAT.

1. MOLESCHOTT: *Proportion of the White to the Red Blood-globules.* (Wien Wochenschrift. 1854, No. 8, pp. 113—118.)
2. VON ANCUM: *Iodine in the Air of the Netherlands.* (l. c.)
3. KLETZINSKY: *On Iodine and Nitric Acid in the Air.* (Keller's Archiv, and Canst. Jahresb. l. c. p. 104.)
4. TH. WEBER: *On the Origin of Sounds in the Bloodvessels.* (Vierordt, 1855, vol. xiv. pp. 40 ss.)
5. BRUECKE: *Physiol. Remarks on the Arteriæ Coron. Cordis.* (Report of the Imper. Academy of Vienna, November, 1854.)
6. KUSSMAUL: *On the Influence of the Circulation on the Movements of the Iris and other parts of the Head.* (Diss. Inaugur. Würzburg, 1855.)
7. M. GARCIA: *Observations on the Human Voice.* (Proceed. of the Royal Society, vol. vii. No. 13, pp. 399 ss.)
8. BERNARD: *Experimental Researches on the great Sympathetic Nerve, and especially on the Influence of its Section on the Animal Heat.* (Paris, 1854.)
9. BUDGE: *Report of the Physiolog. Institution at Bonn.* (Pr. Ver-Zeit. 1854. 48—50, and Schmidt's Jahrb. vol. lxxxvi. p. 6.)

Moleschott gives a series of observations on the proportion of the colourless to the red corpuscles. The average taken from forty-eight persons is 1 to 357, the maximum being 1 to 115, the minimum 1 to 709. This agrees well with Dr. Welcker's statement, who found 1 colourless to 335 red corpuscles. The number of colourless corpuscles is increased by albuminous food, diminished by fasting; it is greatest in boys, smallest in women, except during menstruation and pregnancy, when it is rather above the average. The observations of Vierordt and Gray make it probable that the blood of the splenic vein is much richer in colourless corpuscles than that of the other bloodvessels.

Von Ancum found iodine in the *air* in various places in Holland, in the proportion of $\frac{1}{150}$ to $\frac{1}{800}$ milligramme in 10,000 litres of air; the quantity of iodine in rain-water amounted to $\frac{1}{100}$ milligramme in 2 kilogrammes. Kletzinsky, on the contrary, could not detect any iodine in the air of Vienna, although his observation extends over four months (from July to October, 1853). But Kletzinsky obtained indubitable proof of the presence of *nitric acid.*

The experiments of Brown-Séquard, Budge, Kölliker, and others, concerning the influence exercised on the respiration and circulation by section and irritation of the vagus, will be recorded under V.

Th. Weber has given an elaborate paper with experiments on the origin of sounds in the bloodvessels, but the subject does not permit of a condensed extract; we must therefore limit ourselves to the quotation of a few of the inferences which he draws. 1. The sounds which we perceive in tubes traversed by a fluid, depend on the vibrations of the walls of the tube, excited by the motion of the fluid, and not on the friction of the particles of the fluid amongst themselves. 2. Sounds are produced sooner when the walls are thin than when they are thick. 3. Sooner in wider than in narrower tubes. 4. Water more easily gives rise to sounds than milk, milk more easily than a mixture of water and blood, diluted blood more easily than pure blood. 5. Narrowing of the tube favours the production of sounds, as does likewise the transit from a narrower into a wider tube, provided the movement of the stream be rapid enough. 6. A certain velocity of movement is indispensable for the production of sound. 7. The conduction of the sounds is accomplished principally by the walls of the tubes; the more dense and elastic the latter, the better the conduction.

The *placental murmur* is ascribed to pressure on the arteria iliaca externa, or communis or a. hypogastrica or aorta abdominalis; in rare cases, to the veins accompanying the arteries. Th. Weber found in every case examined, that the bruit disappeared when the person leant, to a certain degree, forward.

Bruecke draws attention to the physiological importance of the fact, that the openings of the coronary arteries in mammalia (man included) and birds, are situated within the sinus valsalvæ. Some of the prominent effects of this arrangement are, according to him, that the openings are closed during the ventricular systole, opened during the ventricular diastole; that the blood entering the arteries during the diastole, serves to expand the walls of the ventricles, and thus to facilitate the admission of the blood into their cavities.

Kussmaul effected opposite states of the circulation in the head,—*a*. By compressing or tying both the subclavian and carotid arteries in the anterior mediastinum; *b*. By compressing or tying the external jugular, or the external jugular and subclavian veins. His experiments, made on rabbits, lead to the following conclusions:—1. The arrested supply of arterial blood causes, in general, at first, contraction of the pupil, the palpebral fissure, the nostrils, the mouth, and pinna of the ear; this contraction is soon (after from eight to twenty seconds) followed by dilatation of the same parts. The phenomena of the eyelids, and still more those of the iris, are the most constant and the most marked; thus the pupil contracts in the beginning from 3 or 3·5 millimetres (longitudinal diameter), to 1·5 or 2 millimetres, and dilates afterwards to 7 or 8 millimetres. The symptoms of the first period of arterial anæmia are attributed to cerebral irritation, those of the second to torpor or paralysis; this view is strengthened by the circumstance, that during the first period the conjunctiva and eyelids are very sensitive, while they appear in the second altogether deprived of sensation. 2. The re-entrance of the arterial blood is immediately followed by considerable dilatation of the pupil, the palpebral fissure, and the pinna auris; other symptoms are less regularly observed; and by degrees the parts mentioned assume again their normal shape. 3. Prevented reflux of the venous blood frequently causes contraction; the removal of the obstacle, dilatation of the pupil. 4. To the second period of arterial anæmia belongs rotation of the eyeball from the interior and lower to the exterior and superior part of the orbit; the opposite movement of the eyeball is at first produced by the restoration of the arterial circulation. At the same time, the eyeball appears retracted in the former, more prominent in the latter case. 5. Increased prominence of the eyeball and width of the palpebral fissure are also consequences of prevented venous reflux; while the opposite phenomena are observed during the first moments after the venous circulation is re-established.

M. Garcia has made his interesting 'Observations on the Human Voice,' by placing a little mirror, fixed on a long handle, suitably bent, in the throat of the person experimented on, against the soft palate and uvula. Thus he finds that, by a deep inspiration, "The arytænoid cartilages become separated by a very free lateral movement; the superior ligaments are placed against the ventricles; the inferior ventricles are also drawn back, though in a less degree, into the same cavities; and the glottis, large and wide open, is exhibited so as to show in part the rings of the trachea." (p. 400.) The anterior part of the glottis—at least, one-third of the whole—remains concealed by the epiglottis.*

Movement of the Glottis.—"As soon as we prepare to produce a sound, the arytænoid cartilages approach each other, and press together by their interior surfaces and by the anterior apophyses, without leaving any space or inter-cartilaginous glottis; sometimes even they come in contact so closely as to cross each other by the tubercles of Santorini. To this movement of the anterior apophyses, that of the ligaments of the glottis corresponds, which detach themselves from the ventricles, come in contact with different degrees of energy, and show themselves at the bottom of the larynx under the form of an ellipse, of a yellowish colour. The superior ligaments, together with the arytæno-epiglottidean folds, assist to form the tube which surmounts the glottis; and being the lower and free extremity of that tube, enframe the ellipse, the surface of which they enlarge or diminish, according as they enter more or less into the ventricles. These last scarcely retain a trace of their opening. . . . . When the arytæno-epiglottidean folds contract, they lower the epiglottis, and make the superior orifice of the larynx considerably narrower." (p. 400.)

Concerning the *chest voice,* Garcia observes, that, in emitting the veiled and feeble sounds, the larynx opens, the lips of the glottis are seen agitated by large and loose vibrations "throughout its entire extent." As the sounds ascend, the apophyses, by gradual apposition, commencing at the back, encroach on the length of the glottis; and as the sounds become still higher, the vibrations are accomplished by the vocal ligaments alone. The glottis then presents the aspect of a line slightly swelled towards its middle; the cavity of the larynx having become very small, the superior ligaments having contracted the extent of the ellipse to less than one-half. During the production of the low notes of the *falsetto,* the glottis can be much better seen. Its vibrating sides are then formed by the anterior apophyses of the arytænoid cartilages, and by the ligaments; they become gradually shorter as the voice ascends.

As regards the theory of the manner in which the *sounds are formed,* Garcia entertains the view "that the voice is formed by the compressions and expansions of the air, or the successive and regular explosions which it produces in passing through the glottis. . . . . The ligaments of the glottis," Garcia continues, "are situate about the mean level of the upper border of the cricoid, close the passage, and present a resistance to the air. As soon as the air has accumulated sufficiently, it parts these folds, and produces an explosion; but at the same instant, by virtue of their elasticity, and the pressure from below being relieved, they meet again, to give rise to a fresh explosion. A series of these compressions and expansions, or of explosions, occasioned by the expansive force of the air and the reaction of the glottis, produces the voice." (p. 404.)

During the chest-register, the vocal ligaments are more stretched and in nearer contact with each other than in the falsetto, the lateral crico-arytænoid muscle remaining inactive during the latter; but in both cases the sound is formed, according to the author, "not by actual vibrations of either the whole or part of the tendons, but by the successive explosions which they allow." Concerning the *qualities of the voice,* Garcia says—1. According as the glottis partially or entirely closes the passage "between the explosions, it produces veiled and brilliant sounds; 2. The tube which surmounts and surrounds it also greatly affects the quality of the voice; its contractions give brilliancy, and its widening, volume, to the vocal sound; 3. The epiglottis also plays a very important part, for every time that it lowers itself and nearly closes the orifice of the larynx, the voice gains in brilliancy; and when, on the other hand, it is drawn up, the voice immediately becomes veiled." (p. 410.)

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* [Some years ago, we had a small oval mirror constructed by Coxeter, for the purposes of diagnosis in affections of the epiglottis and larynx; it was fixed on a handle, upon which it moved with a hinge joint; but the condensation of vapour on the mirror, and the absence of due reflection from the epiglottis, rendered it useless. We have recently repeated the attempt, with the same unsatisfactory result. It would be undoubtedly of practical utility if M. Garcia would explain his mode of manipulating more fully.—Ed.]

A. Bernard has renewed his experiments concerning the connexion of *animal heat* with the nervous system. From his observations after section of the sympathetic nerve, extirpation of the superior cervical ganglion, section of the nervus facialis and of the nervus trigeminus, &c., &c., as well as from the experiments of other physiologists, Bernard draws the following inferences :—1. Section of sensory or motor nerves produces diminution of temperature; 2. Section of the sympathetic nerve causes increased heat without alteration of either motion or sensation; 3. Section of a nerve composed of the three kinds of fibres is followed by increase of temperature, though of course in a less marked degree than in the preceding case. The increase of temperature is most considerable soon after the operation: thus it rose on the right side of the head of a rabbit, within fifteen minutes after the extirpation of the right superior cervical ganglion, from 91·4° to 102·2°; on the following day the heat is less augmented, but still between 8° and 10° higher than on the other side. Irritation of the peripheric end of the divided sympathetic nerve reduces the previously increased temperature even below the standard. It becomes also diminished by the tying of the arteries, but remains still considerably higher than that of the opposite side.

The fact that the division of either half of the spinal marrow at the inferior cervical or superior dorsal region, produces likewise increased heat in the corresponding side of the face, is confirmed by Budge's experiments in the Physiological Institution at Bonn. The increase is greatest at the external ear and in the meatus auditus externus. The circumstance that the sympathetic nerve of the neck originates from the spinal marrow near the place where the division was performed, explains the similarity of the phenomena with those resulting from section of the sympathetic nerve itself.

III. SECRETION; EXCRETION; NUTRITION; METAMORPHOSIS OF MATTER.

1. ARNOLD: *On the Physiology of the Bile.* (Mannheim, 1854.)
2. KÖLLIKER and H. MÜLLER. (l. c.)
3. FIGUIER and LONGET: *On the Formation of Sugar in the Liver.* (Gaz. de Paris, 5, 6, 7, 1855; and Schmidt's Jahrb. vol. lxxxvi. p. 145.)
4. PAVY: *An Experimental Inquiry into the Nature of the Metamorphosis of Saccharine Matter as a Normal Process of the Animal Economy.* (Proceed. Roy. Soc. vol. vii. No. 13.)
5. RUDOLPH: *De Urina Sanguinis, Potus et Chyli.* (Diss. Inaug. Marburgi, 1854.)
6. W. MOORE: *Experiments as to the Existence of Sugar in the Urine of the Fœtus.* (Dublin Quarterly Journal. August, 1855.)
7. FALCK: *Contribution to the Knowledge of the History of the Development of Animal Bodies.* (Virch. Arch. vii. pp. 37 ss.)
8. BÖCKER: *On Sleep.* (Arch. f. gemeinschftl. Arb.; v. Vogel, Nasse, and Beneke ii. 1, pp. 76 ss. 1855.)

(The Investigations of SCHMIDT, KÖLLIKER, and H. MÜLLER, on the 'Gastric and Pancreatic Secretions,' are recorded under I.)

The result of Arnold's experiments is in accordance, in the principal points, with that lately obtained by Nasse, Bidder and Schmidt. 1. Arnold's observations confirm the fact, that the bile may be drawn off from the body without considerable harm to the constitution, provided the quantity of food be increased. 2. The absence of bile in the intestinal canal hinders neither the digestion and absorption of albuminous substances, nor that of amylum; but the absorption of fat appears impaired, as a proportionally large quantity is passing off with the fæces. 3. The bile seems to prevent the putrid decomposition of the contents of the intestinal tube, the fæcal discharges and flatus of dogs with fistula of the bile duct being of a remarkably offensive character. 4. The quantity of bile secreted in twenty-four hours increases and decreases with the quantity of food. Nasse's experiments had already shown that the quality of food also is of great influence; that meat produces in the dog a much more copious flow of bile than bread and other vegetable aliments.* 5. The quantity of bile secreted under *normal* circumstances cannot be calculated with accuracy from these experiments, as dogs affected with fistula consume a much larger amount of food. Arnold considers, therefore, the numbers given by other observers as being too high—1 kilogramme of dog yields, according to him, only about 2 grammes of bile within twenty-four hours. 6. The quantity of bile was largest soon after meals, decreasing again from the fourth hour after the meal. The ingestion of water is more quickly followed by increased flow of bile, this being largest after the lapse of an hour.

Similar experiments have been performed on several dogs, in the Physiological Institution at Wurzburg, by Kölliker, H. Müller, &c. The amount of bile obtained there is larger than that assumed by Arnold, otherwise the result does not materially differ from that described by other observers.

The same physiologists confirm the observation of Bernard respecting the *formation of sugar in the liver*, and its absence in the blood of the portal vein.

Figuier and Longet, on the contrary, deny the correctness of Bernard's and Lehmann's repeated statements, adopting the view that the sugar is not formed in the liver, but absorbed as such from the intestinal tube. Longet contends that sugar cannot be detected by the usual oxide of copper test, when it is mixed with proteinaceous compounds (albuminoids) altered by the action of the gastric juice. To the presence of these albuminoids in the blood of the portal vein, he attributes the cause of the seeming, but not real, absence of sugar. He has in several cases proved its presence by the fermentation test. Figuier has also lately found sugar in the portal vein of several dogs, sometimes in larger, sometimes in smaller proportions (according to the length of the interval between the last meal and the death of the animals). He concludes from his experiments, that the sugar is absorbed in the alimentary canal, conducted through the vena portæ to the liver, where it may be stored up for some time before it is delivered to the general circulation.

Dr. Pavy confirms, in general, Bernard's statements. According to his experiments, the blood of the right ventricle contains most sugar, that of the systemic arteries only a small, and that of the veins a still smaller, quantity; sugar is altogether absent from the blood of the portal vein, provided none have been recently introduced with the food, and provided also the animal be not at a period of full intestinal digestion. We cannot give here in detail the interesting contents of Dr. Pavy's papers, as they belong more to the chemical part of these records; we only mention that, according to his view, based on experiments, the destruction of sugar in the animal system is not effected by combustion, as generally adopted, but by a process analogous to acid fermentation, induced through the molecular changes taking place in the azotized substances, principally the fibrin of the blood.

Rudolph, under the superintendence of Falck, has made interesting observations on himself and several colleagues, concerning the characteristics of the *urina sanguinis, potus*, and *chyli*. That urine only should be called *urina sanguinis* which is secreted during the state of inanition. The experimenters, therefore, did not take any solid or fluid food for twenty-four hours, and examined as urina sanguinis only that urine which was secreted after the twelfth hour from the commencement of fasting. The urine thus obtained was always acid, of a saturated yellow colour; specific gravity, 1·009—1·030; the quantity secreted within an hour never exceeding 90 C.C. (rather more than three fluid ounces), containing 3·6 to 5·0 per cent. of solid matter. There is always a certain proportion between weight of body, quantity of urina sanguinis, and solid residue; the per-centage of the latter increases with the period elapsed after the last meal. The urine passed after the ingestion of solid food should be called *urina chyli* or *cibi;* it only contains little fluid. Such urine is of course different in colour and composition, according to the amount and quality of the food taken; its specific gravity varies from 1·020 and 1·030; the quantity is about 50 C.C. in an hour; the solid residue weighs about three times as much as that of the urina sanguinis, this considerable increase commencing as early as two hours after the meal. As *urina potus*, that urine only has been examined which had been passed after the exclusive use of fluids, the experiments did not commence until twelve hours had elapsed after the preceding solid meal. The character described of the urine thus obtained does not differ from that generally known. We mention however the fact, that after the ingestion of coffee and tea in large quantities, the total amount of solid substances excreted was not larger than that contained in the urina sanguinis; after the use of milk it was slightly increased; *beer*, which acted with considerable diuretic power, did "not only not carry off any of the solid tissues of the body, but added its own solid constituents." On these grounds the author thinks himself entitled to contradict Becquerel's statement, that, by the ingestion of large quantities of fluid, the excretion of solids through the kidneys becomes increased.

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* Commentatio de bilis quotidie a cane secreta copia et indole, p. 18. Marburgi, 1851.

Moore describes the *fœtal urine* as an albuminous fluid, *free from sugar*, containing some of the usual salts of the urine, abounding in an highly-nitrogenized principle, probably allantoin, affording no urea, and depositing a remarkably large amount of nucleated basement epithelium.

Falck has performed a series of interesting observations on young dogs, by which he shows the daily increase of the young animals, and the proportion in which the increase is distributed amongst the single organs. Our space does not permit us to insert any of his tables exhibiting the difference with which the different parts of the body participate in the increase of its weight. To give an instance, we mention that the development of the brain and spinal marrow is, in bulk at least, much less rapid after birth than that of other organs; the nervous centres in the new-born animal forming 3·5 to 4·5 per cent. of the whole weight of the body, and only about 1·9 per cent. of the dog three months old.

From Dr. Böcker's paper, treating on the influence of sleep on the metamorphosis of matter, we can likewise offer only a very short extract. Without entering into the particulars concerning the manner in which the experiments are performed, we mention, that they are all instituted at the same time, and in order to avoid the influence of digestion during the state of fasting. While Guenther* and Purkinje† speak of diminution of the secretions and excretions during sleep, Böcker found that *the excretion through the skin is slightly*, and that through *the kidneys considerably, increased*, with the *exception however of the alkaline phosphates* and the *lithic acid*, the quantity of which is materially smaller than it is during the state of waking. Schultz-Schultzenstein had already previously noticed the increase of the excretions, but not the last-mentioned exception. The inferences drawn by Böcker from his observations are: that during sleep the retrogressive metamorphosis of all the organs of the body is increased, except that of the nervous centre, which is, on the contrary, lessened (diminution of alkaline phosphates); that during sleep the progressive nutrition (*Anbildung*), and principally that of the brain, is effected.

IV. Nervous System.

1. Foltz: *On the Cerebro-spinal Fluid.* (Gaz. de Paris, No. 10, 1855, and Schmidt's Jahrb. vol. lxxxvi. p. 292.)
2. Vulpian and Philippeaux: *Notes on Experiments regarding the deep Origin of the Nerves of the Eye.* (Gaz. Méd. de Paris, No. 30, 1854; and Canstatt, l. c. p. 190.)
3. Brown-Séquard: *Proof of the Crossing of the Sensitive Fibres in the Spinal Marrow.* (Gaz. Méd. de Paris, No. 9, 1854; and Canstatt, l. c. 179.)

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* Lehr. der Physiol. Leipzig, 1846.

† R. Wagner's Handwörterb. der Physiol. iii. 2, p. 429.

4. Budge: *Report of the Physiol. Inst.* (l. c.)
5. Kölliker and H. Müller: *Report of the Physiol. Inst.* (l. c.)
6. Brown-Séquard: *On the Effects of the Section of the Pneumogastric Nerves on the Force of the Heart.* (Gaz. Méd. de Paris, l. c.)
7. Snellen: *Influence of the Vagus on the Respiratory Movements.* (Nederl. Lancet, Dec. 1854 & Jan. 1855; and Schmidt's Jahrb. vol. lxxxvii. p. 161.)
8. Wundt: *On the Influence of the Section of the Pneumogastric Nerves on the Respiratory Organs.* (Müll. Arch. pp. 269 ss., 1855.)
9. Schiff: *On the Sympathetic Nerve.* (Gaz. Hebd. de Méd. No. 54, pp. 421, ss. 1854; and Canst. l. c. p. 179.)
10. Brown-Séquard: *On the Sympathetic Nerve.* (Compt. Rend. pp. 72—76, 1854; and Canstatt. l. c.)
11. Schiff: *On the Influence of the Nerves on the Nutrition of Bones.* (Compt. Rend. pp. 1050, 1854; and Canstatt, l. c.)

The *cerebro-spinal fluid* possesses, according to Foltz, a specific gravity of 1·010, while Turner had found it to be only 1·006. Concerning the physiological functions of the liquid, Foltz proposes: 1. That it acts as a ligamentum suspensorium; that the brain, suspended in this fluid, weighs only 26 grammes, while it weighs in the air 131·8 grammes; that by this diminution of weight the parts situated at the base of the brain are prevented from being compressed, and thus impaired in their function. 2. That it diminishes considerably the force of external mechanical influences, as jumping, &c. 3. That it regulates the circulation within the cranium and spinal canal. In favour of this proposition, Foltz mentions, amongst other points, that all the veins of the brain take a more or less ascending course towards the sinuses, without being provided with valves; that they possess only a thin internal and middle, but no external, tunic; that the sinuses are wide channels with rigid walls—circumstances which, by themselves, facilitate the afflux, and render difficult the reflux, of blood. Through the cerebro-spinal fluid the pressure from the arteries is communicated to the veins, and congestion is prevented.

Vulpian and Philippeaux have performed a series of experiments on the encephalon of the rabbit. A superficial longitudinal incision on the floor of the fourth ventricle, about one millimètre from the middle, produced more or less complete paralysis of the facial nerve of the opposite side, and complete paralysis of the nerves of the external muscles of the eye of the same side. A great number of the fibres of the oculo-motorius of both sides cross each other on the floor of the aquæductus Sylvii; irritation of this place, by means of a pin, produces convulsions and squinting. Lesion of the nerve-tract below the corpora quadrigemina is in general followed by dilatation of the pupil of the same side; destruction of either half of the corpora quadrigemina, by blindness and dilatation of the pupil of the opposite side. Convulsions of the muscles of the eye are very frequently effected by injuries to the pons, and the several crura cerebelli. These are also observed after lesions near the calamus, and are frequently accompanied by contraction of the pupil of the same, and dilatation of that of the opposite, side.

Brown-Séquard and Budge arrive, by their experiments, both at the conclusion, that of the *sensitive*—i.e., the *posterior*—fibres of the spinal marrow, the greater part cross immediately after their entrance from the periphery; the transverse section of one half of the spinal cord produces therefore almost complete anæsthesia of the opposite side of the body, supplied by nerves issuing below the place of section, while the sensation of the same side becomes only slightly impaired; thus, after section of the right half of the cord, near the termination of the medulla oblongata, the nerves of the opposite ear, which arise from the spinal marrow, may be pinched or irritated in every possible manner, without inducing signs of pain, while those of the same side exhibit an unusual sensibility (Brown-Séquard). The *motor* fibres, on the contrary, remain, until they reach the medulla oblongata on the same side.

The influence of lesion of the spinal marrow and the sympathetic nerve, relating to the production of *animal heat*, has been mentioned under that head (II.).

Several physiologists have lately occupied themselves with experiments on the *pneumogastric nerve*. Kölliker and H. Müller, Budge, R. Wagner, Brown-Séquard, and Snellen, all agree that after section of the nerve, galvanic irritation of the *centric* end produces cessation of the respiratory movements, while irritation of the *peripheric* end suspends the action of the heart. Kölliker, Müller, and Snellen always found the cessation of the respiratory movements take place with a deep inspiration; Budge, on the contrary, observed this to be the case during the act of expiration. Brown-Séquard found, by means of the hæmadynamometer, the pressure of the blood in the arteries increased during the first hour, and longer after the section of the nervi vagi; this increase is followed by the normal pressure, which gradually sinks below the standard. His view is, that in consequence of the section of the vagi, the vessels of the heart become distended, that more blood circulates through its walls, that this blood is richer in carbonic acid, and acts therefore as a greater excitant on the muscle, which contracts with more energy as long as its irritability lasts. The same author considers the suspension of the heart's action, through irritation of the nerves, to be the effect of the contraction of the arteries of the heart.

Wundt communicates his able researches on the influence of the *section of the pneumogastric* nerves on the *respiratory* organs. The principal inferences are:—1. The impediment of respiration thus produced is twofold, the one being caused by the paralysis of the larynx (recurrent nerve), the other by that of the bronchi and lungs. The impediment in the larynx bears to that in the lungs the proportion of 2 : 3; this was ascertained by dissecting separately the recurrent and the pneumogastric nerves. 2. Through this, the time and force required to effect a *sufficient* inspiration is increased, the number of respiratory movements diminished; the quantity of air inspired remaining at the beginning unaltered, becoming however gradually diminished as the impediment in the air-passages increases, and the muscles lose their power. 3. The latter circumstance—i.e., the diminished ingestion of air—is always accompanied by sinking of the animal heat. 4. The principal alterations in the respiratory organs consist of lobular inflammation, and of passive congestion and its further stages. The former is caused by the paralysis of the larynx permitting the entrance of particles of food into the bronchi, without leading to cough and the consequent expulsion of the foreign substances. The passive hyperæmia is occasioned by the coincidence of the paralysis of the bronchi and part of the bloodvessels, and the increased action of the heart during the first period after the section of the nerves. By the disproportion between inspiration and expiration, another alteration is caused—namely, the vesicular emphysema. 5. Concerning the influence of age: the lobular inflammation occurs most rarely in young animals, and most frequently in those of middle age. The passive congestion sets in most quickly in young animals, less so in those of middle age, and least so in old animals. 6. Congestion and inflammation may be retarded by tracheotomy, but not altogether prevented. 7. The proximate cause of death is asphyxia.

Brown-Séquard's and Schiff's experiments on the *sympathetic nerve* confirm that its section in the neck produces retraction of the corresponding eyeball, contraction of the pupil, increased secretion of lachrymal fluid, and mucous discharge of the eyelids. (The influence of the proceeding on the animal heat is described under II.) Less constant symptoms were, diminished brightness or ulceration of the cornea, change of colour of the iris, inflammation of the conjunctiva, &c. Galvanic irritation of the dissected nerve causes the eyeball to return to its normal place, and effects dilatation of the pupil; the muscles of the face and ear, which had been in general contracted after the section, become relaxed: the bloodvessels, on the contrary, become contracted.

Schiff examined the influence of the section of nerves on the nutrition of bones. A few weeks after the section of the nerves of a limb, the bones of the latter exhibit hypertrophy of the periosteum, and commencing rarification of the osseous tissue; some months later, the hypertrophy of the periosteum appears much increased, and the proportion of the inorganic constituents of the osseous substance considerably diminished. The atrophy of the osseous tissue is ascribed by Schiff to the paralysis of the limb, and can therefore be prevented by regular galvanization; the hypertrophy of the periosteum is attributed to the paralysis of the nerves of the vessels.

V. SENSES.

1. BUROW: *The Macula Lutea visible in our own Eye.* (Müll. Arch. pp. 166 ss., 1854; and Canstatt, l. c. p. 177.)
2. H. MÜLLER: *On the Retina.* (Verhandl. d. Würzburg Gesellsch. pp. 411 ss., 1855.
3. DONDERS: *On Albinos.* (Nederl. Lancet, Maart. 1854, and Schmidt's Jahrb. vol. lxxxvi. p. 23.
4. ENSMANN: *On the Duration of the Impression of Light.* (Poggend. Ann. vol. xci. p. 611, and Canstatt, l. c. p. 10.)
5. KÖLLIKER: *Experimental Proof of a Dilatator Pupillæ.* (Zeitsch. für Zoologie, vol. vi. p. 143, and Canstatt, l. c. p. 176.)

Burow, in producing Purkinje's figure (Purkinje's Adernfigur), observed the *macula lutea* of his own eye as a round, well-defined spot, in the centre of the optic field. From the arrangement of the shadow, he concludes that it projects from the surface of the retina into the corpus vitreum.

H. Müller has continued his experiments on the retina. The present essay tends to the following inferences:—1. Purkinje's figure is the shadow thrown by the vessels of the retina on that layer which is endowed with the faculty of the perception of light. 2. The direction of the motion which this vascular figure seems to take when the source of light is moved, confirms this explanation. 3. The layer for the perception of objective light must lie behind the vessels, therefore, at least, behind the layer of fibres of the optic nerve and of the nerve-cells. By means of the parallax, Müller has calculated the distance between the vessels and the light-perceiving membrane to be 0·17—0·33 millimètre. 4. As this distance corresponds with that existing between the layer of fibres of the optic nerve and the bacillar layer, the latter may with probability be considered to possess the faculty for perceiving objective light. 5. There is an essential difference between the origin of Purkinje's figure—i.e., the *shadow* of the vessels —and a similar figure produced by *pressure*, a figure which, besides, is always less defined in its outlines and ramifications.

Ensmann observes, that the duration of the impression of colours occupies a different space of time, according to the quality of the colour; that it continues longest from yellow, second longest from white, less from red, and least from blue.

Donders attributes the colour of the pupil of albinos to the light entering through the sclerotica and iris, and not to that entering through the pupil. By placing before the eye an untransparent capsule, with an opening corresponding to the pupil, the latter appears as dark in the albinos as in the eye of any other person. The power of vision was likewise much improved by the application of this capsule. Donders considers, therefore, as the principal cause of disturbed vision, the light entering through the sclerotica and other membranes being constantly diffused over the retina.

Kölliker proved the existence of a muscular *dilatator pupillæ*. After having removed the cornea and the sphincter of the iris in an albino rabbit, he applied weak streams of galvanism to the remainder of the iris: dilatation of the pupil, accompanied by convexity of the anterior surface of the iris, was the effect of repeated experiments.

Concerning the physiology of the *ear*, we ought to report on a paper by Dr. Rinne,* containing a series of interesting experiments and deductions, which do not accord in all points with the views generally received; but as it is impossible to condense them sufficiently for the purposes of this Report, we must defer doing so for the present.

VI. Generation: History of Development.

1. Duplay: *On the Alterations in the Secreting and Excreting Apparatus of Sperma in Old Men.* (Archiv. Génér., pp. 428 ss. Oct. 1855.)
2. Bischoff: *Confirmation of the Discoveries of* Newport *and* Barry. (Giessen, 1854.)
3. Meissner: *On the Entrance of the Elements of the Sperma into the Ovum.* (Zeitsch. für Zoologie, pp. 208 ss.; 272 ss., 1854; and Canst., loc cit. p. 195.)
4. Leuckart: *On the Micropyle of the Ova of Insects.* (Müll. Archiv, pp. 193 ss., 1855.)
5. Bischoff: *The History of the Development of the Roe.* (Giessen, 1854.)
6. Küchenmeister: *Development of Cysticercus Cellulosæ in Tænia Solium.* (Correspond. Blatt. der Ver. für Gemein. Arbeit., p. 158. No. 13, 1855.)

Duplay draws, from numerous examinations of the secreting and excreting organs of sperma in *old men*, the inferences—1. That the changes in the *secreting* apparatus consist only in a very slight atrophy; that these are not sufficient to explain the want of procreative power, as the sperma continues to be secreted, although in diminished quantity, and to contain the elements that are considered necessary for fecundation. 2. That more frequently the cause of impotence is situated in the *excreting* apparatus, consisting sometimes in the obliteration of the canal of the epididymis, of the ductus deferens, or the vesicula seminalis. 3. That there is, in old age, no such specific and constant alteration for the testicle, as rarefaction is for the lungs, but that all the changes observed are occasionally met with also in the other periods of life. On the whole, Duplay is inclined to think that, in the majority of old men, the want of procreative power is not to be attributed to anatomic alterations in the secreting or excreting apparatus, but to some other cause or causes.

Bischoff, in opposition to his former views, admits now the facts discovered by Newport and Barry, concerning the entrance of the spermatozoa into the interior of the ova of the frog and rabbit.

Meissner found, likewise, several times, spermatozoa within the ovum of the rabbit. He further describes the ova of several insects (musca vomitoria, musca domestica, various species of tipula, calea, &c.), with their micropyles, and the entrance of the spermatozoa through the latter. The spermatozoa undergo, according to him, a kind of fatty metamorphosis within the ovum; a change that, however, may be observed also in those spermatozoa which are retained in the testicles or in the vesiculæ seminales.

Another elaborate essay on the *ova of insects* has been furnished by R. Leuckart, of Giessen. He examined the ova of about 180 different kinds of insects. From a careful analysis of the results of these researches, Leuckart considers himself entitled to the following inferences:—1. That the ova of all insects are provided with a micropyle apparatus; 2. That this consists of a single canal, or of several canals passing through the membranes of the ovum; 3. That these channels serve as passages for the spermatozoa into the ovum. It is to be remarked, however, that the last circumstance has been witnessed by the author only in twelve distinct species.

Bischoff supplies us with the history of the development of the roe. He confirms the fact, that the fecundation takes place towards the end of July or beginning of August—that the ovum, after having previously undergone the process of furrowing, remains during the following four months and a half in an unaltered state in the uterus, retaining the diameter of $\frac{1}{12}$th of a line. The uterus also participates in this state of rest until after the middle of December, from which period the process of development of ovum and uterus is similar to that of the ruminating animals.

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* Zur Physiol. der menschlichen Ohres: Prager Vierteljahrsschrift, xii. 1. 1855.

Küchenmeister gives the interesting experimental proof that *cysticercus cellulosæ* becomes transformed into *tænia solium* in the intestinal tube of man. A criminal received with his food a certain number of cysticerci 72, 60, 36, 24, and 12 hours before his death. At the examination (48 hours after death) ten young tænias were found in the duodenum, four of which were provided with two pairs of hooks; the length of all was three or four millimètres, except one, that measured about six millimètres. We shall probably have an opportunity of returning to this subject, as we are led to expect an elaborate work by the same author on the parasites of the living human body.

HALF-YEARLY REPORT ON MATERIA MEDICA & THERAPEUTICS.

By Edward Ballard, M.D.,
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I. *Formulæ for the Internal Administration of Chloroform.* By M. Dannecy. (L'Union Méd., April, 1855.)

M. Dannecy employs oil to dissolve the chloroform. The formula he uses is as follows:—Take of pure chloroform, 2 grammes; oil of sweet almonds, 8 grammes; gum arabic, 4 grammes; syrup of orange flowers, 30 grammes; distilled water, 60 grammes. Mix the oil with the chloroform, and make with the mixture an oily draught, in the ordinary way. When gum alone is employed to suspend chloroform in a draught, separation of the latter sooner or later takes place; and where alcohol is used, as by many practitioners, in proportion of one part chloroform to four parts alcohol, an excitant is introduced which may not be desirable, and if the quantity of chloroform prescribed be considerable, this objection is a serious one. The advantages which M. Dannecy sees in his formula are—1. That a perfectly homogeneous and stable mixture is produced, whatever be the proportion of chloroform prescribed; 2. That no excitant like alcohol is introduced into potions which are most frequently intended to be calmative; 3. That it dispenses with every kind of precaution on the part of the patient, or those who have the care of him in administering the remedy. He thinks, further, that the mixture of the chloroform with the oil, without any detraction from the limpidity of the latter, is a test of the purity of the chloroform.

The Commission of the Société de Pharmacie,[*] while admitting M. Dannecy's formula as rational, proposes the following:—Chloroform, 2 or 4 grammes; sugar, 12 grammes; gum arabic, 5 or 10 grammes; water, 100 grammes. The chloroform is added to the sugar in a mortar, then the gum is added, and lastly, by degrees, the water. M. Deschamps, in commenting on the several formulæ which have been suggested, considers that of the Commission as preferable both to that of M. Dannecy and of M. Wahu, who dissolved chloroform in three or four parts of alcohol, and then mixed it with a solution of gum, on the ground that submitting all patients to the action of much alcohol or oil is not a matter of indifference. It is true that, after a time, a whitish flocculent deposit takes place, but a little shaking restores the appearance of the mixture. M. Deschamps proposes another formula—viz: Chloroform, 2, 4, 6, &c. grammes; syrup, 30 grammes; yolk of one egg; water, 150 grammes. Dilute the yolk of egg with the water, and strain; weigh the syrup, then the chloroform; add the strained liquor, and shake the whole together.

[*] Bull. Gén. de Thérap. p. 73. July, 1855.

II. *Preparation of Soluble Citrate of Magnesia.* By M. ROBIQUET. (Journ. de Pharm. d'Anvers, Aug. 1855, and Pharmaceutical Journal, Oct. 1855.)

Take of citric acid, 1 kilogramme; carbonate of magnesia, 630 grammes; boiling water, 350 grammes. Reduce the citric acid to coarse powder, and dissolve it in boiling water. When the solution shall have cooled, and before it crystallizes, pour it into a large stoneware pan, and by means of a sieve allow the magnesia to fall rapidly over its surface, taking care not to stir it. The reaction takes place gradually; when it appears to have ceased, which is easily observed, mix thoroughly and as rapidly as possible while the paste remains soft and ductile. The utmost care must be taken that the mass do not heat, for if the temperature suddenly rises, it is a certain sign that the citrate of magnesia is undergoing that molecular change which causes it to pass into the insoluble variety, and the product is lost. Thus, for better ensuring success, it is not useless to set the tub in a pan of cold water, and constantly to bring the citrate in thin layers into contact with the sides of the vessel. When this manipulation is over, the whole product should be left at rest for twenty-four hours; the swelled-up mass of citrate should then be divided into fragments, and dried in a stove at a heat not over 60° or 68° Fahr. The secret of the process is to use the smallest quantity of water, and to avoid an elevation of temperature at the moment of combination.

III. *Mode of Administering Phosphate of Lime.* (Bull. Gén. de Thérap., p. 542. June, 1855.)

The insolubility of phosphate of lime has led to its disuse by many practitioners. According to M. Küchenmeister, of Zittau, this objection is removed by uniting the phosphate with carbonate of lime, and then adding an acid, when a soluble combination and an useful medicine results. The proportions recommended are—carbonate of lime, 8 grammes; phosphate of lime, 4 grammes; sugar of milk, 12 grammes. M. Küchenmeister sometimes adds from 1 to 2 grammes of lactate of iron, and directs three pinches of this powder to be taken at the commencement of a meal. The object of the addition of carbonate of lime is to favour the solution of the phosphate. Under the influence of the lactic acid or of the hydrochloric acid present in the alimentary canal, carbonic acid is disengaged, and renders part of the carbonate soluble. The sugar of milk is intended to furnish lactic acid. Finally, the author remarks that the presence of albuminates is necessary to determine the solution of the phosphate of lime, a condition which is present when administered with the food.

IV. *Notes on Native Remedies. The Chaulmoogra.* By F. J. MOUAT, M.D. (Indian Annals of Medical Science, p. 646. April, 1854.)

This drug is furnished by the Chaulmoogra (Roxburgh) or Gynocardia (Lindley) odorata, of the nat. ord. Pangiaceæ. Another name for the tree is the Petarkura. According to Roxburgh, it is indigenous to the Sylhet district. The fruit, which is succulent and indehiscent, ripens towards the close of the year, and the seed it contains being taken out and dried, is sold to the drug dealers. The seeds yield by expression a bland, fixed oil, with a peculiar and slightly unpleasant smell and taste, with the faintest possible after-flavour of the bitter almond. The oil procured from the bazaars is invariably impure. It appears to have been long known to and prized by the natives in the treatment of leprosy; and few of the faquirs travelling about the country are unacquainted with its properties. The author relates three cases in which he has employed it:

Case 1 is that of a creole who had been for eighteen months an inmate of a leper asylum, greatly emaciated, and whose body exhaled an extremely offensive odour. The body was covered with livid patches; the nails had fallen from the toes of the left foot, and upon both feet were large ragged excavated ulcers. These ulcers were dressed daily with the oil of chaulmoogra, and he took, three times a day, a pill consisting of six grains of the seed. At the expiration of a fortnight the patient began to amend rapidly, the ulcers granulated and healed, and the general health improved up to the time of his leaving the hospital. Case 2 is that of a young man, aged 26, of scrofulous habit, suffering from enormous enlargement and ulceration of the parotid and submaxillary glands, cleft palate, and ulceration of the roof of the mouth. As the usual treatment failed of success, the chaulmoogra was given in doses of twelve grains three times a day, and the external ulcers were dressed with the oil, and in less than ten days an evident beneficial change occurred, and in eighteen days the sores were nearly cicatrized, and he became an out-patient. In another fortnight, the ulcers being healed and the glands much diminished in size, he ceased attendance at the hospital, and the cure was permanent two months subsequently. Case 3 was a case of secondary syphilis, with destruction of the bones of the nose, ulceration of the pharynx, and chronic laryngitis. In six weeks from the commencement of the chaulmoogra, this man left the hospital, stout and well. Besides the above, the author states that he has used the chaulmoogra in a mild case of ichthyosis, in three cases of syphilitic rheumatism, in two of scrofulous enlargement of the cervical glands, in two cases of elephantiasis of the face, and in an example of leucopathia of ten years' standing, with apparent benefit.

V. *The Preparation of the Caustic of Landolfi.* By M. QUEVENNE. (Rev. Méd.-Chir., p. 243. April, 1855.)

The proportions recommended by M. Quevenne are—chloride of zinc (deliquesced); chloride of antimony (deliquesced), chloride of gold, and chloride of bromine, of each 5 grammes; flour, 20 grammes; water, 18 grammes. The chloride of gold is triturated in a porcelain mortar with the chlorides of zinc and antimony; the water and half the flour are added, so as to make a somewhat liquid paste; the chloride of bromine is then added, and the whole mixed as quickly as possible with the rest of the flour. The operation should be performed in the open air, to avoid the inconvenience arising out of the vapours of bromine abundantly disengaged. The chloride of gold may be omitted without injuring the efficacy of the preparation. The application of the caustic may be rendered less painful by the addition of powdered opium to the mass.

VI. *Action of Baths and Douches of Carbonic Acid Gas.* By M. HERPIN. (Archiv. Générales, p. 630. May, 1855.)

The first impression made by the bath is an agreeable sensation of warmth, which is succeeded by a prickling sensation, a peculiar formication, and, at a later period, a burning sensation similar to that produced by the commencing action of a sinapism; old pains, especially those of old wounds, revive; the skin becomes red, an abundant perspiration occurs from those parts exposed to the action of the gas, and the urine is considerably augmented. The sensation of heat and the perspiration continue for several hours after leaving the bath. At first the action of the heart is only slightly accelerated, but this symptom becomes more marked if the bath is prolonged. The pulse is full and quick, the heat becomes burning, with turgescence and reddening of the skin, headache, thoracic oppression, &c. Prolonged for too long a period (several hours), the bath produces stupor, and the venous blood assumes a black colour; but after a bath taken in a proper manner, a person feels lighter and more active for several hours. The carbonic acid gas acts energetically upon the vascular and nervous system, and by its antiseptic properties promotes the cure and amelioration of wounds and unhealthy suppuration.

VII. *On the Production of Opium in Asia Minor.* By SIDNEY H. MALTASS, Esq. (Pharmaceutical Journal, p. 395, Mar. 1855.)

Mr. Maltass observes, that no crop is so uncertain as opium. The poppy seed is sown in a rich moist soil, improved by an abundance of manure, and ploughed till the soil is pulverized, immediately after the first autumnal rains till November, and even later in the highlands. It is sown broadcast; mixed with sand, to avoid throwing too large a quantity; and the field is subsequently harrowed. It is not customary for large landed proprietors to grow opium, nor would it pay them, on account of the difficulty they would experience in procuring labourers on hire. Every peasant either possesses or rents as much land as he and his family can cultivate, and grows opium on his own account. About May the plants flower, and a few days after the petals have fallen, the head or capsule is ready for incision. In this operation the whole family commonly takes part; it is performed in the afternoon of the day, and in the following manner:—A transverse incision is made with a knife in the lower part of the capsule, the incision being carried round until it arrives nearly at the part where it commenced; sometimes it is continued spirally to half-way beyond its starting-point. In this way it appears that the mode of incision differs from that adopted in India, where several oblique incisions are made. The following morning, the capsules are scraped, and the juice laid on a poppy leaf on the palm of the left hand, till a mass of sufficient size is obtained. If the dew has been heavy during the night, the yield is greater, but the opium of darker colour; if, on the contrary, there has been no dew, then the yield is less, and the opium of lighter colour. A high wind is prejudicial, as the dust raised from the pulverized soil adheres to the exudation, and cannot be separated. The capsules are cut but once. The average yield of a toloom (1600 square yards) of land is about $2\frac{43}{100}$ lbs. of opium and 200 lbs. of seed. After the opium is collected, the capsules are gathered, and the seed shaken out and carefully preserved, the straw being then given to the cattle. The seed is afterwards pressed in wooden lever presses, and the oil extracted: it is used for burning and for culinary purposes. The cake is given partly to cattle, and partly pulverized and mixed by the poorer families with their bread. The average yield of oil is 35 to 42 per cent. After the opium is collected, it is wrapped in poppy leaves, and dried in the shade. After the opium is purchased in the interior, it is put into thin cotton bags, and these into circular baskets. To most of the baskets, a proportion of *chicantee*, or inferior adulterated opium, is added, usually about five per cent. This chicantee is opium mixed with sand, pounded poppy capsules, half-dried apricots, and, in some instances, turpentine, figs or gum tragacanth of inferior quality. The baskets are sent to Smyrna, where they are stored in damp warehouses, to avoid loss of weight: they are sold without being opened, and it is only when they reach the buyer's stores that they are opened in presence of the seller and of a public examiner. The examiner then examines the opium, piece by piece. By constant practice, he can usually tell by the weight if the opium is pure; but any suspected piece is cut open, and if bad, thrown aside as chicantee. Sometimes chicantee is thrust in between two pieces of good opium; it is then cut out, and thrown aside. The strength and quality of opium is reckoned in carats, like gold, twenty-four carats constituting good opium; but according to custom, the examiner must pass as pure any which reaches twenty carats; so that, in purchasing opium, a difference of twenty per cent. may exist between the value of one basket and another. After the opium is examined, the tare is taken, including the chaffy seeds in which it was packed; these seeds, which are those of a species of rumex called Afion Oto, or opium weed, are afterwards returned to the buyer, to pack his cases. The purest opium is collected at Ushak, Bogaditz, and Simav; but the pieces are small, and stick together, which makes it unsightly. Karahissar and its environs produce one-third of the annual crop, but the quality is not good, and the pieces are usually larger. According to Mr. Wilkin, who had witnessed the collection of opium, a substance made by evaporating the juice of the grape, and thickening this with flour, is often used for adulterating opium. The value of an average annual produce of 100 tolooms of land may be stated at 20,000 piastres; the expenses of cultivation and tithes, 15,424 piastres; so that the average gain to the grower would be 4576 piastres (a piastre is worth about 2½*d.* sterling).

VIII. *On the Production of Indigenous Opium.* By M. Decharmes.

(Comptes Rendus, Oct. 16, 1854, and Jan. 3, 1855.)

The author advocates the cultivation of the poppy in France with a view to the manufacture of opium—first, because the opium obtained from the poppy (*l'œillette*) cultivated in the north of France contains never less than 13 and sometimes 18 per cent. of morphia, while Smyrna opium contains only from 5 to 9 per cent.; and secondly, because M. Bénard, of Amiens, has shown by experiment that the cultivation would furnish a fair amount of profit.

M. Decharmes has found in indigenous French opium of 1854 as much as 16 per cent. of morphine, while that of 1853 yielded only 14·75 per cent. He finds also that, in the combustion of opium or of morphia, there is not a complete decomposition of the alkaloid, but a partial sublimation of it. He concludes from this that it is the morphia which acts on the nervous system when opium is smoked.

IX. *On Tragacanth and its Adulterations.* By S. H. Maltass, Esq.

(Pharmaceutical Journal, p. 18, July, 1855.)

The small prickly shrub which produces tragacanth grows wild in many parts of Asia Minor, particularly in Anatolia. The gum is chiefly collected in Caissar or Kaisarieh (ancient Cæsarea), Yalavatz, Isbarta, Bourdar, and Angora. In July and August, the peasants clear away the earth from the lower part of the stem of the shrub, and make several longitudinal incisions with a knife in the bark; the gum exudes the whole length of the incision, and dries in flakes. Three or four days are sufficient for this purpose, and the gum is then collected. In some places the peasants also occasionally puncture the bark with the point of the knife. If the weather be hot and dry, the gum is white and clean; but if the atmosphere be damp, and the heat but moderate, the gum requires a longer time to dry, and assumes a yellow or brown tinge. High winds are favourable for drying, but the gum accumulates a certain proportion of earth. Whilst the peasants are engaged in this labour, they pick off from the shrubs the gum which exudes naturally, and it is this which chiefly constitutes the quality known in England as *Common* or *Sorts*. The mixed gum is sold to native merchants, who send it to Smyrna, where it is prepared by several pickings and siftings for shipment to Europe; several qualities being thus separated. Tragacanth is adulterated with Mossul and Caramania gum, collected principally, as Mr. Maltass is informed, from the wild almond and plum. As, however, neither Mossul gum (which is used to adulterate the better kinds of tragacanth) nor the Caramania gum occurs in flaky pieces, and as they are of a dark colour, especially the latter, the Caramania gum is broken into small irregular pieces, and whitened with *white lead*, and mixed with leaf gum to the extent of fifty per cent. To adulterate *sorts* or *common* gum, the Caramania gum is prepared in a similar manner, but the pieces are left larger; the proportion added is frequently 100 per cent. Mr. Hanbury has readily detected lead in the adulterated "*Small Tragacanth*," imported into the London market. The botanical source of the Mossul and Caramania gums requires further investigation.

X. *On the Poisonous Properties of Brine.* By M. Reynal.

(L'Union Méd., p. 254, May, 1855.)

The brine obtained from the process of salting various kinds of meat and fish is used by the lower classes in France as a condiment in place of common salt, and by farriers as a remedy for the diseases of domestic animals. Instances of poisoning, however, from its use having been noted in Germany, M. Reynal proceeded to investigate its action, and from a series of experiments detailed, draws the following conclusions:—1. That three or four months after its preparation, it acquires poisonous properties. 2. That the mean poisonous dose for the horse is two litres; for the hog, half a litre; and for the dog, one to two décilitres. 3. That in less doses it produces vomiting in the dog and hog. 4. That the employment of this substance mixed with the food, continued for a certain time, even in small quantity, may be fatal. These facts are important, when it is recollected that smoked meats and sausages have sometimes exhibited poisonous properties.

XI. *On Nicotin.* By Dr. Leonides van Praag. (Virchow's Archiv für Path. Anat. und für Klin. Med., Band viii. Heft 1, p. 56.)

From the experiments upon the action of this principle on mammalia, birds, frogs, and fishes, Dr. van Praag concludes, first, that Stas and Albers were incorrect in asserting that nicotin, topically applied, operates as a caustic irritant. The first effect of the poison upon the respiration is to increase its rapidity; but this increase is always followed by retardation, a fact which all former observers appear to have overlooked. This oversight seems attributable to the late period at which the retardation may take place. In one of Van Praag's experiments, the greatest fall in the frequency of respiration was observed at a period when all the other symptoms of poisoning had already ceased. In birds, there is indeed no retardation of breathing, but there is also no increase in its frequency. In twenty-one experiments, Van Praag on no one occasion observed increased rapidity of respiration without a subsequent retardation of it. Another important symptom, which was also observed by Bernard in his experiments, is a peculiar sibilus during respiration. This is attributed by Bernard to an over-active movement of the diaphragm; but Van Praag, with more probability, ascribes it to a contraction of some part of the air-passages, and suggests that its seat is the larynx, and that its muscles are thrown into a tetanic spasm, similar to that which affects other parts of the muscular system. The pulse is increased in rapidity by nicotin, but at a later period becomes slow or imperceptible. As respects the operation of the poison on the muscular system, all observers agree. In cases which do not proceed too rapidly, it is marked by very severe and frequently alternating tonic and clonic spasms, which attack different parts of the body, either simultaneously or consecutively. Subsequently to the convulsive stage occurs great debility, connected either with partial muscular trembling, or with a lively tremor of the whole body. In cases which run a rapid course, the convulsive state is often altogether wanting, and adynamia sets in at once, with tremor. In the most rapid cases of all, the muscles are not at all affected, and the animals sometimes die without any muscular movement. The influence of nicotin upon the sensory nerves varies; in some cases, pain is experienced on its application; in others, in the larger number of instances, none. And so, too, with respect to sensibility. In some instances complete anæsthesia was induced, while in others no alteration of sensibility was traceable. In all cases the pupils were dilated at first, in some at a later period contracted. Salivation occurred in many instances. Purging and vomiting only occurred in those cases which recovered; but recovery may ensue without vomiting. The excretion of urine was in general not remarkably altered. The duration of the poisoning varied with its severity. When very severe, death has occurred immediately, without a single symptom. Van Praag is unable to state the largest dose of nicotin which would not be dangerous to man; at all events, a dose of half-a-grain is not fatal. He thus sums up the operation of nicotin:—"The physiological operation of nicotin is at first stimulant, and at last depressing, not only to the circulation and respiration, but also to the nervous system. Accelerated circulation, increase of the respiratory movements, and excessive irritation of the muscular system, are the phenomena observed first; the concluding symptoms are those of general depression, both of animal and organic life." He recommends further investigation into the therapeutical applicability of nicotin to the treatment of the chronic skin disease and chronic inflammations.

XII. *On the Use of Aconite in Disease.* By Dr. K. D. SCHROFF. (Wochenblatt der Zeitsch. der Gesellschaft der Aerzte zu Wien, p. 281. April, 1855.)

Dr. Schroff draws attention to two conclusions which he drew from his experiments with aconite—viz., 1. That both aconite and aconitin in adequate doses produces in healthy men and in rabbits increased secretion of urine. 2. That they act remarkably in depressing the action of the heart, either immediately or after a brief increase of the heart's action. He now says that he has observed both these effects, also, on administering aconite in disease. He relates, by way of illustration, a case of pleurisy in which he gave it with these results:—Appropriate treatment had already lessened the fever, and reduced the frequency of the pulse to 100; but the urine remained scanty. On the 13th July, he began to give one-sixth of a grain of the alcoholic extract of the root of the Aconitum neomontanum four times a day. After the first six doses, the frequency of the pulse was reduced about six beats, and the urine became somewhat more abundant, lighter coloured, and less thick. The dose was now increased to one-third of a grain four times a day, and then the quantity of urine became increased in a very remarkable degree, simultaneously with a diminution of all the morbid symptoms, while the pulse sank to 50. He considers the employment of aconite adapted for those cases in which it is desired to reduce increased action of the heart, and mentions especially hypertrophy of the heart, aneurism of the aorta and large arteries, and effusion into the pericardium, pleura, &c. The latter half of the paper is occupied by the re-assertion of the conclusions derived from his physiological experiments, on which doubt has been thrown by Van Praag. He maintains his conclusions on the ground chiefly of his experiments on the human subject and rabbits, while Van Praag made no experiments upon the former, and only three upon the latter—in two of which death either occurred too rapidly for the diuretic effect to be observed, while in the third the dose given was smaller than Schroff has observed to produce this effect. In Schroff's experiments on the human subject and rabbits, large doses invariably operated in increasing the urine. In the former, the aconitin was given in doses of 0·02 to 0·05 grammes, while of the alcoholic extract 0·1 gramme was necessary. As to the reduction of the pulse, he asserts that, putting aside numerous experiments upon rabbits, this result occurred in 12 experiments made on the human subject with aconitin, and in 38 experiments made with different preparations of various parts of the plant, and of three varieties of aconite. Large doses, however, are necessary. The effect was first observed with doses of 0·01 gramme of aconitin, and increased proportionally with the increase of the dose; 0·1 gramme of the alcoholic extract was necessary, and 0·2 gramme of the watery extract.

XIII. *On Aconitin.* By Dr. LEONIDES VAN PRAAG. (Virchow's Archiv für Path. Anat. und Phy. Med., Band vii. Heft 3, 4, p. 438.)

The alkaloid employed by Dr. van Praag was obtained from Trommsdorf of Erfurt, who assured him of its perfect purity. It was prepared from the root of the blue variety of aconite indigenous to Switzerland. Experiments were made upon mammalia, birds, frogs, and fishes. From the examinations of the bodies of the poisoned animals after death, he saw no reason to conclude that aconitin produced gastro-enteritis; neither do his examinations lead him to place prominently forward, as Schroff has sought to do, a non-coagulable state of the blood as a symptom of poisoning by aconitin.

As to the physiological operation of aconitin, the general conclusions drawn are —"that aconitin exercises a retarding influence upon the respiration, a paralysing operation on the voluntary muscular system, and a depressing influence upon the brain." A retarding operation on the circulation was less marked than in the experiments of Schroff, and he concludes "that aconitin varies very greatly in the frequency with which it induces a reduction of the pulse." In general it produces dilatation of the pupils; Schroff says that at the commencement of the experiment the pupil exhibits great variability, and from time to time even becomes contracted, but that this at length always terminates in dilatation. Salivation and increased excretion of urine must be regarded as amongst the less constant symptoms. Schroff describes as occurring in the human subject a peculiar contractile, compressing, even painful, sensation in the cheeks, over the jaws and forehead—in short, in the parts supplied by the trigeminous nerve. The only objective symptom observed by Van Praag that could be explained by such a sensation, was licking of the mouth, which was noticed in two cases. Where death occurred suddenly it was by asphyxia; but in cases where it was deferred for some time, the animals died apparently from exhaustion. From one experiment made with the alcoholic extract of aconite, it was observed, that while for the most part its action agreed with that of the alkaloid, the symptoms referable to the stomach and bowels were more severe, and gastro-enteritis was moreover induced.

Judging from its physiological operation, Dr. van Praag would consider aconitin adapted to those cases of delirium and mania which proceed from over-irritation. Perhaps, also, he suggests, it might be tried in severe tonic or clonic spasms, tetanus, trismus, chorea, and pure spasmodic asthma. He sums up thus his observations on its therapeutical applicability:—1. Aconitin operates much in the same way as the alcoholic extract of aconite, and is therefore to be recommended in those diseases in which this remedy has been proved to be serviceable. 2. Aconitin is far preferable to any other preparation of aconite, on account of the unchangeable nature of the well-prepared alkaloid, whereas the activity of the aconite, and consequently of its ordinary preparations, varies with a number of circumstances—such as the locality in which it grows, the year, &c. 3. Aconitin is wanting in the undesirable acridity of the extract, and consequently it exerts only the favourable operation of the extract without its injurious accessories.

XIV. *Experiments on the Operation of Loss of Blood upon the Course of Poisoning by Strychnine.* By W. Kaupp. (Vierordt's Archiv für Phys. Heilk., Heft 1, p. 145. 1855.)

The object of these experiments was to test the doctrine which, since the known experiments of Magendie, has been universally held, that the rapidity of absorption, and thus of the operation of poisons, was lessened by a full condition of the vascular system, and increased by loss of blood. The method of experiment adopted was the introduction of a solution of nitrate of strychnine ($\frac{1}{32}$nd of a grain he found best adapted for the purpose) beneath the integument in the back of rabbits. Dr. Kaupp compares the rapidity of poisoning in those animals which were not bled, and in those which were bled from the jugular vein before and after the application of the poison. He furnishes the results in some tables, of which the following may be regarded as a *résumé*:—

In the case of those rabbits which were not bled, the tetanus set in earlier than in those which were bled—viz., in a mean period of 4 mins. 30 secs.; while in those which were bled, in an average time of 5 m. 13 s., giving thus a difference of 43 s. The weight of the animal seemed to exert a marked influence upon the early or late occurrence of the tetanus. Comparing the results in the six heavier and six lighter unbled animals, the average time of occurrence of the tetanus in the former was 5 m. 45 s., and in the latter 3 m. 36 s. This result is much more striking in the instances of those animals which were bled, in which the tetanus on the average occurred in the heavier after 36 m. 48 s., but in the lighter after 14 m. 18 s. Much more striking even than the time of occurrence of tetanus, was the difference between the periods of death in the bled and unbled animals. The average time which elapsed before death in those not bled was 9 m. 39 s., while in those which were bled it was 27 m. 56 s. The weight of the animals here also exerted an influence—the mean period of death (taking the bled and unbled together) was 23 m. 12 s., after application of the poison, for the heavier, and 13 m. 56 s. for the lighter animals. The sex of the animals also seemed to exert an influence, the males dying on an average in 15 m., and the females in 25 m. The amount of the venesection exerted an influence, the proportion of the duration of poisoning after a large and small bloodletting being as 4 : 2½. It was further observed, that when the animal was placed in a small basket, and thus hindered from springing about, both the occurrence of the poisoning and death were delayed.

The results of these experiments are thus directly opposed to the doctrine generally accepted, and show that both the occurrence of the symptoms of poisoning and the death (using these as the measure of the rapidity of absorption) are really delayed by loss of blood.

This is a very important paper, and the subject well deserves following up, since the conclusions of this experimenter, if confirmed and extended, must lead to therapeutical reforms.

XV. *On the Effects of the Baths of Creuznach in Female Complaints.* By Charles Engelmann, M.D. (Edin. Monthly Journal, p. 483. June, 1855.)

This paper professes to be the result of more than fifteen years' experience of the use of the baths at Creuznach.

Diseases of the Mammæ.—"Indurations of particular glands," arising from inflammation of the breast during the period of suckling or at the time of weaning, or produced by hyperæmia, or else attributable to some mechanical injury, such as a blow or pressure, or, lastly, resulting from cold or anomalous menstruation, may confidently be expected to be dispersed by the use of the baths. In "glandular hypertrophy" the degree of success depends on the duration rather than on the size of the tumour: when not above a year's standing, a single course of the baths has frequently dispelled it,—otherwise, two or three courses may be necessary. In every instance of "ectasia of the milk-vessels," a decrease of the swelling took place, and in some cases, total dispersion. To the class of tumours which cannot be dispersed, but which otherwise derive benefit from the baths, belong "sarcoma and cysto-sarcoma of the breast," and "simple cysts and cystoids." The reduction in the size of the swelling here takes place by the absorption of the hypertrophied cellular substance surrounding the tumour. "Scirrhous tumours" are also reduced in size by absorption of the cellular tissue, but are as incurable by the baths as by any other medical treatment.

Affections of the Ovaria.—The baths must be considered as highly pernicious in cases of "cancer," or where great exhaustion exists and hectic fever has manifested itself. They are unsuitable, but not injurious, in "cysts of the ovaries, ovarian dropsy, cysto-sarcomatous concretions, and alveolar degeneration." All the cases in which they were beneficial were "solid tumours." All swellings of the ovaria due to "real hypertrophy, or effusion of blood in the tissues," or "fibrous tumours," are capable of absorption if treated at a period when they are not developed in too high a degree. We may advise the use of the baths in those cases where there is no "hydræmia," and when the constitution is not suffering more than can be accounted for by the presence of the tumour on surrounding organs. The good effects will be proportional to the shortness of duration of the disease. In feebly developed disease the full effect cannot be judged of till three or four months have elapsed from the termination of the course. Mother-lye is here always added to strengthen the bath.

Affections of the Uterus.—Those which can be efficaciously treated by the waters are "chronic engorgements and indurations" of a benignant character, affecting the whole uterus or some parts of it, and "hypertrophy of the uterus" accompanying "fibrous tumours." The os uteri was commonly the seat of the idiopathic benignant engorgements and indurations, and in some cases Dr. Engelmann prescribed the uterine douché in addition to drinking and bathing. There was only one case of chronic idiopathic engorgement of the whole uterus in which the diagnosis was clear. Among the idiopathic indurations there were only two in which the fundus uteri only was enlarged and indurated, and both were complicated with retroversion. In one case the swelling was reduced, and though, from adhesion, re-position was impossible, yet the symptoms became tolerable. In the other the tumour diminished, and all the symptoms improved. The greatest number of affections treated were "hypertrophy" produced by "fibrous tumours." The tumours can be absorbed when not actively of a cartilaginous structure. All the cases met with had already existed for years, and many remedies had been resorted to in vain. The extent of the cure was here again proportional to the size, hardness, and duration of the tumour. Tumours of a cartilaginous structure were never dissolved by the waters; only a diminution of the enlargement of the uterus took place. On some tumours of the size of a walnut, the baths acted so powerfully that they could not be felt at the end of the course. The swelling and softening of the tumours are regarded as indications of commencing absorption. Mother-lye is here also added to the baths.

XVI. *Remedies for Intermittent Fever—Substitutes for Quinine.*

Parsley Oil (*Apiol*).—MM. Joret and Homolle* state that parsley oil, in doses of fifty centigrammes to one gramme, determines a slight cerebral excitement similar to that produced by coffee, with epigastric warmth, and a sense of strength and comfort. After doses of two to four grammes, phenomena of intoxication are observed, scintillations, dizziness, vertigo, hissing in the ears, frontal headache, &c. They compare these symptoms with those which follow a strong dose of sulphate of quinine. It is only exceptionally that they have found borborygmi, nausea, and colic, with bilious diarrhœa, to supervene. They also consider that it is emmenagogue, and they place it in the class of tonics.

In discussing its applicability to the cure of intermittents, they describe briefly the particulars of forty-three cases treated by M. Lefévre at Rochefort, M. Dupré at Bourg-en-Bresse, M. Dénis at the hospital of Auray, M. Fernet of Paris, and by M. Amic in Martinique. Of this number, thirty-seven were cured and had no relapse; and in six, though the fever was not removed, yet it was modified in intensity. Of these forty-three cases, twenty-one were quotidians, eighteen tertians, and four quartans; five quotidians and one quartan resisted the remedy—all the others were cured. The writers consider that a proportion of cures thus amounting to eighty-six per cent., suffices to prove the value of parsley oil in indigenous intermittents. As respects the intermittents of hot countries, they group together the observations accumulated by a Commission of the Society of Pharmacy to test the substitutes for quinine at Rome, Perpignan, and Ajaccio, with those of Dr. Amic of Martinique. Of thirty cases thus treated, sixteen were cured; nineteen of these were quotidian, of which twelve were cured; ten were tertians, of which four were cured; and one quartan, which was not cured. The conclusion drawn is, that if parsley oil be not of equal value with quinine in treating the intermittents of hot climates, it may yet be very well substituted for that remedy in indigenous intermittents, and they consider that it may also prove serviceable in intermittent neuralgia, and the night sweats of phthisis.

Sulphate of Cinchonine.—M. Hudelet† having used this salt very extensively, has arrived at conclusions respecting its value quite at variance with those of M. Torget, who, after administering it in ten cases, only found it efficacious in three. M. Hudelet administered it in quantities similar to those in which he has administered the sulphate of quinine, in order that a fair comparison might be instituted—viz., thirty centigrammes. He has, however, combined it with ten to twenty drops of laudanum, given in three or four doses. The following is a summary of his results:—1st. In five hundred and seven cases of every type of intermittent, the treatment has only been unsuccessful in nine. 2nd. In the doses above noticed, neither the digestive nor cerebral organs have been in any way disordered by it. 3rd. The relapses have been neither more nor less numerous than those after sulphate of quinine. 4th. It has acted as quickly as the sulphate of quinine. 5th. Its action on the spleen is the same as that of the sulphate of quinine—i.e., none at all on spleens enormously enlarged (five to ten kilogrammes), but very marked in less voluminous, and especially very recent, engorgements. 6th. It is the only substitute proposed during the last ten or fifteen years which has furnished M. Hudelet with satisfactory results. It is preferable to the sulphate of quinine also as being half the price. He has found a small dose, taken each morning by labourers exposed to malarious poison, prove preservative against fever.

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* L'Union Médicale, Jan. and Feb. 1855.

† Rev. Méd.-Chir., p. 33. Jan. 1855.

Olive leaves are no new remedy for intermittents, but attention has of late again been drawn to them by Mr. S. H. Maltass.* He states, in a letter to Mr. Daniel Hanbury, that in 1843, when fever and ague of the worst description were raging in the island of Mytelene, the quinine being exhausted, he commenced the administration of a decoction of olive leaves, made by boiling two handfuls in a quart of water down to a pint. Of this he gave a wineglassful every three or four hours with remarkable success. He has since informed Mr. Spencer Wells that he has even found it more effectual than quinine.

Quiniodine.—Dr. Da Costa† furnishes in a tabular form the notes of fifty-three cases of intermittent treated by *Quiniodine.* In many of these it is said the disease was of long standing; the chills were arrested in forty-nine cases by the first administration of the medicine, only four requiring a repetition of the dose. In ten cases the disease returned. The doses in which it was given varied for adults from sixteen to forty grains. The average dose was twenty grains, six of which were given shortly before the expected paroxysm, while the rest was taken during the intermission. These doses did not give rise to headache, ringing or buzzing in the ears, nor to sickness.

Oxalate of Iron.—Dr. Gamberini‡ recommends the use of an oxalic ferruginous lemonade, prepared according to the following formula:—Take of sulphate of iron ℈ss, oxalic acid gr. vj, distilled water ℔iij, white sugar ℥iss—mix. An oxalate of iron results, of a pale yellow colour, and nearly insoluble in water. This quantity is given in divided doses during the apyrexia.

XVII. *On the Treatment of Neuralgia by the direct Application of Opiates to the painful parts.* By ALEXANDER WOOD, M.D. (Edin. Med. and Surg. Journal, pp. 265, April 1, 1855.)

This communication contains the results of the injection of a solution of morphia or opium, by means of the fine syringe constructed for injection of nævi, &c., with perchloride of iron, into the cellular tissues of the part where the neuralgic pain appears to start from, and which is most sensitive to pressure. Dr. Wood relates eleven cases, two of which were treated by Dr. Thomas Wright. In several of them vomiting followed shortly after the injection; in one case the injection failed to give any relief.

The conclusions drawn by the author are—1st. That narcotics injected into the

* Pharmaceutical Journal, p. 353, Feb. 1854; Med. Times and Gaz., p. 433, May, 1855.

† Philadelphia Medical Examiner, p. 295, May, 1855.

‡ Bullettino delle Scienze Mediche, p. 134, Feb. 1855.

neighbourhood of the painful point of a nerve affected with neuralgia, will diminish the sensibility of that nerve, and in proportion diminish or remove pain. 2nd. That the effects of narcotics so applied are not confined to their local action, but that they reach the brain through the venous circulation, and there produce their remote effects. 3rd. That in all probability, what is true in regard to narcotics would be found to be equally true in regard to other classes of remedies. 4th. That the small syringe affords a safe, easy, and almost painless method of exhibition. 5th. That, destitute as we are of any precise experiments as to the applicability of the cellular tissue as a medium for the reception of medicinal agents, the experiments made with the syringe show that it seems to offer an excellent surface for the absorbent action of the venous system. 6th. That the method now detailed seems as extensively applicable as any of the methods of applying remedies to the skin, whether enepidermic, iatraleptic, endermic, or by inoculation.

XVIII. *On the Changes produced in the Blood by the Administration of Cod-liver Oil and Cocoa-nut Oil.* By THEOPHILUS THOMSON, M.D. (Proc. Roy. Soc., No. 3, p. 41, 1854.)

The author has found that, during the administration of cod-liver oil to phthisical patients, their blood grew richer in red corpuscles, and he refers to a previous observation of Dr. Franz Simon to the same effect. The use of almond oil and of olive oil was not followed by any remedial effect, but from cocoa-nut oil results were obtained almost as decided as from the oil of the liver of the cod, and the author believes it may turn out to be a useful substitute. The oil employed was a pure cocoa oleine, obtained by pressure from crude cocoa-nut oil, as expressed in Ceylon and the Malabar coast, from Copperah, or dried cocoa-nut kernel, and refined by being treated with an alkali, and then repeatedly washed with distilled water. It burns with a faint blue flame, showing a comparatively small proportion of carbon, and is undrying. The analysis of the blood was conducted by Mr. Dugald Campbell. The whole quantity abstracted having been weighed, the coagulum was drained on bibulous paper for four or five hours, weighed, and divided into two portions. One portion was weighed and then dried in a water-oven, to determine the water; the other was macerated in cold water until it became colourless, then moderately dried and digested with ether and alcohol to remove fat, and finally dried completely, and weighed as fibrin. From the respective weights of the fibrin and the dry clot that of the corpuscles was calculated. The following were the results observed in seven different individuals affected with phthisis in different stages of advancement:

	Red corpuscles.	Fibrin.
First stage, before the use of cod-liver oil—		
Female	129·26	4·52
Male	116·53	13·57
First stage after the use of cod-liver oil—		
Female	136·47	5·00
Male	141·53	4·70
Third stage after the use of cod-liver oil—		
Male	138·74	2·23
Third stage after the use of cocoa-nut oil—		
Male	139·95	2·31
	144·94	4·61

XIX. *On Injection of the Bronchial Tubes and Tubercular Cavities.* By Dr. HORACE GREEN. (Amer. Med. Mon., p. 5, Jan. 1855.)

Dr. Green having satisfied himself of the practicability of passing not only his sponge, but an elastic catheter, into the trachea, has proceeded to the injection of solutions of nitrate of silver, of as great a strength as forty grains to the ounce of water, into the lungs, and appears to consider that he can pass the tube into either bronchus at pleasure. The evidence, however, both of this last point and of the injection of a tuberculous cavity, is very defective. He injects as much as one to three drachms of the solution at a time, and does not hesitate to repeat it daily, or at intervals of two or more days. He has used it in several cases of bronchial and tuberculous disease, and, as would appear from a few cases briefly detailed, with almost immediate relief to the thoracic symptoms; and he states that the effects of the medication have been invariably salutary. He has used it in more than twenty cases of chronic bronchitis, some of a very severe and protracted nature, injecting every few days from one to three drachms, and in every case with relief. In those cases where tubercles exist, crude or softening, the beneficial effects of the treatment have been thus far as uniform and certain, though the improvement has not been as rapid in these as in the former cases.

XX. *On the Treatment of Epilepsy by Indigo.* By Dr. HUBERT RODRIGUES. (Rev. Méd. Chir., April, p. 198, 1855.)

The writer directs attention to the fact, that the cures obtained by indigo in the hands of M. Ideler, at La Charité Hospital, in Berlin, were effected by very large doses of the medicine; and expresses a belief that the want of success of others—as of M. Rech, at Montpellier—arose from administering it in too small quantity. The difficulty of following M. Ideler, however, in his practice, arises from the intense repugnance of patients to the continuance of such doses as he gave. M. Rodrigues' experiments with the medicine were made upon eleven epileptics. Four of these took the medicine according to the Berlin formula—viz.:—Powdered indigo, 15 grammes; aromatic powder, 2 grammes; simple syrup sufficient to make an electuary. To four others it was given in pills, or suspended in water. To the first he administered, at first, half the dose; and then, at the end of some days, the entire dose, increasing it gradually, according to the tolerance, to 60 grammes and more, *per diem*. To the second, the dose was constantly much less, commencing with a gramme or a gramme and a half a day, and not exceeding 30 grammes. The remaining three patients were treated by a mixed method, which he regards as adapted to most chronic cases. This plan consists in making a marked impression upon the system at first, by means of a sufficient dose, which is carried as far as possible during the first five or six days, and then in sustaining the action of the remedy by small doses, which have the advantage of being readily borne during the necessary period, reviving the therapeutic influence of the drug at regular intervals by the repetition from time to time of the first large doses, which may even be increased if necessary.

When administered by the first method, the indigo at first produced intense disgust, nausea, and vomiting. From the twelfth to the twentieth day, borborygmi, colic, and diarrhœa set in, the stools—serous, pultaceous, and blackish—varying from three to six in the day, but without lessening the patient's strength; the urine, coloured like the stools, was not increased, nor altered in taste or odour, and chemical analysis discovered nothing special in the secretion. The fits were immediately lessened in frequency and violence. In two who were children, aged ten and twelve years, a radical cure was effected. In the other two, who were adults, the disease recurred. The duration of the treatment was three or four months, and the quantity of the drug taken varied from 900 to 1500 grammes.

The patients of the second category were two females, a young man, and a child. The small dose of the indigo—1 gramme per diem to commence with—induced nausea from the first, but no vomiting. On the fifteenth day, the dose being three grammes, the stools and urine exhibited the bluish colouration. About half through the second month, while at a dose of fifteen grammes, the child lost appetite; suffered from spasms referred to the base of the chest, and vomiting. After some days at 30 grammes, all presented diarrhœa. The treatment was continued to the fourth month, the repugnance to swallow it increasing; and no benefit, or scarcely any, resulted from its use.

Two months subsequently, he again commenced treating the child, who had a fit every three days. He gave 8 grammes the two first days, 15 grammes the third and fourth, and 30 grammes the fifth and sixth, allowing roast meat and wine as diet. Nausea, slight colic, blackish stools, and coloured urine occurred. At the end of the week, the attacks were trifling. On the seventh day, the dose was reduced to 1 gramme, and continued thus till the twentieth day. The attacks were now replaced by a sort of absence of mind, which passed off in a moment. On the twenty-first and two following days, 40 grammes per diem were given, and then the 1 gramme doses returned to till the end of the month. The epilepsy had completely ceased. The same treatment was continued during the second month. A fall which the child had did not renew the disease. In the third month, 5 décigrammes per diem were given; and 20 grammes on two occasions at ten days' interval. A complete cure was accomplished. Two adults were treated with complete success in a similar manner.

Commercial indigo was used, which contains, among other foreign matters, an albuminous substance resembling leucine or casein, and to the presence of these matters it is that chemists attribute the production of valerianic acid when fused potash acts upon indigo. Is valerianic acid formed during digestion of the indigo in the stomach, and is the curative operation of indigo due at all to such a change?

XXI. *On a New and Easy Method of Cleaning the Skin after the Removal of Plasters.* By Professor FORGET. (Rev. Méd. Chir., May, 1854, p. 309.)

This method consists simply in placing upon the part contaminated with the plaster some very dry linen, and over this a napkin sufficiently warmed, applying it accurately, and pressing upon it for a moment with the flat of the hand, then removing the linen just as the original plaster was removed. The matter of the plaster adhering more strongly to the linen than to the skin, leaves the latter perfectly clean after two or three repetitions of this manœuvre.

[We are obliged to postpone the remainder of the present Report for want of room.—ED.]

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

By EDWARD H. SIEVEKING, M.D.

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, ETC. ETC.

I. *Instantaneous Hemiplegia in a Child.* (L'Union Médicale, Sept. 22, 1855.)

[Want of space compels us to omit the details of this and several other interesting papers, of which we give the titles as a reference.]

II. *Cases of Obturation of the Pulmonary Artery by Fibrinous Coagula.* By Dr. KLINGER, of Würzburg. (Vierordt's Archiv für Physiologische Heilkunde, Jahrgang xiv. Heft 3.)

The occurrence of fibrinous coagula in the pulmonary and other arteries during life, has attracted the attention of numerous observers, since the appearance of Paget and Virchow's memoirs on the subject. Dr. Klinger details eight cases, presenting fibrinous deposits in the pulmonary artery, six of which he observed himself. He divides them into two classes. The first comprises the cases in which coagula are found in the ramifications of the second and third order, which are the more common, and are met with as the concomitants of other diseases, such as hepatization, tuberculosis, apoplexy of the lungs, and pleurisy. These cases are attributable to the disturbance and arrest of the circulation caused by the mechanical impediment. The second class includes those cases in which the trunk or the primary divisions of the pulmonary artery are obliterated, and which are of much less frequent occurrence, while the symptoms accompanying them are of a very violent character. The author attributes them to morbid processes affecting the parietes of the vessel, or to the arrest of fibrin, which has been detached at other points of the circulating apparatus, and carried into the pulmonary artery.

The following is a summary of the cases detailed by Dr. Klinger; the first four he regards as belonging to his first class; the last four as being instances of the second class:

1. A female lunatic, aged fifty, affected with albuminuria, dropsy, constriction of the mitral orifice, and bronchial catarrh, was seized, three days before her death with sudden paroxysms of asthma, during which the face and extremities became cold, the radial pulse disappeared, and the expectoration was nummular, somewhat frothy, viscid, and dark-red. These symptoms recurred twice a-day, lasting two hours each time. An attempt at venesection was followed by the discharge of only a few drops of blood. In addition to the mitral disease and granular kidneys, the autopsy revealed several apoplectic spots at the base of both lungs; the small branches extending from these spots towards the trunk of the pulmonary artery were filled with coagula; there were similar coagula in other vessels of the same calibre, not connected with the apoplectic spots.

2. Eve B., a married woman, aged sixty, was admitted into the Würzburg Hospital on the 3rd Jan. 1852, affected with emphysema, bronchitis, bronchial dilatation, and dropsy. She had been subject to catarrh and dyspnœa for several years. The symptoms were cough; great dyspnœa; lividity of the face and mucous membranes; extremely feeble pulse; scanty, non-albuminous urine; percussion of thorax sonorous; respiration accompanied by every variety of noises; heart sounds at first normal, subsequently presenting a systolic murmur over the left ventricle, which again disappeared. The patient lingered until the 2nd of March, the symptoms becoming more and more aggravated. At the autopsy on the 4th of March, the lungs were found universally adherent; in parts, emphysematous and anæmic; in other parts, œdematous and congested; in others again, spots of chronic pneumonia; the bronchi hyperæmic, containing a muco-purulent secretion; some much dilated. Several of the smaller branches of the pulmonary artery obstructed with coagula. The heart was atrophied, with thickening of one edge of the mitral valve. With exception of an atrophied spleen, and of the lining of the coats of the stomach, there was no pathological change in the other organs of any consequence.

3. Marianne M., aged thirty-eight; single; previous health good; admitted into the Würzburg Hospital on the 10th Jan. 1852. Extreme prostration, livid countenance, some dyspnœa and cough, sleepless nights, normal percussion of the thorax, various rhonchi on both sides, nothing abnormal about the heart or other organs. She improved somewhat. On the 26th January, sudden fever and great dyspnœa, with a lancinating pain on the right side of the thorax, supervened. On the 27th and 29th she had two extremely severe suffocative attacks, which lasted several hours; dulness was detected on the right side of the thorax, posteriorly and below; the vesicular murmur had disappeared, and there was feeble bronchial breathing under the scapula, and an absence of vocal vibration. On the 30th January, at nine A.M., a violent attack of dyspnœa, the muscles of the neck working violently; great anxiety; slight œdema of the legs; heart sounds normal, beat very feeble; pulse 120; respirations 48. Urine scanty, highly albuminous, containing fibrinous casts and renal epithelium. No material change took place up to the period of death, on the 3rd Feb., except that the patient suffered less

during the last three days. The intellect remained clear to the last. In addition to copious serous exudation into the right pleura, with copious fibrinous flakes, the middle and lower lobes of the right lung were in various stages of inflammation; several smaller branches of the right pulmonary artery were obliterated by coagula. The pericardium contained five ounces of yellow serum; the mitral valve was contracted; the liver presented the nutmeg character, and the kidneys contained numerous cysts, and were otherwise diseased.

4. Kunigunde N., aged forty, had always suffered from thoracic affections; for three years she had been subject to œdema; for six weeks previous to admission, on the 21st November, 1851, the symptoms had become aggravated; the dyspnœa was intense, the dropsy had increased, respiration was sibilant, but heart and kidneys apparently normal. Icterus supervened before death, which ensued after the lapse of five weeks. *Autopsy:* The pericardium was distended with eighteen ounces of a pale icteric fluid; the heart large and dilated; the auricles and ventricles filled with coagula; valves normal. The inner surface of the pulmonary artery, and especially the divisions of the first and second order, presented much atheromatous deposit, of which very little was found in the aorta; the smaller branches of the pulmonary artery (of the size of a goose-quill) were obliterated with fibrin. The lungs presented adhesions at the apices; there was some serum in the left pleura; and in the right one, a large sacculated effusion at the base, which compressed the base of the lung. The liver presented nutmeg degeneration; and there were extensive adhesions between various parts of the peritoneal expansion.

In the following cases, the obliteration of the pulmonary artery appears to have commenced at the trunk of the vessel:

5. John R., miller, aged thirty, was taken suddenly ill the day before admission, on the 5th May, 1854. There was high fever, quick breathing, and dyspnœa, and coarse vesicular breathing. The symptoms yielded to treatment; but on the 14th of May, at 11 P.M., there was sudden and intense dyspnœa, heart impulse and radial pulse almost imperceptible, and not to be counted; no results obtained by auscultation and percussion. No affection of the sensorium took place, and patient died on the 16th of May, the symptoms gradually becoming more urgent to the last. *Autopsy:* Brain normal; heart enlarged, full of black syrupy blood; the valves normal. The pulmonary arteries were choked up from the trunk towards the smaller divisions, with large, slightly-adherent, slate-coloured coagula; in the left trunk a cicatrix was found which indicated the previous existence of a similar process; the lungs were otherwise healthy. All the larger veins were much distended. An organized coagulum was found in a small ileo-lumbar vein; and, at the point where it opened into the hypogastric vein, there was a projecting, slate-coloured substance, which Dr. Klinger considers as favouring the assumption that the coagulum had first formed here, and been afterwards carried into the pulmonary artery. The liver, spleen, and mesenteric glands were hypertrophied.

6. Ann S., aged sixty, while recovering from chronic hepatitis and bronchitis, died suddenly, and presented a colourless, firm, unadherent coagulum at the point of bifurcation of the pulmonary artery.

7. This is a case* in which a primipara, aged twenty-one, three days after parturition, was seized with phlebitis of the left extremity; during convalescence, she suddenly uttered a scream, fell, and expired. The left crural vein and its branches were found obliterated with coagula, extending up to the junction of the crural and iliac veins. The pulmonary artery presented similar coagula, which could be traced into the smaller ramifications.

8. This case is also quoted from another observer,† and is very briefly given. Sudden death occurred in a case of pleuro-pneumonia of the left side, and the post-mortem exhibited a firm coagulum in the pulmonary artery, besides extensive hepatization, together with chronic catarrh of the ileum and colon.

* Reported by Dr. Hoogeweg, in the Preussische Vereinszeitung, 52. 1851.

† Canstatt's Klinische Rückbliche, Heft 2.

Dr. Klinger's interesting paper concludes with some remarks on the diagnosis of the morbid condition which the cases illustrate, but for these we have no room.

III. *On Angina Membranacea as a Complication of Typhoid Fever.* By M. OULMONT, Physician to St. Antoine. (Revue Médico-Chirurg. July, 1855.)

In the history of epidemics, the complications that present themselves at different times and in different countries, form an important and interesting feature. They assist us in the interpretation of the morbid phenomena of the main disease, as of the peculiarities generally characteristic of the period or the nation. The greater frequency of diphtheritic exudation in France generally, prepares us to meet with it as a complication with other diseases in that country, though typhoid fevers would not be the class in which we should have expected to find it. Its occurrence in that combination must be rare, even in France; for Trousseau states that it has not occurred in his experience. M. Oulmont, during the typhoid epidemic of the winter of 1855, met with 6 cases at the Hôpital St. Antoine, in which croup supervened in 2, respectively, on the fifteenth, sixteenth; in 2 on the twenty-second; in 1 on the twenty-sixth day of the fever; and in 1 on the fifteenth day of convalescence. Five proved fatal. The main symptom by which its supervention was indicated, was dysphagia. A few hours sufficed to cover the pharynx with false membranes, and in 2 cases they extended into the larynx. In these cases tracheotomy was performed, but without success. In the 5 fatal cases, in addition to the grey, soft membranes lining the gullet, the lesions of the intestines characteristic of typhoid fever were discovered. M. Oulmont states that, at the time the diphtheritis attacked the hospital inmates, it was epidemic in the environs of the hospital, but it is remarkable that it affected only the typhoid patients. The author ascertained positively that 3 of the patients attacked had held no communication with others labouring under diphtheritis; with regard to the others, he obtained no data. At all events, if contagion exerted any influence, the period of incubation must have been very long, as the disease occurred respectively on the fourth, sixth, seventh, eighth, eleventh, and thirty-eighth days of the admission into the hospital. The treatment consisted in emetics, followed by tonics and restoratives, with the local application of caustic—but, as we have seen, with but a poor result.

IV. *On Concentric Hypertrophy of the Left Ventricle of the Heart.* By ROBERT LAW, M.D., Professor of the Institutes of Medicine in the School of Physic in Ireland. (The Dublin Quarterly Journal of Medical Science, Nov., 1855.)

The object of Dr. Law is to prove the existence of permanent concentric hypertrophy of the left ventricle of the heart, in opposition to the views of those who regard it solely as a post-mortem effect, and to explain the manner in which it is produced. He considers that it may occur under three conditions:—1. When both the aortic and mitral valves are diseased; 2. When the mitral valve is affected, while there is, at the same time, some distant obstruction in the course of the circulation; 3. When there is distant obstruction, to which is superadded a diminution of the mass of the blood. In the case of concentric hypertrophy, the law of the economy comes into play, by which the permanent capacity of the bloodvessels accommodates itself to the quantity of fluid habitually contained in them; hence, argues Dr. Law, "the lesion of the heart, which supplies us with perhaps the most striking proof of the capacity of a cavity accommodating itself to the quantity of blood habitually sojourning in it, is aortic valve disease, when it not only prevents the ventricle discharging its contents, by the obstruction which the diseased valves present, but when they are so diseased as to allow free regurgitation." A case is given in corroboration of Dr. Law's views.

V. *On Hydatids in the Thoracic Cavity.* By Dr. VIGLA. (Archives Générales de Médecine, p. 282, Sept. 1855, and p. 523, Nov. 1855.)

C. R., a cattle driver, aged thirty-two, was admitted into the Municipal Maison de Santé, to which Dr. Vigla is attached, on the 20th November, 1853. He had been subjected to deprivation, but had always enjoyed good health until, fifteen months previously, he was thrown down by a bull, whose horn grazed his scrotum, while one foot of the animal trod on the right side of his chest. From that time he had laboured under dyspnœa, and pain in the right hypochondrium, unaccompanied by fever, cough, expectoration, or hemoptysis. On admission, the dyspnœa and pain were urgent, the voice feeble, and the whole right side of the thorax much enlarged; the intercostal spaces being obliterated, the subcutaneous veins dilated and prominent. The right side was 3·8 centimetres larger on the level of the seventh dorsal vertebra than the left. With exception of the first intercostal space, the entire right half of the thorax was dull, and the dulness extended over the right hypochondrium to a level with the umbilicus, and was bounded to the left by a line drawn from the umbilicus to the left armpit; the upper boundary of the dull space was limited by a curved line descending from the left armpit to the right intercostal space. The corresponding part of the side and the lower right back were also found to be dull. Throughout this dull space no respiratory or bronchial murmur was audible, nor was any vibration or vocal resonance perceptible. The heart sounds were heard only in the left armpit, but in no way abnormally altered. The lower right intercostal spaces presented a sensation resembling fluctuation. Dr. Vigla diagnosed the affection as an hydatid cyst; the only alternative lay between that disease and cancer; he excluded the latter, from having always observed a remarkable increase in the respiratory and cardiac murmurs accompanying the formation of thoracic cancer. We have seen that the very reverse of this symptom existed, and that the pulmonary and cardiac sounds were not transmitted at all. Besides, there was no symptom of cancerous cachexia.

Dr. Monod, on the 9th of December, at the request of Dr. Vigla, made an exploratory puncture between the sixth and seventh right ribs. Clear water was evacuated, which did not affect litmus, and was not changed by nitric acid; débris of transparent membranes floated in the liquid, which were recognised by M. Robin as the characteristic laminæ of an hydatid vesicle. Dr. Monod having evacuated above 2450 grammes (about 80 oz.) of fluid, injected a weak solution of iodine and iodide of potassium; a bandage was applied, and no inconvenience followed.

Directly after the operation, the heart was found to have approached nearer to the median line, and the clear resonance was much extended below the clavicus and on the left side of the thorax. On the 10th of December the heart was found to occupy its normal position, and the normal respiratory murmur was heard throughout the lateral and posterior portions of the left side. We cannot follow Dr. Vigla through his entire details and reflections on the case; suffice it to say that the man was discharged cured, thirty-seven days after the operation, and that when seen again, nearly a year later, he was able to follow his employment of cattle driver; and walk thirty miles a day with facility, though he was rather less strong-winded than before his illness.

In the second paper, Dr. Vigla puts together a series of cases of thoracic hydatids recorded by various authors, and analyses them with regard to their pathological, nosological, and therapeutic bearings. We have extracted and put together the chief points contained in the history of these cases, including, for comparison, the one just detailed, in the following table:

No. of case.	Name.	Age.	Sex.	Side of chest affected.	Probable duration.	Result.
1.	C. R.	32	M.	Right	15 months	Cure.
2.	—	28	M.	Both.	4 years	Death and PM.
3.	C. M.	39	M.	Right	3½ years	„ „
4.	A. H.	36	M.	Right	4 years	„ „
5.	—	41	F.	Left	17 months	„ „
6.	G.	63	M.	Right	8 years	„ „

It is probable that in all these cases the hydatid was developed in the pleural cavity. The result of its growth was the compression of the lung, or, as in Case 2, of both pulmonary organs, with such displacement of other organs—the heart, liver, spleen—as necessarily resulted from the accumulation of liquid in the thorax. The effect and symptoms were closely analogous with those of hydrothorax, to which condition they in fact referred, excepting in Dr. Vigla's own case.

As the differential diagnosis between the presence of thoracic hydatids and chronic effusion into the pleural cavity is a point of great nicety, we quote Dr. Vigla's comparative analysis of the symptoms entire:

Intra-thoracic hydatids.	*Chronic effusion into the thoracic cavity.*
Pain constant, occupying a considerable extent, or radiating frequently over the affected side.	*Pain* only at the commencement of the disease, ordinarily circumscribed, not radiating.
Dyspnœa, always increasing; the most severe symptom of the disease; ordinary cause, so to speak, of death.	*Dyspnœa* often slight; becoming stationary after a certain time; exceptionally the cause of death.
Dilatation of affected side unequal; partial deformity and enlargement.	*Dilatation* uniform, general.
Dulness on percussion proceeding from any point at the circumference;	*Dulness* ordinarily beginning at the lower part, and extending upwards;
Without regular level;	Level almost uniform all round;
Capable of advancing into the opposite side in an angular form.	Capable of advancing into the opposite side to a greater or less extent, but always bounded by an almost vertical line.
Development independently of the laws of gravitation.	*Development* in relation to the form of the pleura, and its distribution following the laws of gravitation.
Respiratory murmur ordinarily absent.	*Bronchial murmur* commonly, more or less distant.
Bronchophony and egophony absent.	*Bronchophony* and egophony present.
Fever absent.	*Fever* almost always present.
Continued integrity of all the functions but that of respiration.	*Rapid* alteration of almost all the functions.
Commencement not well defined—chronic condition.	*Commencement* generally marked by an acute stage, which subsequently passes into the chronic form.
Progress slow.	*Progress* much more slow.
Duration very considerable.	*Duration* much less.
Terminating in cachexia, the chief traits of which are anæmia and emaciation.	*Terminating* frequently in hectic fever.
Death by asphyxia, excepting in case of accidents or complications.	*Death* by exhaustion.
External violence occasionally exercises a definite influence upon the production of the malady.	*External violence* ordinarily exercises no influence.

By a careful comparison of the points indicated, we may probably succeed, as Dr. Vigla has in fact done, in establishing a diagnosis. The analysis of the two series of symptoms is rational and valuable, though we should be inclined to leave out the last feature of *external violence,* which may be a coincidence, but can scarcely be regarded as the efficient cause of a parasitic growth containing echinococci.

Dr. Vigla next quotes three cases of hydatids of the liver, which encroached so much upon the thoracic cavity as to simulate pleuritic effusion.

VI. *Hydatid Cyst of the Liver opening into the Vena Cava Inferior.* By M. HERARD. (L'Union Méd., Sept. 18, 1855.)

VII. *Epidemic Jaundice in Montgomery County, Pennsylvania, U.S.* By HIRAM CORSON, M.D. (The Med. Examiner, Philad., July 1855.)

VIII. *Statistics of Two Hundred and Fifty-eight Cases of Intestinal Obstruction, with Remarks.* By S. FOSTER HAVEN, Jun., M.D. (The American Journal of the Medical Sciences, Oct. 1855.)

IX. *On the Epidemic of Gangrenous Ergotism observed at the Hôtel Dieu of Lyons, in* 1854 *and* 1855. By M. BARRIER. (Revue Médico-Chirurgicale, Sept. 1855; extracted from the Gaz. Méd. de Lyon.)

It appears that ergotism is endemic in the vicinity of Lyon, but that only a very limited number of cases occurred during the last ten years. A sudden increase in the number of persons affected took place in 1854, so that in the course of one year thirty patients suffering from ergotic gangrene were admitted into the Hôtel Dieu of Lyons; but it was evident from the statements of these patients that they formed but a small portion of the total number affected.

The age of the persons affected fluctuated between twelve and sixty years. Males were more prone to be affected than females; and the majority of the patients demonstrably suffered from great debility previous to the actual seizure. Frequently a single member of a household, all of whom had partaken of the vitiated grain, was affected, the others escaping; in such cases, the weakest were those most liable to suffer. The gangrene most commonly affected the toes, feet, and legs; the worst case was one in which the mortification extended to the middle of the thigh. In some the fingers and hands were attacked; but in no instance did the face or trunk suffer.

The treatment is not particularized. The rule was to assist nature when the gangrenous portions were falling off, by accelerating the separation artificially; but only in one or two instances was it thought right to amputate in the healthy tissues. Three patients are spoken of as having died, but we do not gather whether these were the only fatal cases. The author regards arteritis as the pathological element of the disease.

X. *Creatine spontaneously deposited in Oxaluria.* By GEORGE W. MILTENBERGER, Professor of Pathological Anatomy in the University of Maryland, U.S. (The Med. Examiner, edited by S. L. Hollingsworth. June, 1855.)

The formation of creatine crystals on the mere evaporation of urine in which oxalate of lime octahedra had been observed, has been noticed in five separate cases by Professor Miltenberger; and he states that the appearances were confirmed not only by comparison with the plates of Robin and Verdeil, and of Funke, but also by several other competent microscopic observers. In the first instance the creatine crystals were accidentally discovered by leaving the urine of a gentleman affected with oxaluria, accompanying nervous and dyspeptic disorders, on a slide covered with a thin operculum. When re-examined twelve or fifteen hours later, and again the day after, "there could be no doubt, as far as microscopic appearances went, that it was creatine."

In four other cases of oxaluria the same occurrence was met with; while the urine of numerous other individuals examined at the same time, some in health, some in various morbid states, presented nothing of the kind.

With the exception of one of the five cases, a boy labouring under numerous congestive abscesses of the lower extremities and tuberculosis, the patients promptly responded to the treatment, a marked improvement and diminution of the creatine being observed. The treatment consisted, in three of the four cases, in the administration of nitro-muriatic acid; in the fourth, in whom there was a predominance of oxalurate of lime, of nitrate of silver.

In the September number of the same Review from which we extract Dr. Miltenberger's observations, Dr. Cheston Morris records the case of B. P., a blacksmith, aged forty-five, and suffering under the phosphatic diathesis for three years, in whose urine he met with creatine crystals, which were found at the edge of the cover-glass after the urine was evaporated.

XI. *On Icterus Typhoides.* By Professor Lebert. (Virchow's Archiv für Pathologische Anatomie, &c.; Band viii. Hefte 2 & 3.)

This is a continuation of a paper contained in the seventh volume of Virchow's 'Archiv.' Professor Lebert, in the former paper,[*] gave a summary of the pathological changes, of the causes, prognoses, and treatment of the disease to which he applies the name of icterus typhoides. He now enters into a disquisition on the nosological position to be assigned to this disease, with a view more particularly to determine, whether it is mainly a disease of the liver or a disease of the blood. As Oppolzer and Horaczek regard acute yellow atrophy of the liver as the main element of the disease in question, Professor Lebert quotes several cases of fatal icterus with typhoid symptoms, in which no such lesion was discoverable. Nor does the microscopic alteration of the hepatic cells, according to his investigation, indicate any changes which would account for the pathological occurrences characterizing the disease. He denies the partial distinction of the hepatic cells, which has been asserted to exist in acute yellow atrophy; and states that he has met with profound alteration, and even partial destruction, of the hepatic cells, without co-existing atrophy of the organ. He does not deny that typhoid and icteric symptoms occasionally accompany acute yellow atrophy of the liver; but he refuses to admit this pathological state as an essential constituent of the malady. He is disposed to place it in the same rank with pyæmia, necræmia, and uræmia; an excess of biliary constituents in the blood giving rise to the typhoid condition which characterize the just-mentioned diseases. The French regard the disease as sporadic yellow fever, but the author demurs to this view, owing to the absence of all miasmatous or epidemic influences in the cases of typhoid icterus which he has collected. The general conclusion, then, at which Lebert arrives, is, that although acute yellow atrophy of the liver may be a concomitant, it is not the essential constituent of the disease, which most probably depends upon a morbid condition of the entire mass of the blood. He inclines to regard a retention of bile-forming materials in the blood, and possibly their decomposition, and a consequent formation of toxic products, as the *causa proxima.*

XII. *A few Pathological Facts, intended to Elucidate the Question of the Production of Sugar in the Animal Economy.* By M. Andral. (Revue Médico-Chirurg. p. 98, August, 1855.)

That the liver possesses the power of producing saccharine matter has been satisfactorily demonstrated by M. Bernard,[†] who showed that this function was exercised whether animal or vegetable food was taken, and during the digestive process as well as during fasting. M. Andral, in a paper read before the Academy of Sciences in July last, supports M. Bernard's physiological results by pathological observations. He states that he has observed in diabetic patients a diminution or disappearance of the sugar in the urine, concurrently with deprivation of food. A female, whose urine had been analysed daily, passed every twenty-four

[*] See the sixteenth volume of this Review, p. 246.

[†] For a summary of M. Bernard's discoveries, see the Medico-Chirurgical Review, p. 54. Jan. 1854.

hours from 40 to 70 grammes of sugar per litre (600 to 1050 grains in about 35 ounces). A copious and stimulant dietary brought on a gastro-intestinal affection, with entire loss of appetite, and diarrhœa. In proportion as the diet was reduced, the amount of sugar also diminished—on the first day to 54 grammes, forty-eight hours later to 34 grammes, and twenty-four hours after to 28 grammes per litre; the patient was then deprived of all aliment, and after forty-eight hours of abstinence there was no trace of sugar in the urine. It was not until the third day after food was again administered, that the sugar began slowly to reappear. It subsequently regained the same proportions which it had originally exhibited. Andral also adduces an instance in proof of M. Bernard's statement, that the presence of amylaceous compounds in the food is not necessary to the production of sugar in the system; and that, although the amount may be reduced by a rigid adherence to purely albuminous diet, it does not always, or generally, entirely disappear. He quotes the case of a female who subjected herself rigidly to an exclusively animal diet for two entire months, taking nothing but boiled or roast meat; her beverage consisting of water with a small quantity of spirits. At the commencement of this system of diet, the quantity of sugar amounted to 27 grains per litre, and successively sunk to 20, 15, 12, and 10 grammes. It then rapidly rose, successively, to 15, 20, 30, 44, and 49 grammes per litre, without any infraction of the animal diet. But it is still more remarkable, that a rapid reduction again took place on the adoption of a mixed diet of meat, eggs, milk, a little bread and vegetables, with wine and water. Three weeks after the adoption of this mixed diet, the urine again showed an increase in the quantity of sugar. M. Andral states that he has met with other analogous cases, and he concludes that, while a sudden change in the diet—even allowing an admixture of amylaceous matter—causes a reduction in the amount of sugar, an exclusively animal diet prevents the production of sugar in man, as little as it does, according to the researches of M. Bernard, in animals.

With regard to the evidence afforded by morbid anatomy, M. Andral observes, that, while pulmonary tubercles are almost invariably found to accompany diabetes, the absence of sugar in the urine of phthisical subjects is too general to establish a necessary relation between pulmonic disorder and glycosuria. On the other hand, in five post-mortem examinations of diabetic patients which he has made since the publication of M. Bernard's results, he has always met with an hepatic lesion of the same character; the organ presented a marked reddish-brown colour, which was so uniform as to entirely efface the ordinary distinction of the two substances; he attributes this to an intense hyperæmia, differing in character from the ordinary appearance of congestion of this organ. He meets the objection that this might have been the result of the peculiar diet to which diabetic patients were subjected, by stating that, in two of the cases, the diet had remained nearly unchanged. M. Andral concludes his paper with the following suggestive question:—May not the congestion of one or the other system of hepatic capillaries determine either an alteration in the secretion of the bile, an alteration in the secretion of the sugar, or a modification of such other organic function as the liver may be destined for?

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E., London.

I. *On Ophthalmia caused by the Presence of Lime in the Eye.* By M. Gosselin. (Archives Générales, pp. 513—23, Nov. 1855.)

It has long been stated that the presence of lime in the eye induces accidents similar to those produced by heat and powerful caustics—viz., rapid opacity of the cornea, sloughing of this membrane and the conjunctiva, consecutive suppuration, and wasting away of the eye. In the cases that have been published, however, the effects of quick lime and of slaked lime have not been sufficiently distinguished.

M. Gosselin's observations entirely apply to the latter, the unexpected course of the symptoms in a case that recently occurred to him having induced him to investigate the matter.

The subject of the case in question was a mason who (June 6) received in his left eye some of the lime employed in whitewashing, and was seen by M. Gosselin within a quarter of an hour after. A large portion of lime was found between the eyelids, and the cornea was completely whitened. When the lime had been washed off, by means of a hydrocele syringe, the cornea was found to be perfectly opaque over its whole surface, so that the iris and pupil were invisible, and vision quite lost. The patient complained of no pain whatever. An ocular douche was ordered every second hour, in order to wash away any lime that might still adhere, and to anticipate the intense phlegmon that was expected. The patient slept well all night, and next day there was only perceived some œdematous swelling of the ocular conjunctiva, the cornea continuing white. The douche was continued, and he was bled. The day after there was abundant watering of the eye, but neither pain, redness, nor suppuration were present. In consequence of experiments, to be presently noticed, having shown M. Gosselin the utility of sugared water, he ordered on the 9th, while continuing the ocular douche, some drops of distilled water, highly sugared, to be instilled every other hour. On the 11th the cornea was less white, although the pupil could not yet be seen. The œdematous chemosis persisted, with here and there some ecchymosis. By the 20th the iris and pupil were somewhat visible, although the patient could not distinguish objects. The chemosis remained as it was. There was little pain, and no suppuration; but the patient could not raise the eyelid, and the weeping continued abundant. Purgatives were ordered, the douche omitted, and the sugared collyrium continued. On the 24th the chemosis had diminished, and the patient began to recognise objects. There was more redness of the conjunctiva, but not more pain. Some leeches were applied behind the ear; and, as improvement continued very slow, they were repeated on the 23rd. By the 30th the patient could open the eye better, and the conjunctiva was less red, but it was thick and vascular all round the cornea. Two bridles were also now observed, stretching from the conjunctival *cul-de-sac* to the neighbourhood of the cornea, one above, and the other below. On the 12th of August the patient, wishing to resume his occupations, was discharged. There was then but little redness of the conjunctiva, except at the external edge of the cornea. The eye was not quite so open as the other. The watering had quite disappeared, and no kind of uneasiness was felt. The conjunctival bridles did not seem to impede the movements of the globe. The cornea still continued cloudy in places, but the iris and pupil could be well seen, and the patient could recognise all objects with his eye, although he could not read small print.

To this case M. Gosselin appends some interesting observations.

1. *Nature of the Opacity.*—Opacity of the cornea is most frequently due to the effusion of plastic lymph, secreted during the progress of a keratitis, either at the surface or in the substance of the membrane, the transparency of which is gradually replaced by a white tissue. Sometimes it arises from the coagulation of the albuminoid matter of the cornea produced by the contact of a heated body, or some chemical agent, as a concentrated acid or caustic potass. In this case, the opacity occurred too rapidly to be due to inflammatory exudation, and it seemed natural to refer its production to chemical action. But then, on the one hand, it was surprising to find that the superficial layers of the cornea had not lost their smoothness and polish, as would be expected from such a cause; and on the other, it was difficult to understand how coagulation through the entire thickness of the cornea should result from mere contact at its surface. In order to clear up this point, M. Gosselin instituted a series of experiments, which consisted in allowing milk of lime to fall into the eyes of rabbits and dogs. The rapidity with which opacity resulted was remarkable, the cornea becoming entirely white in two or three minutes. The rapidity was proportioned to the amount of lime held in

suspension; and although even mere lime-water effected the opacity, a much longer time was required. The opaque cornea was detached, and subjected to various reagents. Immersed in diluted muriatic acid (8 to 10 drops to 30 grammes), or acetic acid (20 to 30 drops to 30 grammes), or in sugared water, the cornea resumed its transparency in a few minutes in the first two liquids, and in about an hour in the last. Calcining the opaque cornea, and precipitating an oxalate of lime by means of the oxalate of ammonia, this was found to be far more abundant than that producible from the normal cornea. These results, and those obtained from other therapeutical experiments, convinced the author that the opacity in these cases is due to the infiltration of the molecules of the lime into the meshes of the cornea, and their combination with its tissue.

2. *The Clinical Phenomena.*—M. Gosselin had fully expected in this case violent suppurative inflammation, while there was only observed a serous and ecchymotic non-phlegmonous chemosis, with but little redness, and neither suppuration nor severe pain. M. Guépin and other observers had already noticed the mild character of the inflammation in these cases. In the present instance such mildness may be however somewhat due to early treatment; for in the experiments, when the cases were left to themselves, violent conjunctival suppuration and perforation of the cornea followed. The very *chronic* character of the affection is to be remarked, for at the end of two months the ocular conjunctiva was still injected, the vessels extending over the periphery of the cornea, unaccompanied, it is true, by photophobia, pain, or weeping. The formation of the conjunctival bridles is also worthy of attention; for if the patient had not been observed so carefully, these might have been deemed cicatricial, the results of eschars. In the absence, however, of all breach of surface, the bridles seem to be due to the special inflammation that took place, which manifested itself at first in the production of the serous tumefaction, accompanied by ecchymosis and watering, but without acute pain. A partial retraction of the conjunctiva probably occurred, as sometimes takes place in other tissues as the consequence of inflammation. The most striking phenomenon in the case is the restoration of the transparency of the cornea and the recovery of vision, due to the lime being dissolved in, and conveyed off, by the liquids placed on the surface of the eye. Although this is in great part attributable to the treatment pursued, it is possible that such result might be produced through the sole efforts of nature, by the mixture of the tears with the lime in the cornea.

3. *Treatment.*—The indication in these cases is to obtain the discharge of the lime, the presence of which gives rise to irritation proportionate to the length of its sojourn, and to combat the inflammation that has been induced. The first of these points has hitherto been too much overlooked, and it was for its illustration the author undertook the experiments we have mentioned. As soon as he was aware that fluids placed in contact with the eye penetrated into the cornea, he sought for the one best able to dissolve and facilitate the elimination of the lime. Strictly speaking, the tears, or water projected on the surface of the eye, might fulfil this object; and for this douches were first resorted to. But as lime is little soluble in water, it was to be feared it would remain long enough to do mischief. In two rabbits, in which it was only employed, the opacity diminished very partially, and severe inflammation supervened. In other experiments, a diluted muriatic acid collyrium, dropped into the eye of a rabbit for half an hour, restored the transparency of the cornea in that time; but next day it was again whitened, and the conjunctiva was much inflamed. Acetic acid, when used so weak as to cause no inflammation, produced no effect upon the opacity. M. Bussy suggested sugar as the substance best able to aid the solution of lime, by forming a soluble saccharate, and yet being a substance that diminishes rather than increases ophthalmia. The result was highly satisfactory, although in the experiments it diminished the opacity far less rapidly than did the acids.

Quick lime acts through the modification which its caloric produces on the cornea, coagulating its albumen, and disorganizing the conjunctiva. As it is possible that when it reaches the cornea it may have lost sufficient of its caloric to act merely as slaked lime, the sugar treatment may be tried even here, if the consecutive inflammation be not excessive. If the opacity result from albuminous coagulation, it may be of no use, but harmless; while if there is calcareous infiltration, it may contribute to the restoration of transparency.

II. *The Results of M. Sichel's Operations for Cataract.* By M. Doumic. (Annales d'Oculistique, tome xxxiv. p. 172.)

During the last nine years (1846–54), M. Sichel has operated for cataract 1026 times in 641 individuals. Taking the whole period, the two *sexes* furnished nearly the same number of patients—viz., 313 males and 328 females. The necessity, however, of dealing with a sufficient number of figures in determining their respective liability, is seen from the fact that while among the patients during 1846–51 (432) there was a preponderance of 44 females (238) over the males (194), among those of 1851–4 (209), the males (119) exceed the females (90) by 29. The *ages* at which the operations were performed are thus set forth:

5 months	to 10 years	26
10 years	to 20 „	19
20 „	to 30 „	14
30 „	to 40 „	13
40 „	to 50 „	50
50 „	to 60 „	136
60 „	to 70 „	229
70 „	to 80 „	139
80 „	to 90 „	15
		641

In the first years of life, congenital cataracts are pretty frequent. Up to about the age of forty, the greater number operated upon are of congenital origin, the remainder being traumatic; spontaneous cataract occurring very rarely. After forty years, on the contrary, and especially after fifty, double senile spontaneous cataract becomes very frequent, the proportion of traumatic cataracts continuing the same.

The *seat* of these cataracts is stated to have been

Lenticular in	930
Capsulo-lenticular in	77
Capsular	19
	1026

The *consistence* of the cataract was

Hard in	23
Semi-hard in	232
Soft in	269
Semi-soft in	459
Semi-liquid in	42
Ossified in	1
	1026

Thus the soft and semi-soft cataracts, taken together, are of much more frequent occurrence than the hard and the semi-hard united. Hence the reason for the more frequent performance of the operation of extraction; for in these cataracts the fragments, immersed in the aqueous humour, would swell, and give rise to various serious accidents, especially in persons liable to even slight chronic congestions of the brain. Moreover, the absorption of the *débris* of the crystalline takes place very slowly in the aged, when it takes place at all. It is therefore in these cases better to avoid any risk of compression of the internal membranes, and extract the cataract when it is soft or semi-soft. In children, the fragments of the lens are absorbed rapidly after division, without accidents; and in them division of soft cataracts sould be the rule. When the cataract is hard, depression should be performed at all ages, for the lens cannot swell, and it may be lodged in the lower and deeper part of the eye; and whether absorbed or not, will not there injure the internal membranes. But the nucleus may be hard, and the cortical substance soft, and then the cataract must be extracted if the cortical substance is abundant; while, when this is small in quantity, the cataract may be depressed without danger.

The *results* of the operations are given as follows:

Extraction, 780	Successful	616
	Partial success	71
	Failure	93
		780

Thus the proportion of successes is 79 per cent. as compared with 12 per cent. failures. In explanation of even this proportion of failures, it is to be observed that two of the nine years were cholera years, and vomiting impeded in some of the cases the healing of the corneal wound.

Division, 136	Successful	100
	Partial success	24
	Failure	12
		136

Depression, 98	Successful	67
	Partial success	19
	Failure	12
		98

The whole of the operations united gave the following results:

Cases.	Success.	Partial.	Failures.
1014	783	114	117
	73 per cent.	15 per cent.	12 per cent.

The proportion of success attending the different operations was, in

Extraction	79 per cent.
Division	73 „
Depression	67 „

III. *On Contraction of the Neck of the Bladder.* By Dr. SLADE.

(Boston Journal, vol. lii. p. 429.)

In this paper, Dr. Slade gives an account of an affection to which, he says, M. Caudmont of Paris has paid much attention. By neck of the bladder, M. Caudmont does not merely understand the urethro-vesical orifice, but comprehends under the term the membranous and prostatic portions of the urethra. The muscular fibres surrounding these parts are the seat of contraction, which is distinguished from spasm by its greater permanence. It may, however, be preceded by, or become complicated with, spasm. In its uncomplicated state it has been termed by Roux, Velpeau, and Civiale, neuralgia or nervous condition of the parts affected.

The symptoms chiefly consist in difficult micturition and pain. The impulse to pass water is frequent and sometimes irresistible. The stream is smaller, not so well thrown out towards the end; is accompanied by straining, and may be suddenly arrested in mid-course. Incomplete erections often accompany micturition, and at night they may become complete and troublesome. Pain is not always present, especially in children; and it may vary much in intensity as well as precise locality. It is often intermittent, and is most severe in rheumatic subjects, and when the contraction is due to chronic inflammation of the neck of the bladder. One form of this pain is characteristic of the affection being due to the forced opening of the contracted muscles, occurring at the commencement of micturition. After the affection has existed some time, we may have spasm after intercourse, retention of urine, vesical catarrh, incontinence of urine (especially in children), obstinate erections, or gleets. To these may be added a constricted condition of the sphincter ani and of the muscles of the perineum, causing to some more suffering than the original disease. This is the *ano-vesical neuralgia* of some authors, and is accompanied by obstinate constipation, and lancinating pains at stool.

The diagnosis of the affection must be established by the bougie, to the passage of which a temporary resistance is offered, very like that due to stricture.

The disease of itself is not of great importance, but it gives rise to serious complications. Among its local causes, gonorrhœa stands foremost, especially when this is old, and attacks the deep-seated portions of the urethra. Other local causes are inflammation or irritation near the neck of the bladder, stricture, calculus, diseased prostate, hæmorrhoids, constipation, &c. The general causes are the nervous temperament and affections, debility, scrofulous habit, and, above all, according to M. Caudmont, the rheumatic diathesis. All ages and both sexes are equally liable to it.

In treating the affection we must first seek out its cause. Tonics, combined with the use of sulphur baths and frictions, or cold douches upon the pubes, groin, and perineum, are useful, as in some cases is the application of electricity. The bowels should be kept gently open; and suppositories, opiated enemata, and belladonna ointments are of great service. Dr. Slade has seen great advantage derived from the internal use of belladonna, especially in children suffering from incontinence of urine, which is, according to M. Caudmont, almost always dependent upon this contraction. Local treatment has, however, usually to be had recourse to, and this consists in the passage of a wax bougie every two or three days, retaining it in the canal for a few minutes only. Cauterisation is oftener called for in cases dependent upon chronic inflammation, and where gleety discharge is present, than in the rheumatic form. Various forms of mercurial ointments, passed into the urethra by means of an olive-shaped bougie, are often extremely useful: and in obstinate gleets, Dr. Slade speaks highly of their use.

IV. *On a New Mode of Treating Prolapsus of the Rectum.* By M. CHASSAIGNAC. (Revue Médico-Chirurgicale, tome xviii. pp. 113 and 207.)

Under this name very different pathological conditions have been comprised, that must be well distinguished from each other in order to judge of the value of any form of treatment. Foremost are we to distinguish the cases which consist of mere prolapse of the mucous membrane, and which ought not to be termed prolapsus of the rectum at all, from those which are formed by the descent of the higher portions of the rectum, presenting externally, after a time, invagination. In the first the tumour is formed of the mucous membrane alone, while in the other it implicates all the coats, not excepting the serous. Prolapsus of the mucous membrane, too, must be distinguished according as it is simple or complicated with hæmorrhoids. A prolapsus is often a trifling affection, especially in children; if such cases be excluded, any method may be pronounced successful in its treatment. It varies indeed from an ailment that calls for mere precautions rather than treatment, to an affection of the most obstinate nature, perplexing to the surgeon, and most discouraging to the patient. Before describing his own treatment, M. Chassaignac adverts to the different modes of managing the disease.

1. *Reduction.*—The patient is to be placed in the horizontal position, the surface of the tumour cleaned by an astringent lotion, and smeared with a fatty body, and having passed the fingers equally around it, a concentric compression is to be exerted, avoiding, on the one hand, all intermission, and on the other, all sudden increase of this. Various are the contrivances for retaining the part when reduced; but many of the means used for this purpose have the effect of dilating the rectal tumefaction, rather increasing the laxity of the tissues than tending to restore their tone; so that, if they mechanically and temporarily remedy the prolapsus itself, they do nothing for its definitive cure. In slight cases, M. Chassaignac has had recourse to *ice suppositories,* seven or eight centimetres long, with most excellent results, the affection rapidly yielding to the influence of these, introduced once a day.

2. *Débridement.*—This M. Chassaignac has never resorted to, and he feels convinced that neither in this case nor in paraphymosis, is an operation ever requisite to effect reduction—let the size of the prolapsus or the amount of constriction be what they may. When it fails, compression is performed in a defective manner, or with insufficient perseverance.

3. *Excision of the folds around the anus.*—This mode, which in the hands of Dupuytren and other surgeons led to successful results, is based upon the expectation that the contraction arising from cicatricial tissue will impede the future descent of the gut. Moreover, the adhesion of the skin to the subjacent parts which takes place, prevents the too easy sliding of the integument that surrounds the orifice, and opposes that laxity of this part which notably predisposes to prolapsus of the mucous membrane. M. Chassaignac believes the advantages of this procedure have been exaggerated, while it exposes the patients to the danger of diffuse suppuration, purulent infection, and inguinal adenitis. But besides these inconveniences, which are common to all the operations by cutting instruments, there are others which especially attach to this. The anal extremity of each incision terminates at the mucous membrane, just above the anal orifice, and this is just the point where a varicose state of the hæmorrhoidal veins often complicates the prolapsus—hence danger of hæmorrhage and phlebitis. The more attentively surgical affections of the lower extremity of the rectum have been studied, the more surgeons have shown themselves disposed to refrain from the use of cutting instruments in a region so eminently vascular.

4. *Ablation.*—With the above, Hey's operation of ablation has been confounded. On examining his narrations, it is evident that he has frequently mistaken hæmorrhoidal tumours for prolapsus of the rectum; and for this class of tumours, excision, owing to the dangers it gave rise to, has been well nigh abandoned.

5. *Actual cautery.*—This means has been much recommended by several surgeons, and especially by M. Bégin; and it is the procedure to which, in spite of the great suppuration it gives rise to, M. Chassaignac gives the preference next to his own operation.

6. *Linear écrasement* is the title given by M. Chassaignac to a new operation that has recently excited much attention in Paris. It is especially applicable to the removal of pediculated tumours and growths in which the occurrence of hæmorrhage is feared; and the great success that has attended its adoption for hæmorrhoidal tumours has induced M. Chassaignac to extend it to the present affection. The operation consists in surrounding the part to be removed by a loop of chain-work, the ends of which are contained in a tube, and are susceptible, by the aid of a balance lever, of being drawn to any required degree of tightness, the constriction being operated at slow and regular intervals, and the part separated at the will of the operator. The noose of a ligature is first thrown around the part, to mark where the chain of the *écraseur* is to be applied, the mucous membrane

being previously drawn down by an expanding six-pronged tenaculum, that had been introduced in its closed state. A completely dry section results, no blood being lost. In complete prolapsus, the muscular and mucous coats are divided, but the implication of the peritoneal *cul-de-sac* is to be avoided. There is no exact line of demarcation which enables us to point out the limits of this; but for the cure of the prolapsus it is not necessary that the whole of it should be excised, and the surgeon will incur no risk if he does not remove more than two fingers' breadth of the prolapsed part.

M. Chassaignac speaks in sanguine terms of the results he has hitherto attained; and in the present paper relates two cases that had lasted several years, and three others that were complicated with hæmorrhoids.

V. *On the Destruction of Non-vascular Nævi by the Vienna Caustic.* By M. Chassaignac. (Gaz. des Hôpitaux, Nos. 123 and 124, 1855.)

The pigmentary stains that have often been confounded with erectile tumours, under the common term of *nævi materni*, have been made the subject of special study by M. Chassaignac. He has shown that so far from being congenital, as generally believed, they do not appear, in the immense majority of cases, until after birth, and in many not until an advanced period of life. They do not consist solely in an accumulation of pigmentary matter, there existing, in a great number of instances, besides this, an appreciable quantity of a tissue he terms "fungoid." He distinguishes these merely pigmentary deposits from erectile tumours, which they often resemble, by observing the effects of the pressure of the finger, this rendering an erectile discoloration pale, while it effects no change of colour in the pigmentary one. For treating this affection, M. Chassaignac regards the Vienna caustic as the best means, this inducing a dry eschar without suppuration, which leaves after its fall a smooth even cicatrix, that is moveable over the parts it covers, and differs little in colour from the surrounding integuments.

The procedure is only applicable to slight thicknesses of living tissues. As soon as an eschar has been produced by the caustic sufficiently thick for the object in view, its surface is washed with a little vinegar and water, completely dried, and then it is covered with a piece of very supple amadou, cut so as to fit it exactly. If the access of all moisture be prevented, the amadou becomes so identified with the eschar that it falls off only with it, while the latter is not detached until the tissues subjacent to it are completely cicatrized. The intimate adhesion of the amadou is a *sine quâ non* of success. M. Chassaignac maintains that all these spots, whatever their size or position, may be entirely removed by successive partial cauterisations of this kind, although the procedure, where they are large, exacts much time and patience. As an example of this, he relates a case in which a nævus the size of a crown piece, situated on the forehead, was attacked five times during seven months, the caustic being applied for from three to eight minutes, according to the different thickness presented, the application causing very considerable pain for some hours. A bandage was at first applied to retain the amadou, but after a few hours this was not required. The amadou came off with the eschar in from one to two months, leaving cicatrisation complete.

In a recent communication to the Paris Society of Surgery, M. Leclerc related a case of *true erectile tumour*, occurring in a child, which was successfully treated by the external application three times a-day of the *perchloride of iron*. It was continued during two months, giving rise to no pain or irritation whatever. It was long before any change occurred in the tumour, and its diminution was very gradual. The perchloride has already been several times resorted to in France, but only by injecting it into the vascular tissue of the tumour, or by applying it after prior blistering.

VI. *On the Treatment of Syphilis.* By Dr. HANSELMANN. (Wiener Medicin. Zeitung, No. 42.)

In this paper, Dr. Hanselmann describes a somewhat novel treatment, which he states he has found very successful in military practice. 1. He prefers to all other means the employment of lemons and corrosive sublimate in the treatment of indurated chancre, syphilitic eruptions, condylomata, mucous tubercles, periostitis, &c. Two lemons are eaten, peel and all, daily, for a fortnight, and then one-eighth of a grain of sublimate is given daily for a week. This three weeks' course, he states, is in general sufficient, and a repetition of it is seldom required. In the summer, the patient takes a bath daily. 2. The local treatment of chancre consists in employing, three times a day, a penis bath, formed of two drachms of alum to one pound of water, and continued until a reddish circle is formed around the sore, and a yellow membrane is detached from its surface. The ulcer is, for the most part, left of a pure red, its bottom being either quite even or irregular. In the first case, an indurated cicatrix results from the continuance of the alum bath, and it is better to substitute one of diluted acetic or tartaric acid, which induces a far better cicatrix, though its formation requires more time. In simple chancres, the bottom of which is not yellow, but mingled red and yellow, the acid bath should be used from the beginning. No yellow membrane is here cast off, but the sore gradually cleans, and an even cicatrix is formed. The healing is more tedious, but neither induration nor bubo occur during its progress, while they may arise during the use of the alum bath. During the employment of these penis baths, the number of ulcers may become increased, owing to the bursting of small subcutaneous abscesses. The baths are to be continued, and two lemons should be eaten daily for awhile. 3. In balanitis, in inflammatory induration, complicated chancre, and in congenital phymosis, the alum baths have been found especially useful. The watery, eczematous ulcers of balanitis heal in a few days, inflammatory indurations disappear in about a week, and for the operation for phymosis, formerly of frequent occurrence, is now substituted the mere scarification of the upper border of the prepuce. Indurations induced by the local application of corrosive sublimate are quickly dissipated by the alum bath, but those which result from the use of blue vitriol, common among soldiers in garrison, are more obstinate. Tow compresses, with rectified spirit, are first to be applied, and then lemons and the alum bath resorted to. The same plan has been often successful in the Hunterian chancre, when the induration has not yielded to five or six days' use of the alum baths. 4. Broad condylomata are treated by pencilling them twice a day with rectified spirit or concentrated acetic acid; and in the few cases of pointed condylomata he has so treated by the vinegar, the author has found a rapid diminution result.

VII. *Vinous Cataplasms in Hospital Gangrene.* By M. PAYAN. (Gazette des Hôpitaux, No. 133.)

Hospital gangrene has frequently shown itself among the wounded sent from the Crimea to the hospitals in the south of France. It has been generally during the passage from Constantinople to Toulon or Marseilles that it has broken out; and it has resisted treatment in the hospitals with great obstinacy, usually requiring very painful cauterisations to arrest its progress. M. Payan has had under his care in the hospital at Aix ten cases, in different stages of the disease. Recollecting the great efficacy of vinous cataplasms in sanious ulcers, he determined to resort to them on this occasion, and found that a speedy amelioration ensued in all these cases upon employing them twice a day. Some slices of bread are placed in a pipkin and *vin ordinaire* is poured over them, and when the bread has thoroughly imbibed the wine, the whole is boiled for a few minutes, stirring it the while with a spatula, so as to form it into a kind of paste.

[We may observe that M. Denonvilliers has recently found glycerine of great service in these cases at the St. Louis Hospital.]

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D. (Lond.)
Physician to the Metropolitan Free Hospital, late Physician-Accoucheur to the Western General Dispensary.

I. Physiology and Pathology of the Non-pregnant Woman.

1. *The Secretion of the Mucous Membrane of the Vagina and Cervix Uteri.* By A. Kölliker and F. W. Scanzoni. (Schmidt's Jahrb., No. 10. 1855.)
2. *On the Secretions of Fluor Albus.* By H. Beigel, of Vienna. (Monatsschr. f. Geburtsk. June, 1855.)
3. *A Masculine Uterus in a Man Sixty-three Years old.* By Professor Langer. (Zeitschr. der k. k. Ges. d. Aerzte zu Wien. July and August, 1855.)
4. *Cysts in the Mucous Membrane of the Cavity of the Uterus.* By Dr. E. Wagner, of Leipzig. (Archiv f. Physiol. Heilkunde. June 15, 1855.)
5. *On the Sacrum, considered as forming part of the Vault of the Pelvis, &c.* By Dr. Matthews Duncan. (Edinb. Med. and Surg. Journ. August, 1855.)

1. The following account of the secretions of the mucous membrane of the vagina and cervix uteri, by Kölliker and Scanzoni, is reported on account of the importance of possessing precise information upon the subject; an object which may be promoted by comparing this account with the full and interesting description by Dr. Tyler Smith.

I. *The Secretion of the Vagina*, in the perfectly normal condition of the vagina —which, according to the authors, is only found in women who have never borne children, and have not been much accustomed to sexual intercourse—is a scanty, transparent, mucous fluid, almost always acid, at times neuter, never alkaline; it contains an immense quantity of pavement-epithelium. *Shortly before* and *after* menstruation, the quantity of vaginal mucus is at times considerably increased. Before menstruation, it is always clear, and usually very thin. For the first two or three days after, it has the same properties, but mostly a reddish colour; under the microscope, we find a more plentiful pavement-epithelium, and sometimes a considerable number of blood-corpuscles.

In the *last period of pregnancy*, the quantity of the vaginal mucus is always considerably augmented. It is either white, thin, milky, or—especially as seen upon the dusky bluish-red colour of the vaginal mucous membrane—somewhat yellow, thick, and creamy; its reaction is always acid. The thicker it is, the greater the quantity of pavement-epithelium, mucous, or pus corpuscles it contains; not seldom also the so-called trichomonas vaginæ, filaments of fungi, and a few vibriones. This condition is analogous with that of non-pregnant women with vaginal blennorrhœa.

The authors especially examine the nature of Donné's trichomonas vaginæ, the animality of which had been denied by many, particularly German, observers. They are now convinced of its animal nature. They found it present in more than half the pregnant and non-pregnant women examined; in healthy women as well as in those affected with benignant and virulent discharges; but never in perfectly normal mucus. It was found in greatest abundance in yellow, creamy secretion, containing many pus-corpuscles, and strongly acid.

The vegetable structures found in the vaginal mucus consisted in rigid, fine, long threads, only differing from Robin's leptothrix buccalis in being somewhat thicker, and in being always isolated.

II. *The Secretion of the Mucous Membrane of the Cervix.*—The transparent viscid mucus thrown out from the cervix may collect in the cervical cavity in small quantity, and occasionally in larger quantity, without, in the normal condition and under ordinary circumstances, escaping from the os uteri. But during menstruation it is separated in greater quantity, and is poured out, during and after menstruation, through the os uteri. The authors arrived at this result by the observation of women in different stages. It is worthy of remark, that the secretion separated from the cervical mucous membrane shortly before and after menstruation, is mostly much thinner; and hence, instead of appearing in the form of a gelatinous, fast-adhering plug, comes to view in limpid or yellowish-white drops.

The cervical secretion of the pregnant and non-pregnant is not to be distinguished. It is always alkaline, whilst the secretion of the outer surface of the vaginal portion of the cervix, and even of the lips of the os uteri, is acid. Through mixing with the vaginal secretion, the reaction is not always distinct, but the cervical secretion loses in consistency, and becomes covered on its surface with whitish-yellow streaks and specks. The mixture of the two secretions may take place at the vaginal portion, and when the os uteri is very open, even in the lower part of the cervical cavity.

The microscopic constituents of the cervical secretion are—mucous-corpuscles, mostly in great quantity, unchanged or variously changed; a few oil-globules; sometimes a little cylinder-epithelium; here and there a few thin and short yeast-fungi, with roundish joints; a few vibriones. The trichomonas was never found.

2. The secretion of the cervix uteri is thick, albuminous, bright as glass, of weak alkaline reaction. In its pathological relations it shows the following:—1. An increase in quantity; it flows constantly out of the os uteri, is thinner, but quite clear and bright, of distinct alkaline reaction; under the microscope, scarce epithelial cells. Or, 2. The secretion shows a milk-white, commonly streaky colouring; the quantity of mucus is simultaneously greater than in the normal state, the mucus is tenacious, stringy, alkaline. Under the microscope are seen many long-elliptical, sometimes very narrow, quite string-shaped cells, disposed in rows, so that their long axes lie parallel; they are without cilia, and mostly have one, sometimes three to six, nuclei grouped together. The author regards these as pathologically-altered ciliated cells. Or, 3. The secretion of the cervix in diseased conditions is purulent; its quantity is then mostly profuse; it is still somewhat stringy and alkaline. The microscopic constituents are pus-corpuscles, partly of the common form, partly longer, stretched out; also the before-described altered ciliated cells. This puriform secretion is most frequently found when the vaginal portion shows chronic catarrhal erosions.

The secretion of the vaginal mucous membrane is more scanty, of white colour, little tenacious, acid, and contains numerous pavement-epithelial cells. In pathological relations, this secretion is either—1. Increased; as in chlorosis and acute irritation of the vagina; or 2. Purulent; quantity increased, sometimes yellow in colour; the principal morphological constituents are pus-corpuscles, between which young epithelial cells in numbers are formed. This kind of discharge is found in granulating conditions of the vaginal mucous membrane.

The secretion of the female urinary passages is, in the normal state, so scanty, that it gives no occasion for remark. In simple vaginal catarrh, there is often found a similar catarrh of the urinary mucous membrane. In blennorrhœa of the bladder, which is always the consequence of clap, there is found a great quantity of purulent mucus, which, besides a few epithelial cells, contains always nothing but pus-corpuscles.

The secretion of Duverney's glands is normally scanty, bright, tenacious, neutral, and contains a few small pavement-epithelial cells. In pathological relations, it is either simply augmented in quantity, or in contemporaneous vaginal blennorrhœa it is purulent, and contains numerous pus-corpuscles.

The author never found the trichomonas of Donné.

(It is to be observed that, although the author has made his observations on the living, he has not described his manner of investigating, nor the precautions he took to isolate and identify the secretions from different parts of the mucous membrane.)

3. The case of Professor Langer is a physiological curiosity. Professor Arámij has recently found, at the necropsy of a man sixty-three years old, a structure resembling a uterus, between the rectum and bladder. The man had had a "capon's voice;" beard well grown; he had lived thirty years in childless wedlock. The uterus was two-horned, ending in two large open tubes. The mesometrum (ligamentum uteri latum of the female) ended on either side in a fine doubling of peritoneum; a true ala vespertilionis, which embraced the testicles and epididymi; and, at the upper border, the end of the tube. On the left side, the uterine horn, with its tube, was dragged over by a scrotal hernia. The distance between the two testicles in the preparation is sixteen inches. A round ligament (uterine) is marked by a bundle of vessels on the right side. The uterus is connected with the upper part of the prostate. The arteries of this uterus arise, with those of the bladder, from a common arteria vesico-uterina. The organ could be easily inflated through the abdominal end of the tube. There were no strong folds in the interior, even at the isthmus. Above the isthmus, the walls of the two-horned uterus were soft, the muscular tissue loose, its mucous membrane was easily separated as a distinct layer. On a section, there were detected tubular crypts opening on the free surface.

In fine, there were distinguished three parts of this uterus:—An orificial part; a glandless, thickened portion, terminating at the isthmus; and a part provided with the ordinary uterine glands, which end in two short horns, which again end in tubes. The testicles were of the normal size. The vasa deferentia ran in an oblique direction to the isthmus uteri, to penetrate the prostate. True vesiculæ seminales were absent.

4. Dr. E. Wagner details three cases of cystiform change of the tubular glands of the uterus. He draws the following conclusions from his investigations:

(1.) There occur in the mucous membrane of the uterine cavity, cysts—that is, sacs, closed on all sides—with a simple polygonal epithelium lying on a membrana propria. The contents of these are an albuminous and mucous fluid, with free nuclei, and colloid-granules of various sizes and growth.

(2.) These cysts appear in individuals of different ages, in those who have borne children, and in those who have not.

(3.) They owe their existence to a disease of the tube-shaped glands. The enlargement of the cysts depends sometimes upon the increase of their contents, at others upon the fusion of several cysts.

(4.) Whether the cysts arise from an independent disease of the uterine glands, or from a preceding affection of the mucous membrane, is uncertain.

(5.) It is also unknown to what size the cysts are capable of growing, or whether they persist, change into colloid cysts, or lastly, whether they burst and then become atrophied.

(6.) When they appear largely scattered during the child-bearing period, they may present an obstacle to the formation of the decidua.

(7.) The cysts are, both as to seat and contents, essentially different from the so-called ovula Nabothi. Whether they are related to the so-called mucous or bladder-polypi of the uterine cavity, is doubtful.

5. The title of Dr. Matthews Duncan's memoir on the Os Sacrum is recorded in order to draw the attention of obstetricians to its contents. Being published in an English journal, it is readily accessible.

II. PHYSIOLOGY AND PATHOLOGY OF THE PREGNANT WOMAN; AND OF LABOUR.

1. *Uterine Hydrorrhœa occurring in the last months of Gestation, and persisting after Delivery.* By Dr. B. A. GOMEZ. (Rev. Méd.-Chir., August, 1855.)
2. *Extra-Uterine Gestation.*
    - A. By Drs. PARKHURST and ARMSBY. (Transactions of the State [New York] Med. Soc., 1855.)
    - B. Extra-Uterine Pregnancy, with successful result for the mother. By FR. MIKSCHICK and Dr. V. RITTERSHEIM. (Schmidt's Jarhrb., No. 9, 1855.)
    - C. Abdominal Gestation. By STERN of Steinau a. O. (Ibid.)
    - D. Extra-Uterine Gestation of Three and a Half Years. By Dr. BINET. (Un. Méd. March, 1855.)
    - E. A Case of Tubal Gestation. By SSOBOL'SCHTSCHINOFF of Tiflis. (Medic. Zeitung Russlands, 1854—1, and Monatsschr f. Geburtsk. Ap. 1855.)
    - F. Retroversion of the Gravid Womb. By GUICHARD. (Rev. Thér. du Midi, Avril, 1855.)
3. *Statistics of Labour and Birth.*
    - A. On the Statistics of Operative Midwifery in the Duchy of Nassau. By Dr. RICKER of Ettville. (Monatsschr. f. Geburtsk. August, 1855.)
    - B. Contributions to Obstetric Statistics. By Dr. VEIT. (Monatsschr. f. Geburtsk. August, 1855.)
    - C. Birth Statistics of England and Scotland—Reports of the Registrar-Generals. By Dr. FARR and Dr. STARK.

1. A lady came to Lisbon, aged twenty-six, having been married eight years, and had been eight times pregnant. During her last pregnancy, at the end of the sixth month she perceived a discharge of serosity from the vagina, accompanied by pains like those of labour; similar pains and discharges recurred at times, but she went on to the normal period, and was safely delivered. These discharges were repeated after delivery, and were especially remarked at the third and fifth months. They were not constant, but came on suddenly, were repeated several times in the same day, and then were not observed for some days. They appeared between and during the menstrual epochs, but were much more frequent during these epochs. The quantity evacuated each time was estimated at twelve ounces or more. Sometimes the serosity accumulated in the uterus before being discharged; during this, there was swelling of the breasts, intercostal pains, and emaciation.

The vagina and uterus were in the normal state. Minute examination, chemical and microscopic, of the fluid, excluded the idea that it was urine. It was however acid; it contained some mucous globules, and much epithelial detritus. It was not coagulated by heat or nitric acid.

2. A. The body of Mrs. Amos Eddy, aged seventy-seven, was examined by Dr. Parkhurst in 1852. She became pregnant in 1802, and died in 1852, carrying her fœtus fifty years. No unusual symptoms attended pregnancy; catamenia ceased; quickening at usual time. At end of eight and a half months she had severe labour-pains, following a sudden fright. Labour-pains continued for several hours, but subsided, and she remained comfortable for two or three weeks. Her health then began to decline. She was confined to her bed, and after a year and a half of extreme suffering, her health began to improve. During the rest of her life she had good health, but suffered occasionally from severe attacks of pain in the abdomen, which resembled labour-pains. After her health was restored, her catamenia returned, and continued until the age of forty-five. The specimen removed weighed eight pounds. The external surface of the envelope was smooth and white, composed of concentric layers of fibro-cartilage, from a line or two to

three-fourths of an inch thick. It had no connexion with the Fallopian tubes and omentum. The external surface of the fœtus was encrusted with earthy substance of sufficient thickness to preserve its form when dried. The interior seems to be a soft substance resembling adipocire. Dr. Armsby concludes by enumerating other cases of similar retention of an extra-uterine conception.

B. Dr. Mikschick's case is that of a woman, twenty-four years old, who had menstruated regularly up to September, 1854. In October, she experienced, after violent dancing, lancinating pains in the pelvis. Menstruation did not return, and in January, 1855, her size was obviously greater. At the end of March, she felt, after a brisk movement, a strong pain in the abdomen, which was followed by shivering and vomiting. Next day she was brought to hospital. Dry tongue; cool skin; pulse, 130 small; abdomen meteoric; bilious vomiting. In the abdomen was a swelling the size of the gravid uterus at six months; in the left side of this the heart-sounds of the fœtus were heard; the uterine rush was wanting. Os uteri closed; no part of child felt presenting. Leeches were applied. The peritonitis lasted three days, then the skin became icteric, and the strength failed. Pains came on, and some sanguineous discharge from the vagina; but examination detected no part of child. On the fourth day, the heart-sounds of the child could not be heard. After eight days, the tenderness of the belly had disappeared; the swelling could not be well isolated, its motility was slight. Gradually the swelling increased. On the 22nd May, the uterine sound was used: it penetrated three inches, and the uterus followed its movements. The swelling had now shrunk to the size of a large fist; it was uneven and hard. The patient recovered completely, and was discharged on the 2nd June.

The author considers this to have been a case of tubal gestation; that the growth of the fœtus had torn open the tube, and that this had caused the peritonitis; that the dead fœtus had undergone fatty metamorphosis.

The case of Dr. V. Rittersheim is similar. A woman, aged thirty-two, mother of a child nine years old, experienced a profuse uterine flooding, following upon pains, about three months after arrest of menstruation. A few weeks later she felt in the left side a knotty body; the abdomen began to increase gradually; the breasts swelled; and lastly, although no movements were perceived by the woman herself, the movements of the child could be distinctly felt by others on applying the hand. About the eighth month, strong pains seemed to indicate the advent of labour; but this did not follow. At a later period (not specified), menstruation returned, and lasted regularly until November, 1852. On the 20th Jan. 1853, the author first saw her. She complained of colicky pains in the abdomen; the hypogastrium was much distended; percussion gave a resonant sound to within two inches of the umbilicus, but was dull on either side. Moving was not painful. One inch above the umbilicus, several roundish, moveable substances, of considerable consistence, which were connected with a large body behind the symphysis. Some milky fluid in the breasts. The uterus was low down; the cervix much elongated. The sound penetrated easily. Closer examination showed that the child lay across the fundus of the uterus, the head somewhat higher, the breech deeper, and to the left. The size of the whole indicated a seven months' fœtus. On the 24th January, a severe attack of pains came on, with tympanitic swelling of the abdomen, small frequent pulse, cold sweat, which subsided under use of morphia and hot baths. A month later, menstruation again appeared, and recurred regularly every four weeks. The woman has noticed at the last epoch a sandy admixture with the menstrual blood, and complained of dragging pains in the bladder after holding her water. The volume of the ovum is (Dec. 1853) much shrunk since the first examination, and the head and extremities seem nearer the breech. Digestion is normal; and passing colicky pains appear at intervals.

C. We add another case of extra-uterine gestation, by Dr. Stern. A woman, aged twenty-five, who had borne a child in the normal way twelve years before, felt, in April, 1853, all the subjective symptoms of a new pregnancy; then followed an enormous increase in size, and after the fifth month, movements of the child and secretion of milk; and although menstruation recurred regularly, she expected her delivery with confidence. In Jan. 1854, pains came on, but the midwife declared, on examination, that no labour was at hand. After fourteen days, the pains suddenly ceased, with a strong shivering-fit; and general anasarca appeared, which subsided in nine weeks. Several physicians consulted, diagnosed ovarian dropsy. In September, she was seen by the author. The uterus was not enlarged, the vaginal portion not shortened; in the region of the right ovary was a roundish tumour. The woman was not seen again; and in Jan. 1855, she died. *Necropsy*:—The omentum was a decomposing mass: under it lay a full-developed fœtus in the first breech-presentation, with the head—its bones partly burst—in the right meso-gastric region. It showed but slight signs of decomposition. The umbilical cord was hollow; no trace of placenta or membranes. The uterus was in a very anæmic, decrepid condition; its cavity more contracted than usual.

D. For Dr. Binet's case we must refer the reader to the original.

E. Ssobol'schtschinoff's case is another interesting illustration of anomalous gestation. A soldier's wife, far advanced in pregnancy, twenty-seven years old, was, after a severe march across mountain-country, seized with acute pains in the abdomen, followed quickly by shivering, vertigo, fainting, nausea, bilious vomiting, anxiety, and restlessness. These symptoms increased in intensity, cold sweat covered the body, and in a few hours she died. No assistance had been rendered, beyond that of a midwife, who not being able to feel a presenting part, considered that labour was yet remote. It was learned that this woman had borne three children. The fourth pregnancy, excepting some pains at the outset, and the appearance of the menses for the first three months, had proceeded without disturbance until the fatal end.

Necropsy on the day after death.—Appearances of advancing decomposition. Serous effusions in the cavities. Milky secretion in the breasts. On opening the abdomen there was seen, lying free in the right side, a full-grown child, with the head to the right, the breech uppermost, partly covered by the liver, the bent extremities surrounded by the colon and omentum. The fœtus weighed six pounds; length, sixteen inches. All parts of the fœtus well-developed; nails hard. The *placenta* was of very loose tissue, blackish-brown, weighing six ounces, four inches in diameter. A seat of attachment could not be discovered. The umbilical cord, eleven inches long, was thin, easily lacerable, partly decomposing. The membranes were only to be recognised as shreds on the periphery of the placenta, or floating in the liquor amnii, which, to the amount of 4·5 pounds, had been poured out into the abdominal cavity. The appearances of the uterus bore the type of normal pregnancy; its body was situated in the upper pelvis, flattened from before backwards, and exhibited nowhere signs of mechanical lesion. The outer surface was smooth, cherry-brown, with dark spots, the inner surface is covered with a vascular, blood-infiltrated, easily-removable substance. The cervix uteri was swollen; the os admitted the finger. The left Fallopian tube was normal, the right destroyed to a rudimental state. This latter showed at the commencement a short canal, through which a probe passed from the uterus; at the outer extremity it ended in a warty body, of violet colour, and very vascular, and below was closely united to the broad ligament. The left ovarium was normal; the right one red, hardened, and enlarged; on section, homogeneous; no trace of Graafian follicles; the spermatic vessels of this side were much developed, whilst on the left side they were small and bloodless.

The author, although several particulars might be desired in this report, concludes that this was a case of tubal gestation, the fœtus bursting the original Fallopian envelope, and escaping into the abdominal cavity.

F. Dr. Guichard's case, Retroversion of the Gravid Womb, deserves to be placed in juxtaposition with the foregoing cases of extra-uterine gestation.

A woman who became pregnant in February, felt in March the first inconveniences of a retroversion of the uterus; several attempts made to reduce the uterus were unsuccessful. Notwithstanding the increasing size of the womb, the alvine and urinary evacuations were performed regularly. In April the symptoms of an abortion appeared, but abortion did not take place. In August and September, the seventh and eighth months of pregnancy, a diarrhœa, with difficulty repressed, set in, and a discharge of stinking masses followed by the genitals. In December, after the woman had for some time attended to her household duties, she felt a strong tenesmus follow an unusual swelling of the body. There was a piece of bone in the rectum, which, extracted, turned out to be the half of the frontal bone of a three or four months' fœtus. An examination of the rectum revealed an opening in its anterior wall, through which was felt an agglomeration of pieces of bone, some more of which escaped afterwards. At present, after two years from the occurrences related, the woman is quite well; menstruation takes place regularly, pregnancy has not again happened, the uterus has its normal direction, its body presents, however, a considerable volume, conjectured to be owing to the remains of the fœtus. (Is it not possible that this was a case of extra-uterine gestation? If a case of retroversion of the gravid womb, it illustrates a mode of recovery, without reduction, through the natural efforts not often observed.—REP.)

The memoir of Dr. Ricker is especially deserving of full analysis, because it exhibits an example of statistical research that fulfils most of the necessary conditions of accuracy and completeness. He gives a complete statistical account of the operative obstetrics for the whole Duchy of Nassau, for the years from 1821 to 1842 inclusive. The population of the Duchy is 429,341; there are a hundred private physicians, and about twenty more partly engaged as military or bath physicians. All are required every six months to make a return of their cases of obstetric operations. These are collected and systematically analysed.

1. *Forceps Operations.*

After a close enumeration, it has been found that in the twenty-two years extending from 1821 to 1842, there were 304,150 births in Nassau.

Of these, 4223 were terminated by the aid of the forceps—i.e., 1 in $72\frac{94}{4223}$. It is, however, remarked, that in the earlier years the reports were less full than later, and that many operations remained unrecorded, so that 150 or 200 more forceps-deliveries ought to be added. This would give about 1 in 70.

The indications for the use of the forceps are specified in 708 cases only.

Disproportion between child's head and mother's pelvis gave rise to operation in 287 cases.

Feebleness and absence of pains gave rise to operation in 269 cases.

Weakness, exhaustion, and illness of the mother, indicated speedy delivery by forceps in 33 cases. Under this head are found excessive difficulty of respiration, phthisis, asthma, suffocation, large hernias.

Prolapsus of the umbilical cord with the head led to use of forceps in 29 cases.

Spasmodic and preternaturally painful pains indicated use of forceps in 22 cases.

Free-presentations, in 20 cases.

Eclampsia and convulsions, in 12 cases.

Prolapsus of smaller parts of child with head, in 8 cases.

Placenta prævia, in 3 cases; and *Hæmorrhages of a different kind* during labour, in 7 cases.

Oblique presentation (Schieflage des Kopfes) *of the head* led to application of forceps to rectify position, and to extract, 7 times.

Rigidity of the structures in elderly first-bearing women, in 4 cases; *great swelling of soft parts,* in 4 cases; and *erysipelas pudendorum,* in 7 cases.

Putrescence, and consequent unusual contractility of the uterus, in 1 case.

Result of the forceps operations.—Out of 4223, 93 women died either during or soon after the operation, and 684 children were still-born. Hence, 1 woman out of $45\frac{38}{93}$ delivered by forceps, died; and 1 child out of $6\frac{419}{684}$.

2. *Turning.*

Turning by the head and *turning by the feet* are distinguished:

10 cases of head-turning are recorded, of which 9 ended favourably to mother and child; in the tenth the child perished through morbid contraction of the pelvis. Dr. Ricker points to this as far happier than the result of foot-turning, and a strong incentive to further experiment.

Foot-turning was resorted to 2473 times, or 1 in 123 times.

The *indications* for turning were stated in 530 cases: cross-presentation was the cause in 388 cases; placenta prævia, 82 times; prolapsus of the umbilical cord, 28 times; contraction of the pelvis, 18 times; hæmorrhages, 5 times; various dangerous diseases of the mother, 4 times; face-presentation, 2 times; oblique position of the head, 2 times; convulsions, 1 time.

The *results* of these 2473 cases of turning were:—176 mothers died, and 1431 children were either still-born or died shortly after birth. Thus 1 mother in $14\frac{9}{176}$, was lost; and for every $1\frac{1042}{1431}$ child, one was lost.

Dr. Ricker remarks that, in the majority of cases, midwives being at first in attendance, the cross-presentation was rarely detected until after the membranes had burst, a circumstance which undoubtedly accounts for a part of the mortality.

3. *Perforation.*

The scientific and experienced obstetrician is aware that there are many cases of narrowing of the pelvis, where help from the forceps is not to be expected; that turning by the feet often leads to the wished-for goal; and that there will remain proportionately fewer cases in which recourse to the *jus gladii* is necessary.

143 perforations were performed—that is, 1 in 2126 labours. Of these, 88 mothers were saved, 35 died, and concerning 20 there is no certain information.

4. *Embryotomy.*

There were 22 cases of embryotomy; 16 mothers recovered, 6 died. This gives 1 embryotomy in 13,825 deliveries.

5. *Artificial Premature Deliveries.*

By this is understood the operation undertaken for the purpose of effecting the delivery of the fruit before the normal end of gestation, and when viable. Hence this is distinguished from artificial abortion, and from the accouchement forcé.

The indication is a contraction of the pelvis to such a degree as to render normal birth dangerous or impossible; this exists when the smallest diameter is from three and a quarter to two and a half inches.

Several different methods have been employed: warm baths, frictions of the uterus, warm douches to the os uteri, warm injections into the uterus, partial separation of the membranes from the lower segment of the uterus, the plug, the apposition of a bladder filled with warm fluid, artificial and gradual dilatation of the cervix by the finger, and by the perforation of the membranes. We cannot, for want of details, give an exposition of the value of these means.

Only 3 artificial premature deliveries were effected: 1 by uterine frictions, 1 by a sponge placed in cervix, 1 by puncture of membranes: that is, 1 in 101,383 deliveries. All the mothers lived; 2 children survived, 1 was dead.

6. *Cæsarean Section in the Living.*

This operation was performed 12 times (of which in 1 the abdomen only was opened). 2 mothers and 7 children were saved; that is, 1 section in 26,000 deliveries. Contrasting this result with that of perforation or embryotomy, Dr. Ricker says, that since 9 lives—i.e., 2 mothers and 7 children—are saved, whilst in the latter cases all the children and one-third or one-fourth of the mothers, perish, the Cæsarean section is rather more favourable.

7. *Cæsarean Section in the Dead.*

This operation was performed 27 times. The section of the abdomen only 2 times: 1 for abdominal gestation, 1 for rupture of the uterus. In 4 cases the dead pregnant woman was delivered in some other way, namely, once by forceps after death by convulsions, and 3 times by turning. In no one of these cases was a living child extracted.

From the returns it appears that 211,568 persons died from 1821 to 1842 inclusive. It thus appears that for 6411 deaths and 9436 births, there was *one* dead pregnant woman upon whom the Cæsarean section, or some other artificial delivery, had been completed.

Dr. Ricker concludes by observing, that it would be of great interest to procure like returns in other countries, in order to facilitate comparisons with those now recorded.

B. These statistical researches, by Dr. Veit, are in continuation of others which have been analysed in a previous Quarterly Report.

Dr. Veit says, that in Prussia the proportion of children born out of wedlock is as one to three of those born in wedlock.

A comparison of 2550 observations gave a mean of seven Prussian pounds as the weight of a full-grown child.

466	weighed up to	6	pounds	= 18·3	per cent.
1066	”	7	”	= 40·3	”
767	”	8	”	= 30·0	”
291	”	10½	”	= 11·3	”

The boys weighed, on an average, 7·09 pounds; the girls, 6·88.

893	boys of	primiparæ	weighed	6254½	pounds;	1 = 7·00
419	”	multiparæ	”	3053½	”	1 = 7·28
799	girls of	primiparæ	”	5425	”	1 = 6·78
440	”	multiparæ	”	3107	”	1 = 7·06

Hence the children of multiparæ exceed those of primiparæ in weight.

The Duration of Labour.—I date the beginning of labour from the moment when the patient feels the first true, although weak, pains; the beginning of the second stage from the full dilatation of the os uteri; and its end, and with it the end of labour, at the end of the expulsion of the child. The 9731 children embraced by these statistics were all born by head presentations. The total time of the whole births was 142,112·56 hours, the average 14·60 hours. The average time of 5046 boys was 14·78; of 4685 girls, 14·40. The average time of 3483 primiparæ was 19·52; of 6248 multiparæ, 11·68 hours.

From a closer analysis of 2550 labours, of which fuller details were possessed, Dr. Veit found that the half of all labours of primiparæ ended within eighteen hours; of multiparæ, within nine hours. A labour not exceeding one hour in multiparæ was found in 0·9 per cent.

The duration of the first stage of labour was, in 1692 primiparæ, on an average, 20·32 hours; in 858 multiparæ, 14·16.

The duration of the second stage in 1692 primiparæ, on an average, 1·72; and in 858 multiparæ, 0·99 hours.

We thus see that the difference of time of first stage between primiparæ and multiparæ is 6·16 hours; and of the second stage, 0·73 hours. The size of the child has only a marked influence over the second stage, and this is only perceptible in primiparæ.

The influence of labour upon the life of the child is not easily determined from observations in a lying-in hospital. The experience throughout the whole country, although still open to fallacies, gives truer results. In Prussia there were born in the fifteen years from 1820 to 1834, according to Hoffmann, a yearly average of 506,201 children; and of these, 17,138 were still-born, or 1 in 29·5. An average of the ten years from 1837 to 1846 gives 600,323 children; of which

22,939 still-born, or 1 in 26·2. Casper has further proved that illegitimate birth has the largest share in the returns of still-born children. In the years 1819—1822 every twenty-fifth child born in wedlock was dead, whilst of illegitimate children every twelfth was dead.

Dr. Veit's investigation into the *influence* of the *sex of the child on its mortality* in labour, is full of interest. Examining the grounds upon which Joseph Clarke based his conclusion, that the greater mortality of boys was owing to their greater size and weight, and consequently to the greater pressure upon the head, he shows that this circumstance does not sufficiently account for the different mortality. He cites the objection of Casper as having much weight—viz., that the longer life-duration of the female sex must be admitted as having a deeper relation to this question. Again, he says, the difference in development between boy and girl is too inconsiderable to exert so great an influence upon the life of the child. He found the difference between boys and girls, whether first-born or other, to be only 0·22 of a civil pound; and the difference in the head-circumference to be only six lines. Clarke fixed the difference of measurement at 0·366 inch. Further, Dr. Veit is in a condition to prove that even in *like* bodily development more boys than girls always die. He has studied the influences of weight in 2550 children, under the most simple conditions, in Busch's 'Clinique.'

Thus there were:	up to 6 pounds,	213 boys and	253 girls.
	„ 7 „	497 „	529 „
	„ 8 „	425 „	342 „
	„ and over	177 „	114 „
		1312	1238

The following table results from observations upon the mortality of these children: per centual proportions.

Weight.	Boys.		Girls.		Total.	
			Apparently Dead.			
Six pounds	5·16	...	4·35	...	4·70	5·02
Seven pounds	5·83	...	4·53	...	6·16	
Eight pounds	8·00	...	4·38	...	6·38	6·80
Over	9·04	...	6·14	...	7·90	
			Still-born.			
Six pounds	2·34	...	1·97	...	2·14	1·34
Seven pounds	1·20	...	0·75	...	0·97	
Eight pounds	0·47	...	0·29	...	0·34	0·94
Over	1·69	...	3·50	...	2·40	
			Dead after Birth.			
Six pounds	4·69	...	2·76	...	3·64	2·61
Seven pounds	2·81	...	1·50	...	2·14	
Eight pounds	3·05	...	2·63	...	2·89	2·74
Over	2·82	...	1·75	...	2·40	

This table indeed proves that weight has an influence upon the result of the labour, but it also proves that the difference of weight is *not the only* factor which enters into the solution of the question—why are boys in greater danger than girls? To illustrate this further, Dr. Veit examines the proportion of deaths in boys and girls, as connected with the duration of labour and with first or subsequent labours. He finds the following conclusions: 1. The danger for the child, when the birth is completed within twelve hours, is only half as great as when the labour is protracted to twenty-four hours; and that further protraction is still more dangerous. 2ndly. The danger is visibly increased when the second stage of labour lasts longer than two hours. 3rdly. Both in like duration of entire labour, and in like duration of the second stage in particular, the male sex is more endangered than the female.

The proportion of single and plural births.—In Prussia there took place, in twenty-four years, 13,360,557 labours, of which 151,689 were plural—that is, 149,964 twins, 1689 triplets, and 36 quadruplets.

The difference in the proportion of the sexes of children born.—Upon the same numbers as the preceding it was found that the gross proportions of girls to boys was as 100 : 105·88, and in plural births as 100 : 105·28. Thus the preponderance of the male sex is somewhat less in plural than in single births.

C. To these statistical facts we think it useful to add the analogous facts for England and Scotland issued on the authority of Dr. Farr and Dr. Stark.

Estimating the population of England, in 1850, at 17,766,129, there were registered 593,422 births in that year, or 3·340 births to the whole population, and 3340 to every 100,000 persons living.

In all England the births are registered in the greatest numbers in the *three months* of April, May, and June; but it will be noticed that the distribution of births over the four seasons differs in the different counties. Thus in Kent, extra-metropolitan, Hants, Hertford, Bedford, and Devon, the births are the most numerous in the first quarter of the year.

Boys and Girls.—In every county of any great extent of population, the male exceed the female births. In 1850, 104 boys were born in England to every 100 girls.

If the children are distributed into two classes, it is found that the boys born in wedlock are to those born out of wedlock as 13·78 to 1; while the girls of the corresponding classes are as 13·66 to 1. The reason of the discrepancy is this: the excess of boys *out of wedlock* over girls (103·38 to 100) is not so great as the excess of boys among the children born in wedlock (104·28 to 100).

The mortality of males in the first five years of life exceeds the mortality of females in the proportion of 7 to 6—for 72 of every 1000 boys, and 61 of every 1000 girls in the population, under *five* years of age, die annually; and again, at the age of five to ten, boys die in rather larger proportions than girls. From the age of ten to the age of thirty-five the mortality is greater among women than it is among men; but after the age of forty-five the mortality of men greatly exceeds that of women.

In 1851 the population of England by census was 17,982,849. The births were 615,865, or in the proportion of 3·425 to the population, or 3425 to every 100,000 persons living. This is nearly 1 in 29, a much higher rate than had been before observed.

Boys and Girls.—To every 1000 girls, 1047 boys were born; but to every 1000 girls born out of wedlock only 1039 boys were born. The sex of the child is supposed to be influenced, to a certain extent, by the relative ages of the parents; but the truth of this cannot be tested by the English returns. 42,000 children were born out of wedlock in 1851—that is 6·8, or nearly 7 in 100 of the total births.

In 1851 there were in England and Wales 2,553,894 married women under the age of fifty-five (the extreme of the child-bearing period), and the children born alive in wedlock were 573,865; so that 22 in 100 bore living children. The number of unmarried women, spinsters and widows included, of the same age (fifteen to fifty-five), was 2,449,669, and as the number of children born out of wedlock was 42,000, it would appear that to 100 of them 1·7 children were born.

The greatest number of births took place in the two first quarters of the year.

In 1852 the population of England is estimated at 18,205,627; the births registered were 624,012—that is, in proportion of 3·428 to the population, and 3428 to every 100,000 persons living.

Boys and Girls.—To every 1000 girls there were 1046 boys. 42,482 children were born out of wedlock, or 6·8 to every 100 born.

Twins.—In 6036 cases, women bore *two* living children at a birth; in 37 cases, three living children. In 15 cases the triple births consisted of *three boys;* in 10 cases, of *three girls;* in 7 cases, of *two boys and one girl;* in 5 cases, of *two girls and one boy.* It is evident that in these cases the boys preponderate, and that the cases in which the children are of the same sex occur in undue proportion. Amongst the twins, in 3587 instances the children were of the same sex, and in only 2159 of different sexes.

(In comparing these figures with those of the Duchy of Nassau, it is essential to bear in mind that in England *no account is taken of still-born children;* the living children only are registered.—REP.)

In Scotland the Registration Act only came into operation in Jan. 1855. The returns given refer to the first six months only. It is considered that there was some deficiency in the returns for the first three months. The Reporter considers it better to quote the figures of the second trimestrium only. The population of Scotland by census in 1851 was 2,888,742. The births for the quarter specified were 25,438, giving the proportion of 95 females to 100 male births, or exactly the general proportion of the sexes at birth in England.

III. FŒTAL DEVELOPMENT.

1. *Cases of United Twins.*
   - A. Case of United living Children (Russian). (Monatschr. für Geb. July, 1855.)
   - B. A Description of the United African Twins exhibited in London. By Dr. RAMSBOTHAM. (Med. Times and Gaz., Sept. 29, 1855.)

A. On the 4th of April, 1855, one of the rarest cases of double formations occurred in the St. Petersburg Foundling Hospital—namely, two girls growing together by the skulls—which still live, and so far appear to be in good health. Of all the cases of this kind hitherto known (of which there are seven), the union of the two individuals was never of that kind to bring the face of one child directly opposite to the face of the other. These twins are so united that if the middle line of the face of one child be prolonged from the nose, this would strike upon the ear of the other. Through the mobility of the necks the two children really lie in a straight line, one girl lying on the back, the other on the side, and thus they sleep. The face of one child is quite symmetrical as far as the forehead, and it is first in the formation of the skull that want of symmetry appears. In the face of the other, the right half is much shortened, and the eye of this side opens less than the other. The two children possess a perfectly independent existence from each other as relates to sleeping, waking, want of food, and so forth. The one sleeps quietly, whilst the other takes nourishment or looks about. Common sensibility does not appear to exist, since in cases of this kind the brains and nerves of each individual are preserved distinct. Not so always with the blood-vessels. Once one child screaming loud awoke its sister. The face of the screaming child became suffused and reddened deeply, whilst the other was still asleep. Then the face of the other began to redden and swell, and it was only after this that it opened its eyes. The features of the two children, especially of the one whose face is not shortened, are very pleasing. The physicians of the Foundling promise to observe this case more fully, and publish the results.

B. We must refer to the 'Medical Times and Gazette' for Dr. Ramsbotham's account of the united African twins.

MEDICAL INTELLIGENCE.

The late Mr. Pilcher.

MOST of our readers have ere this seen the announcement of the death of a valuable member of the profession, Mr. Pilcher; but we feel assured that some memorial of a man so justly and generally esteemed will be deemed acceptable.

Mr. Pilcher entered on his professional career in Bristol, where he served an apprenticeship with the late Mr. Hill, a general practitioner of that city. He there displayed those qualities by which in after life he was so eminently distinguished. He subsequently became a student at the Borough hospitals, and during that period resided with the late Edward Grainger, by whom, in common with all who enjoyed the instructions of that remarkable and distinguished man, he was inspired with an enthusiastic love for medical science, which endured to the last day of life. For some years, Mr. Pilcher taught anatomy in the Webb-street School, and afterwards he occupied the Surgical Chair at Grosvenor-place School, where, on the day of his death, he had delivered his lecture as usual, and, as it was remarked, with more than ordinary energy.

The characteristics by which the subject of this notice was more particularly distinguished, were unwearied industry, great judgment, and a benevolence of heart and kindness of manner, which gained for him universal esteem. Mr. Pilcher was more generally known in connexion with aural surgery, to which his attention became particularly directed from having obtained, in 1838, the Fothergillian gold medal of the Medical Society of London, for a prize essay 'On the Economy and Diseases of the Ear.' This treatise must be deemed an important addition to our knowledge of aural affections, and was at the time of publication the only collective essay on the subject in this country. The author, at the time of his decease, was engaged in preparing a new edition, for which he had collected a large amount of valuable matter, and which, we trust, will not be lost to the profession. His death has also deprived the readers of this Review of a comprehensive article on the present state of aural surgery, which Mr. Pilcher had undertaken to supply.

Although engaged particularly in the above department, Mr. Pilcher was very successful as an operative surgeon, in which capacity he was more extensively consulted than was generally known. In hernia and lithotomy he was very successful; and his extensive knowledge of surgery, and of disease in general, inspired confidence among his professional friends, and enabled him to treat surgical cases with great success. He was exceedingly zealous in his search after pathological knowledge, never allowing the opportunity of a post-mortem examination to escape him, whatever might be the trouble and personal inconvenience incurred.

As a teacher, Mr. Pilcher gained that best proof of success, the esteem and love of his pupils, into whom he infused the same ardent love for his profession which he himself so largely possessed.

His lectures were valued equally for the extent of knowledge, theoretical and practical, displayed in them, as for the clearness and perspicuity which sprung from command of language and apt illustration. One well competent to judge, has informed us, that some of these lectures, especially those on inflammation, irritation, hernia, and injuries of the head, were perfect treatises on the subjects to which they related.

In private life we believe no man was more esteemed; his benevolence was unbounded; and many have to lament the loss of a generous and most considerate benefactor. No one who knew Mr. Pilcher could avoid being struck with a benignity of manner which, in his case, was the true index of the heart.

We have before us the letter of a most distinguished member of our profession, whose sentiments will, we are assured, find an echo in a large circle of admiring and sorrowing friends. He writes: "I regarded our friend as a brother, and I grieve for his loss as a bereaved brother. Through a long series of years, under all changes and chances of fortune, in sickness and in health, in joy and sorrow, he was always the same sympathizing and faithful friend. I could always go to him for counsel and help, sure to find all the assistance which a clear and sound judgment, and an affectionate heart, could give."

Mr. Pilcher was a member of several professional and benevolent societies; a Fellow of the College of Surgeons; and a Member of the Council, to which distinguished post he was twice elected.

Mr. Pilcher died at his residence in Harley-street, on November 7th, having been seized whilst at dinner with hemiplegia; this was rapidly followed by insensibility, and death ensued in six hours after the attack.

A deputation from the teachers and pupils of the school where he taught; another from the Literary Society of which he had been the earnest promoter; together with many relatives and friends, followed his remains to Kensal Green; and it may be truly said, that this distinguished surgeon and worthy man descended into the grave amid tears and sorrow.

The Army Medical Officers in the East.

It was an axiom of Napoleon I., that "rewards are the food which nourishes military virtue;" and it is a remark as forcible in its application to the medical as to the other executive officers of the army. But the principle laid down by this great man does not appear to be recognised by the supreme military authorities in this country. While promotion and honour have been lavished with unsparing hand upon the other officers—both staff and regimental—the Medical Officers have been left unnoticed, unrewarded, unhonoured. We understand that some officers have been promoted, on the recommendation of the Director-General, for meritorious services, but we are unable to state the number or their names. In the other ranks such promotions have always been Gazetted as "for distinguished services in the field;" but in the case of Medical Officers this has not been done, and thus one-half the credit has been filched from them, and its effect as a stimulus to honourable exertion on the part of the others, has been lost.

But these very promotions bring out in bold relief the injustice with which the Medical Department has been treated. Lord Hardinge has been made Field Marshal, Lord Panmure a G.C.B., while Dr. Smith, whose exertions have been beyond all praise, and who conducted the responsible duties of his department at least with as much efficiency as either of them, has not only been unrewarded, but, after being acquitted by a Committee of the House of Commons of the grave but unjust charges lavished upon him, has been left to the vituperative abuse of the *Times*, without any step being taken by the Government to save him from this injustice, by a public expression of their opinion of his zeal and efficiency. Lord Hardinge stated before the Sebastopol Committee, that he considered Dr. Smith a "very faithful and good public servant." Why, then, has he not recommended her Majesty to stamp her approbation of his conduct by some honorary reward? But the same invidious distinction has been observed in the Crimea. While the various grades of the Order of the Bath have been showered upon the others, not a single Medical Officer has been included in the lists. It may be true they are not so much exposed to the fire of the enemy as the others, but the number who have been killed and wounded in the present war, proves that they are by no means exempt from this danger, and they are required to show a much higher degree of courage than the other ranks, for in the midst of danger and general excitement they must, to be efficient, remain cool, collected, and unflinching. That as a body they have nobly done their duty, is the unvarying testimony of all who have had personal opportunities of judging. But it is still more clearly shown by the numbers who have fallen victims to that pestilence which proves far more destructive to an army than the sword of the enemy. Many of them died at their posts who might have saved their lives by a timely retreat, but were kept back by a conscientious sense of duty; and many more have come home as invalids, with broken constitutions, broken hopes, and broken spirits from a keen sense of the neglect with which they have been treated by their country. Is it, then, surprising that our letters should speak of a general feeling of discontent and dissatisfaction at the service, among the whole of the members of the department? Of an average of about 600 medical officers, 42 have died, and 160 been invalided since the beginning of the war, but *not one* has been decorated, except with that medal which has been given indiscriminately to all, from the general commanding, down to the smallest drum-boy. When Parliament assembles, we trust Sir De Lacy Evans—who well knows their worth—will ask on what principle the Medical Officers have been excluded from all honours, and will agitate till he obtain for those of the Crimean army that share of justice which he was before so instrumental in extorting for the medical officers of the army generally.

To contribute our feeble quota towards doing justice to the memory of those medical officers who have nobly sacrificed their lives in the discharge of their duty with the army in the East, we subjoin the following nominal list:

Deputy Inspector-General.

Thomas Spence, M.D.	Nov. 14, 1854.

Staff Surgeons, First Class.

G. K. Pitcairn, M.D.	Aug. 16, 1854.
John Mitchell, M.D.	Sept. 24 „
John Marshall	Feb. 10, 1855.
Chilley Pine	March 6 „

Surgeons, Regimental and Second Class Staff.

F. C. Huthwaite, Grenadier Guards	Sept. 30, 1854.
Peter Mackey, M.D., Staff	Oct. 3 „
D. Anderson, M.D., Staff	Nov. 5 „
William Browne, 95th Regiment	Nov. 26 „
William A. Anderson, 41st Regiment	Jan. 3, 1855.
John Newton, Staff	Jan. 26 „
Fras. Smith, 95th Regiment	Feb. 9 „
M. A. Jane, Staff	March 7 „
Christ. Macartney, M.B., 77th Regiment	April 11 „
James A. Wishart, M.D., Staff	May 25 „
Walter Simpson, M.D., 17th Regiment	May 31 „

Assistant-Surgeons.

E. A. Jenkin, 23rd Regiment	Aug. 2, 1854.
Frederick Y. Shegog, 88th Regiment	Aug. 28 „
Ph. G. Martel, 50th Regiment	Sept. 11 „
James A. Shorrock, Rifle Brigade	Sept. 21 „
James Thomson, M.D., 44th Regiment	Oct. 5 „
A. R. Reed, M.D., Staff	Oct. 5 „
Henry Beckwith, 49th Regiment	Oct. 18 „
John James Norris, 55th Regiment	Nov. 22 „
Edward P. Boyle, Staff	Dec. 8 „
Joseph Lamont, M.D., 41st Regiment	Jan. 5, 1855.
Alexander Struthers, M.D., Acting	Jan. 19 „
John P. Langham, 7th Fusiliers	Feb. 4 „
E. S. Wason, M.D., 13th Regiment	Feb. 8 „
Frederick A. Macartney, Staff	Feb. 12 „
John Grabham, 71st Regiment	Feb. 16 „
William Renwick, 14th Regiment	March 4 „
Frederick Graham, Acting	March 21 „
Harvey Ludlow, Acting	April 5 „
Robert T. Simons, Acting	April 28 „
John H. White, Acting	July 3 „
Malcolm C. Ancell, 11th Hussars	Aug. 10 „
John Longmore, Acting	Aug. 22 „

Hospital Dressers.

—— Harrison	May 23, 1855.
—— Fell	Aug. 2 „

Principal Apothecary.

George Home Reade	Nov. 28, 1854

Dispenser of Medicines.

—— Whitwell	Sept. 2, 1855.

List of *Killed.*

Surgeon Le Blanc	9th Regiment.
Assistant-Surgeon O'Leary . . .	68th „

Wounded.

Surgeon Gordon	95th Regiment.
Assistant-Surgeon Wilson . . .	7th Hussars.
„ „ Reade	Rifle Brigade.
„ „ Cockerell . . .	Royal Artillery.
„ „ Lundy	79th Regiment.

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THE

BRITISH AND FOREIGN

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APRIL, 1856.

PART FIRST.

Analytical and Critical Reviews.

REVIEW I.

1. *Report of the Medical Council to the Right Hon. Sir Benjamin Hall, Bart., M.P., President of the General Board of Health, &c., &c., in relation to the Cholera Epidemic of* 1854. Presented to both Houses of Parliament by command of her Majesty.—*London*, 1855. pp. 9.
2. *Report of the Committee for Scientific Inquiries in relation to the Cholera Epidemic of* 1854. Presented to both Houses of Parliament by command of her Majesty.—*London*, 1855. pp. 129.

Appendix to the Report of the Committee for Scientific Inquiries in relation to the Cholera Epidemic of 1854. pp. 322.

3. *Reports relating to the Sanitary Condition of the City of London.* By JOHN SIMON, F.R.S., &c.—*London*, 1854. pp. 312.
4. *Report of the Commissioners appointed to inquire into the Causes which have led to or have aggravated the late outbreak of Cholera in the towns of Newcastle-upon-Tyne, Gateshead, and Tynemouth.* Presented to both Houses of Parliament by command of her Majesty.—*London*, 1854. Fol. pp. 580.
5. *Report made by Dr. Milroy to the Colonial Office on the Cholera Epidemic in Jamaica*, 1850–51. Ordered by the House of Commons to be printed, 1854. Fol. pp. 147.
6. *Report on the Cholera which attacked the Fleet in the Black Sea in August*, 1854, *more particularly as relates to her Majesty's ships*, Britannia, Albion, *and* Trafalgar. Printed by order of the Right Hon. the Lords Commissioners of the Admiralty, 1854. Fol. pp. 15.
7. *Report upon the Sanitary Condition of New Orleans.* By E. H. BARTON, M.D. pp. 249. Being an Appendix to the *Report of the Sani-*

tary Commission of New Orleans on the Epidemic Yellow Fever of 1853. Published by authority of the City Council of New Orleans. pp. 542.

8. *Report on the Cholera Outbreak in the parish of St. James, Westminster, during the Autumn of* 1854. Presented to the Vestry by the Cholera Inquiry Committee, July, 1855.—*London*, pp. 175.

To investigate the history of epidemic diseases, to trace them from their commencement through their periods of growth, culmination, and decline; to mark the circumstances under which they arise, the character and condition of the localities in which they prevail, and of the persons they attack, forms one of the most interesting of medical inquiries. To determine the nature of their proximate causes, the laws which govern them, the conditions requisite for their production or their active development, and the possibility of their prevention or mitigation, is perhaps the most important of medical problems. The thorough elucidation of the former affords the best chance for solving the latter. For this purpose, however, it is necessary not merely to investigate individual cases during the existence of an epidemic visitation, but also to inquire into the character of successive outbreaks in various localities; to ascertain the several phases presented by the same disease at different periods in the same and in different places, and to inquire into the influence exercised over the character of the disease by the varying state and conditions of the populations amidst whom it prevails. Rightly to appreciate this influence, it is necessary to become acquainted with the ordinary result of the circumstances under which the people are placed. Without the knowledge of what may be termed the normal effect of these, it is impossible to estimate justly their agency in disease. Thus the history of an epidemic should comprise some account of the state of the public health, both previous and subsequent to its visitation, as well as of the conditions by which it is affected. Unfortunately, such investigations have hitherto only been entered on when the enemy has given undeniable evidence of its presence; most frequently not until after it had reached or even passed its acme. Investigations so extensive as those at which we have thus briefly glanced, would require for their full realization the co-operation of many observers. Hitherto no such system of observation has ever been organized in this country, nor could it be effectually carried out excepting under the direction of Government, such inquiries being totally different from the everyday duties of the busy practitioner. The only disease in which there has been any attempt on the part of the public authorities to institute such inquiries, is cholera; and, as already intimated, these investigations have rarely commenced until the first force of the disease had expended itself. There has been no systematic inquiry as yet, into the ordinary effect of the causes which are supposed to be necessary for the more malignant development of this pestilence. Much valuable matter illustrative of the nature and history of cholera, has nevertheless been collected, both by public and private inquirers; from which, as contained in the several documents whose titles are placed at the head of this article, together with such as we have gleaned from other sources, we propose to put together the chief features in the local history of cholera, so far as these may seem to be at present ascertainable.

It is commonly accepted that cholera, after prevailing occasionally in India from periods co-eval with the earliest records, appeared with almost unprecedented violence in the Delta of the Ganges in 1817, and from thence as a centre gradually extended itself over the Indian continent. After a time it spread into China, reached Ceylon, and other islands of the Indian Ocean, and crossing the Equator, broke out in the Isle of France. Taking a westward course, it reached Persia in 1821, where it lingered, abating with the cold of winter, and regaining strength with the summer heat, until 1823. In the latter year it appeared in the southern provinces of Russia, and attacked Astrachan in the month of September. It here paused upon the very confines of Europe, and for several years its course in a westward direction was arrested. In 1829, six years after its first visit to Astrachan, it reached Orenburgh by way of Tartary, and in the following summer re-appeared in Astrachan, where it proved much more fatal than at its former visit. It now rapidly extended westward and northward. Following chiefly the course of the great rivers; it reached Moscow, the ancient capital of Russia, in September, and showed itself within a short distance of St. Petersburg before winter. In the following spring it extended to Archangel, the most northern port of Europe, and through Riga, Dantzic, and other towns on the borders of the Baltic to Hamburgh. Late in October it appeared at Sunderland, on the north-east coast of England, where it was believed to have been introduced by a ship from Hamburgh. Such is a brief but tolerably accurate sketch of the geographical history of epidemic cholera, and of its supposed advent in Britain. Let us now give a short glance at the leading features of its history as an epidemic in this country.

Very soon after the appearance of the pestilence in Sunderland, its presence was recognised in several other parts of the kingdom. Altogether the first epidemic extended from October, 1831, to December, 1832, and carried off upwards of 30,000 persons. The next great epidemic, after following a very similar course, appeared at Astrachan in June, 1847; at Moscow in September; at Berlin in June, 1848; Hamburgh in August; in London and other parts of England in September. It is, however, worthy of note, that of the 1934 deaths referred to cholera in 1848, 829 occurred in the first nine months of the year, and previous to the presumed arrival of the epidemic from abroad. The third great outbreak in this country commenced at Newcastle-upon-Tyne, on the 1st of September, 1853. Before the end of the month it had appeared in the metropolis and in other parts of the kingdom. As on former occasions, the pestilence had ravaged Germany some time before its arrival in England. The epidemic now, for the first time, visited Copenhagen, where 4083 persons fell victims to its attack out of a population of about 125,000. This history has commonly been considered as affording *primâ facie* evidence that cholera is an exotic disease, the product of another climate, which, transplanted by human intercourse into this country, or spreading thither from its eastern birth-place by means of some hidden telluric influence, has here met with conditions favourable for its development. By most persons the existence of a poison, somehow introduced from abroad, is believed to be necessary for the production of cholera, although majority of inquirers disbelieve in its spread chiefly by means of contagion. To explain the production of cholera in England in accordance with this view, and with the undeniable correctness of the main points, in the brief outline of its history here given, several explanations have been proposed. Into a consideration of the nature or plausibility of these theories it forms no part of our present intention to enter. They all agree in assigning an eastern origin to the poison of cholera, which is believed to have either reached this country by means of direct human intercourse, as by fomites or individual contagion; or the poison itself is assumed to be migratory, and to have come hither by a kind of wave-like extension from India. With the exception of a single, and in our belief unproved, opinion,—very attractive from its simplicity, but at variance with a great many facts in the history of cholera,—which considers the specific poison to reside in the peculiar excreta from the gastro-intestinal mucous membrane of cholera patients, all these theories consider the existence of certain local conditions, or of a predisposition in the inhabitants of infected districts, as usually necessary to give strength and vitality to the presumed poison. Thus it will greatly depend upon the degree of this local or personal predisposition whether the poison excites a moderate outbreak extending over a considerable period, or a sudden and violent explosion, as at Gateshead, on Christmas eve, 1831; as in the memorable outbreak in St. James's, Westminster, so admirably described in the report presented to the vestry by the cholera inquiry committee; or as on board H.M.S. *Britannia* in the Black Sea, both in 1854. Since, however, the local circumstances which are thus believed to give energy to the ferment are pretty constant, there is a difficulty in understanding why the disease should cease so suddenly as it often does; unless, indeed, we adopt the solution suggested by Mr. Simon, who surmises that the local atmosphere is after a time exhausted of its capability for further zymotic action, just as the fluids of a person who has recently passed through an exanthematous disease are, for a time at least, incapable of being similarly influenced, although again brought into relation with the specific poison. Neither, if cholera be altogether dependent upon any form of contagion for its spread, is it easy to understand how it should ever cease to extend itself in a large city so long as there remain any persons upon whom the poison can act. In truth, whilst each of the opinions is apparently supported by a large amount of evidence, each is equally opposed by a considerable number of obstinate facts.

No doubt "the phenomena of this disease, however capricious they may seem, are obedient to some absolute uniformity as yet beyond our ken—are sustained by that same rigid sequence of cause and effect which is imposed on all remaining nature;" but no explanation hitherto proposed meets the circumstances of the case in that universal manner which must coincide with truth. In fact, the period has not yet arrived when we may safely dogmatize on the subject. Our present object should be carefully to collect information, the interpretation of which may only be attempted with safety when it appears to flow directly and evidently from the evidence itself. The theories here referred to agree only in the two main points,—that cholera is induced by a special poison, and that this is of foreign extraction.

This, then, is one side of the medal: let us proceed to consider the reverse. The possibility of cholera being of indigenous growth has sometimes been hinted at, but scarcely ever been seriously entertained, although many arguments of much weight might be adduced in favour of the supposition. We are ourselves as little disposed to speculate on the subject of cholera, as to yield implicit obedience to the dogmatic assertions of others; it may, nevertheless, not be uninteresting, and it cannot retard the acquisition of a more correct knowledge of the causation of cholera, if we briefly state those facts which seem to point to an indigenous rather than a foreign origin of the disease. This investigation will resolve itself, first, into an inquiry whether cholera be indeed an imported disease, the production of a hot climate, which, assuming all at once a migratory character, has, in the present century, for the first time, extended itself over a large portion of the habitable world; after which we propose to consider the general and local circumstances recorded in connexion with each visitation, with the object of determining how far its appearance has depended upon season, situation, or the local conditions of the people and their habitations. The opinion that cholera has come to us from abroad pre-supposes that the disease, in its epidemic form, is very different from the Cholera Morbus described by Sydenham as annually prevalent in the autumn in his time, and which continues to recur about the same period in ordinary seasons. There is, however, ample proof that this sporadic disease had both been more prevalent and more violent in the summer anterior to the appearance of cholera in October, 1831. In the eighth volume of the 'Medical Gazette' are reports of cases by Sir M. Tierney, Mr. Hingeston, and Mr. Fielding, which unquestionably assimilated very nearly to, if they were not identical with, epidemic cholera. Certain it is that several of these cases, if they had occurred during an epidemic visitation, would have been unhesitatingly referred to the pestilential variety by the most competent judges. In a note to the communication of the latter gentleman, his father, Mr. George Fielding, of Hull, states that he had, during forty years, occasionally seen cases of English cholera "which exhibited considerable collapse, and were without feculent and almost colourless and inodorous excretions."* The appearance of cholera in Sunderland, in 1831, was preceded by an unusual prevalence of milder choleraic disease, which, according to Dr. Brown, passed by insensible gradations into the intenser form which produced so much alarm throughout the empire:

> "Early in the month of August, cholera appeared, and speedily became very prevalent. It raged in all degrees of intensity, from slight bilious attacks to cases attended with violent spasms, coldness, collapse, almost (if not complete) arrest of the circulation, white discharges, suppression of urine, and, in short, all the symptoms ascribed by observers to the Asiatic and Continental diseases. Of these more intense cases, several were fatal, some of them within twelve hours."†

Evidence of a like nature is furnished by Dr. T. M. Greenhow in his account of cholera as it had recently appeared in the towns of Newcastle and Gateshead. He states that the first case of cholera which took place in that part of the country occurred on the 4th of August, 1831, at a village called Team, and that other cases occurred at Newcastle simultaneously with, if not before, the regular appearance of the disease at Sunderland.* This direct evidence of the prevalence of disease of choleraic character anterior to the cholera epidemic of 1831, is much strengthened by an examination of the London bills of mortality. From these we learn that diarrhœa, summer cholera, and other diseases of a profluvious character, had been unusually fatal in several years of the present century—as, for example, in 1802 and 1803, and again in 1811, 1814, 1815, and 1823. The most striking circumstance on this head, however, is, that the mortality from the conjoined diseases of diarrhœa, dysentery, and flux, all of them congeners of cholera, suddenly rose in 1827, and progressively increased until 1831, in which year, in addition to an unusual number of deaths from diarrhœa and dysentery, 48 are recorded from cholera, although, according to the official documents, this disease did not reach the metropolis in its epidemic form until the month of February, 1832. It is true that the numbers recorded in each year are small in comparison with those we are now accustomed to, but the system of registration was at that period most imperfect, so that in all probability a large number of deaths were not recorded at all; moreover, the population of the metropolis in 1831 was much less than it has since become. A straw may nevertheless serve to show the direction of the current; and as diseases of the flux kind are precisely those which the unskilled are least likely to mistake, the fact here recorded would seem to indicate at least a disposition to analogous maladies, on the part of the inhabitants of London, anterior to the advent of the more formidable disease: at any rate, proof is thereby afforded that the mine was ready charged for explosion, and that, even if the spark which excited the epidemic outburst came from without, it found in the existing conditions of the population of Britain a congeniality for its reception. After the subsidence of the epidemic, the mortality from diseases of the alvine flux character never entirely receded within its former limits. In the year 1846, a marked increase occurred in the number of deaths from this class of complaints; the gross amount from each of the three diseases—summer cholera, diarrhœa, and dysentery—being considerably more than double that of any year since 1834. In 1847, the year preceding the second great visitation of cholera, the mortality from the same causes, although somewhat less than that in 1846, was still more than double the average of former years. In 1848, the deaths from cholera in London amounted to 652, of which 181 took place previous to the appearance of the epidemic disease in October. Some of these cases are said to have been nearly allied to the Asiatic form, to which they would certainly have been referred had the presence of epidemic cholera been at that time recognised in the metropolis.† A very careful inquiry into the history of the earliest cases of cholera which occurred in London, in the autumn of 1848, was made for the General Board of Health by Dr. Parkes, who reports, as the result of his scrutiny, that the poison could not have been brought by the clothes or baggage of any persons coming from infected districts in England; neither was there any evidence of the arrival of persons from the Continent,—from Hamburgh, or from Dantzic,—into the localities wherein the earliest cases occurred. Cholera and diarrhœa never retired within their former limits after the year 1849, but continued to

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* Medical Gazette, vol. viii. p. 817.

† Letter from Dr. Brown, of Sunderland, to Drs. Johnson and Tweedie: Medico-Chirurgical Review, Jan. 1831.

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[*] T. M. Greenhow on Cholera in Newcastle and Gateshead. London, 1832. pp. 88, 126.

[†] Registrar-General's Report on Cholera, p. 12; also British and Foreign Medico-Chirurgical Review, vol. iv. p. 257.

occasion a very considerable annual mortality until the outbreak of the third great epidemic in 1853. The mortality of the summer quarter of 1853 was considerably below the average, a depression in which, with the exception of a few districts of Northumberland and Durham, nearly every county and district of England participated. "A similar depression of the mortality was observed in the summer quarter of 1848, immediately before the outbreak of the epidemic cholera;"* and was also noticed in Moscow during the summer of 1830, prior to the outbreak of cholera there in September.

The earliest cases of the epidemic took place in August; its appearance in London and in Newcastle, and other northern towns, being almost simultaneous. Before the end of the year, the pestilence appeared in Liverpool, Plymouth, Redruth, and other places remote from the scene of its first great explosion. A painstaking investigation into the history of the first cases in the vicinity of Newcastle, by Dr. Robinson, of that town, seems to show that the disease was not introduced by direct human intercourse; a result confirmed by the commissioners appointed to inquire into the outbreak, who state in their report that—

> "There is no evidence whatever to show that the late outbreak in Newcastle was in any degree owing to the arrival there of any infected ships, sailors, or other persons, from any already-infected localities." (p. 25.)

Taken altogether, this evidence is opposed to the common opinion, that cholera is somehow directly imported by means of human intercourse. The isolated outbreaks which have occasionally happened at seasons when cholera was not generally epidemic, all tend in the same direction. Thus

> "The House of Industry at Coventry suffered, in the beginning of 1838, a sudden and severe outbreak of cholera, when 55 of the inmates—27 males and 28 females—perished by the epidemic. The first death occurred on the 7th of January, and on that and the four following days 7 cases were fatal; from the 12th to the 16th inclusive, 4 persons died; between the 17th and 21st, 15; between the 22nd and 26th, 20; between the 27th and 31st, 7; and on the first three days of February, 2. 18 of the fatal cases occurred under forty years of age, and 8 between that and sixty; but the greatest mortality was between sixty and eighty years, when 20 patients sunk under the epidemic; the remaining 9 died at upwards of eighty years.
>
> "Fortunately, the disease was confined to the house, and did not extend its visit to the town."†

There had been no case of cholera in Tynemouth, nor in any other place within a considerable distance, for several months, when, in the last week of March, 1849, four deaths occurred in a notoriously unhealthy court of that town, three of them in the same house. The survivors were removed, the house was cleansed, the pestilence ceased, and did not again raise its head in any part of the borough until the following month of July.

The foregoing facts certainly seem to show that the population of this country has been undergoing a morbid change, as regards the tendency to diseases of the flux character, during the second quarter of the present century. They are nevertheless not altogether incompatible either with the introduction theory, or with the opinion expressed by Dr. Farr, "that while the materials were smouldering in England, the flame which threw the mass into combustion has been of Asiatic origin." However probable this might otherwise be, there are not wanting facts which would seem to show that, under a different name, cholera was one of the most fatal epidemics by which the population of London was formerly afflicted. We must here again revert to the evidence on this head furnished by the bills of mortality. An examination of these shows that there was a decided increase of dysenteric and diarrhœal disease for several years anterior to the Great Plague of 1665. In that disastrous year, over and above the mortality from plague, 1600 deaths are recorded from the diseases classed in the bills of mortality under the heads of "griping in the guts," "bloody flux," and "colick," all of which we are justified in referring to the general head of alvine flux by the example of Dr. W. Heberden, jun., whose investigations into the nature of the diseases recorded in the bills of mortality render him the first authority on this subject.

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* Registrar-General's Quarterly Return of Marriages, Births, and Deaths, No. 19.

† Registrar-General's Report on Cholera, p. 79.

It is not unimportant to observe here, that bloody flux, or true dysentery, is always distinguished in the bills from flux and "griping of the guts," showing that a correct discrimination was exercised in regard to the two complaints. We learn from Willis the real character of the disease classed under the head of griping of the guts, which no one who has read it will hesitate to consider, if not identical with cholera, at least closely allied to it:

> "Although," says he, "the word dysentery, in the common acceptation thereof, signifies a bloody flux, yet, saving the etymology, I shall apply that name to this London disease even when it is not at all bloody; for I have often, and a great while since, observed that there are two very different sorts of this same flux, which almost every year is wont to be so rife here about autumn, and is commonly called in our language the *griping of the guts;* in the one whereof the stools were watery, and, as it were, limpid (or clear), with a sudden weakening of the body; in the other they are bloody, but tolerable."
>
> "In the year 1670, about the autumnal equinox, a great many were sick of an unbloody but a very sharp and dangerous dysentery. The distemper came upon them on a sudden, and oftentimes without any manifest cause, and reduced the patients—by grievous vomiting, frequent stools, and these watery ones—in a short time to very great weakness, horrid faintings of their spirits, and destruction of their strength. I know a great many that, though the day before they were well enough and very hearty, yet within twelve hours were so miserably cast down by the tyranny of this disease that they seemed ready to expire, in that their pulse was weak and slender, a cold sweat came upon them, and their breath was short and gasping; and indeed many of them . . . . died quickly of it. This distemper raged for a whole month, but began to decrease about the middle of October, and before the first of November was almost quite gone. Few at that time had bloody stools, and not many bilious ones, but a great many had both vomitings and evacuations that were waterish, almost limpid, and in great quantity."[*]

It is remarkable that Sydenham, who gives an account of the dysentery which prevailed in London from 1669 to 1672, makes no direct allusion to this disease so graphically described by Willis. Probably it may be, as Morgagni has surmised, that in so large and populous a city as London they met with a different phase of disease, consequent upon the different conditions of the atmosphere, and the variety in the occupations, habits, and residences of those affected;[†] an opinion which the concluding remarks of Willis seem to justify, for, says he, "Whilst this common dysentery raged so severely within this city, there was scarce any one sick of it in the country, or at least above three miles off." Another point worthy of notice in reference to this London disease, is, that Sydenham says the dysentery of 1669—70 was preceded by diarrhœa, as if, "at that particular time, the atmospheric constitution was inclining towards the subsequent dysentery."*

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[*] Willis, Pharmaceutice Rationalis, § iii. cap. 3; also a translation by S. Pordage, p. 51. 1684.

[†] Alexander's Morgagni, book iii. letter 31, article 2. London, 1769.

Morton also speaks of epidemic diarrhœas and dysenteries, accompanied by "awful twitching cramps," as prevailing annually from 1666 to 1672 in the autumnal months (August, September, and October), to such an extent as to occasion a weekly mortality of from three to five hundred. The diarrhœa consisted of a copious purging of colliquative and virulent serum.†

It will not fail to be observed that, with the exception of Morton, whose account is much less like cholera than that of Willis, no mention is made of cramp or of the blue skin so frequently observed in severe cases of malignant cholera. These are, however, neither essential nor universal symptoms of cholera; and Bontius, in his account of the cholera in Batavia, written in 1629, and which is usually considered as having been identical with the so-called Asiatic cholera of more recent times, mentions neither spasms nor lividity. Sonnerat,‡ who gives a brief account of cholera in his 'Travels,' whilst he mentions the more prominent symptoms of watery flux, vomiting, extreme faintness, oppression of the chest, and suppression of urine, takes no notice of the cramps and lividity of the skin. Curtis, whose description of cholera is otherwise most excellent, whilst he speaks of a livid circle around the sunken eyes, and of the livid, incurvated condition of the finger nails, describes the countenance as pale, wan, and dejected. No doubt cholera, like every other epidemic disease, varies in its type, as it certainly does in severity; for if we consider it, and the diarrhœa so prevalent during an epidemic, as arising from the same cause, and being, in fact, the same disease in a different degree of intensity, there is as much variety in the aspect and symptoms of cholera as of scarlet fever, between the malignant cases of which and the extremely mild ones frequently encountered, there is so vast a difference as would infallibly mislead an unskilful or unobservant practitioner as to their identity.

Neither was this London disease confined to Britain, for the dysentery of Nimeguen, in 1736, described by Degner, in many respects resembled the cholera of modern times, and especially in the suppression of urine, the vomiting, failing, and sometimes entire deficiency of pulse, coldness of extremities, prostration of strength, and amount of purging. "One," says he, "can scarcely either conceive or believe the great amount of liquid humours passed in this disease. Almost the whole body is dissolved into liquid, and purged away by the intestines."§ Morgagni alludes to these watery dysenteries, or, as he elsewhere calls them, diarrhœas, and describes an attack which he himself suffered in 1733, in which, within twelve hours, he discharged at least sixteen pints of almost

* Greenhill's translation, vol. i. p. 122; published by the Sydenham Society.

† Morton, Pyretologia, appendix, p. 421. Londini, 1692.

‡ Sonnerat, Voyage aux Indes Orientales et à la Chine, fait par ordre de Louis XVI. Vol. i., pp. 201—4.

§ Deynerus, Historia Medica de Dysenteria, etc., pp. 5, 14, 17, 18. 1738.

limpid water.[*] "The pains were slight: the stools not very frequent, but very large."

To return, however, to London. An examination of the bills of mortality shows a great increase of diseases of the flux family after the Great Plague of 1665, as if the conditions favourable to the development of glandular plague having ceased to exist, the pestilential elements still remaining among the people of London, found vent in a disease of different form. This mortality is chiefly assigned to the disease styled in the bills "griping in the guts," sometimes also called "the plague in the guts," between which, bloody flux, flux, and colick, a distinction is uniformly maintained. In the year immediately succeeding to that of the fire—by which event the population within the bills of mortality must have been greatly reduced—the number of deaths from this one form of flux exceeded two thousand. Below this it never fell until near the close of the century, although in some years it exceeded three thousand, and in one or two, four thousand. It must be remembered that the population of London at that date but little exceeded a fifth of the present amount. It was computed by King[†] at 530,000 in 1685, and is believed to have been very stationary, the loss by deaths, which greatly exceeded the births, being barely compensated by immigration from the country. Thus the comparative mortality from this single disease in ordinary seasons, equalled that occasioned by the cholera epidemic of 1854.

From the beginning of the eighteenth century the mortality from the several forms of alvine flux began to fall, and keeping pace with the improved habits and social position of the people, gradually all but ceased before its close. Dr. W. Heberden states, and we have verified his account, that the deaths from flux—which had, as we have seen, for many years annually exceeded two thousand—amounted to one thousand and upwards in the early part of the eighteenth century, had decreased to one hundred and fifty by the middle of the century, and at the close, to twenty.

From the foregoing facts it seems not improbable that the modern cholera is but the re-appearance of pestilential disease amongst us in a form familiar to our ancestors: the appearance, under a new and more formidable aspect, of a malady which has been known as a yearly autumnal visitor since the days of Sydenham. If so, the gradual cessation of this formidable disease during the last century, and the gradual augmentation of the mortality from analogous diseases for a few years antecedent to the first visitation of cholera, and, still more strikingly, both anterior and subsequent to that of 1849, would indicate corresponding variations in the sanitary condition or social habits of the people of England. It may be that the introduction of a leaven from abroad was necessary to call into active operation the slumbering elements of evil. It may be that a true zymosis has been excited in the vitiated atmosphere of our towns by the arrival here, wave after wave, of a ferment generated in the damp jungles and foul hovels of Hindostan. But it may be that the evil is of indigenous growth, and that the poison of cholera rather consists of some peculiar chemical condition of home-bred elements, called into being by the gradual growth of new evils consequent upon the vast comparative extension of our town populations during this century. At present, neither supposition can be entirely and exclusively adopted without hesitation. The time has not even arrived when we can safely attempt the solution of the question. Let both aspects of the subject, therefore, be kept in remembrance; and so let us proceed to the study of any future epidemic visitation of cholera, should such unhappily occur.

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[*] Loc. cit. p. 71, article 9.

[†] King's Natural and Political Observations. 1696. See also the third chapter of Macaulay's History of England.

As regards the view of the case here set forth, it may not improperly be objected that the local circumstances and general condition of the people varies comparatively but little from year to year, and that the insalubrious influences to which they are exposed are tolerably persistent. This is no doubt true, and we are bound to admit, and indeed our object is to show, that some other and occasional circumstance is required to give energy to them. This may consist in some climatic or seasonal condition, such as may have been meant by the term "Constitution of the Year," as used by the older physicians. Temperature would seem to have a large influence, for it is significant to observe how often the prevalence of alvine flux has varied with the fluctuations of the thermometer, both in the same and a succession of seasons. Applying this test to some of the years upon which we have already remarked, we learn that the average temperature of 1846, in which the mortality occasioned by diarrhœa, cholera, and dysentery, was very large, was four degrees higher than that of 1845, and three degrees above the average of the six preceding years. The mortality from these diseases in London, which had been 865 in 1845, reached 2536 in the hotter year. The year 1847, although still warmer than the average, was inferior to that of 1846. This fall of average temperature was accompanied by a corresponding fall of mortality from these diseases, although dysentery was provalent, and diarrhœa fatal. But it may again be objected, that seasons have always varied, and that there were many hot years during the century that England was free from alvine flux of pestilential character. True again; but the remark only proves that a hot season is no more sufficient to induce the outbreak of a pestilential epidemic than the local circumstances of the people were found to be. In other words, the local causes of insalubrity which have grown up amidst and around us, require the combined influence of a certain atmospheric condition to produce pestilence. This brings us to the second division of our subject, which we shall consider under the two heads of "seasonal or meteorological conditions," and "localizing causes."

Ample materials for this investigation are furnished by the very valuable and interesting reports of the Committee for Scientific Inquiries, appointed by the Medical Council of the General Board of Health in 1854, and by Dr. Barton's most elaborate report 'On the Sanitary Condition of New Orleans,' which occupies 250 pages of the report of the sanitary commission appointed to inquire into the recent fatal visitation of yellow fever to that city. Dr. Barton's report, which is unique, comprises the result of many years' careful observation and inquiry as a voluntary labourer in the field of sanitary investigation. It is well worthy of the highest commendation; and if duly appreciated by the authorities of New Orleans, will be the means of inaugurating a system of medical inquiry and hygienic supervision in that city, notoriously one of the most pestilential in the United States, which cannot but eventuate in immense public benefit. Dr. Barton seems of opinion that epidemic diseases are usually the production of the locality in which they appear; and we presume he would assert that cholera is of indigenous origin, requiring only a certain season and certain local conditions for its development. Speaking of the cholera of 1853, he states that all the atmospheric conditions for its development existed, but that—

> "New Orleans was in no condition to localize it. . . . . Whilst an epidemic state of the atmosphere exists over the whole country, the disease will only be developed where there exists also, in more or less intensity, the localizing conditions of filth, moisture, stagnant air, &c."

Referring to the two causes essential, according to his experience, for the production of epidemic pestilence, which he somewhat quaintly calls the "shears of fate," he distinguishes the terrene or localizing conditions as one blade of the shears, and the atmospheric or meteorological as the other. As regards the latter element, he finds that cholera has always existed in New Orleans with an east or south-east wind; a temperature above 70°, increased as the disease attained its maximum; a dew point of from 60° to 70°, and a barometric elevation of over 30°. The year 1853, in which cholera appeared—but, owing to the removal of localizing causes consequent upon the attempt to cleanse the city during the long previous scourge of yellow fever, did not reach any great height—was eminently distinguished by the unusual prevalence of calms or a stagnant state of the atmosphere. With the decline of the epidemic, the climatic states to which, in conjunction with local conditions, he refers it, also changed. The temperature fell, and the wind shifted. "The maximum barometer," he says, "occurred on November 18th, and was 30·46°, a very unusual height here; soon after which the cholera broke out." During December, the wind continued from the east, north, and north-east; the maximum barometer 30·48° on the 2nd, when the cholera was at its height, and declined to its minimum, 29·57°, on the 30th. The cholera ceased soon after the middle of the month. The average temperature fell from 65° in November to 53° in December. The subsidence of the cholera epidemic in 1832 was also most rapid, consequent upon a sudden fall of temperature, with north winds. This visitation, which was one of the most fatal on record, carried off 4340 persons, being at the rate of 78·78 per 1000 of the population. Like the much milder epidemic of 1853, it followed immediately in the train of a disastrous attack of yellow fever, which entirely disappeared a few days after the outbreak of the cholera which supplanted it. Dr. Barton attributes yellow fever and cholera to very similar terrene conditions, but to different atmospheric states; and says the co-existence of the two diseases is incompatible, and that they have never existed together as epidemics in New Orleans. In the year 1833, in which was the next most fatal visitation of cholera, the mortality occasioned by it exceeding 17 per 1000, there was—

> "A great fall in the thermometer on the 8th of June (and of course the hygrometry); a heavy fall of rain on the 9th (over five inches), and severe thunder and lightning; a change of wind from south-east, which had predominated to the western quarter, and the disease gradually declined. It reached its acme on the 8th, and terminated about the 25th."

The first epidemic of cholera in this country, during the present century, began in the north of England, in October and November, 1831. The preceding summer was unusually fine, the harvest early, and the difference between the northern and southern counties of England, as to season and richness of vegetation, much less marked than usual. Mr. Losh, of Jesmond, near Newcastle-upon-Tyne, states that October was fine, and November milder than usual: "The nights have been warmer, in proportion, than the days; and though November is always a gloomy month, it has this year been more subject to a hazy state of the atmosphere than usual."* December, also, was much warmer, and the atmosphere singularly stagnant: "there was always a haze in the air." The month of January, 1832, was of unexampled warmth and dryness; and it was remarked by seamen and others accustomed to observe the weather, that there had been no brisk gale for above four months. Cholera began in Newcastle, early in December, 1831, with a south-east wind. The atmosphere of Newcastle and Gateshead during the epidemic outbreak in September and October, 1853, is described by Dr. Charlton and other witnesses, before the Cholera Inquiry Commissioners, as having been unusually still, stagnant, close, and hot. It was impossible to ventilate even large houses, in which no change of air "seemed to take place for almost a week together." The atmosphere was dark, and the temperature high.†

In Mr. Glaisher's report 'Upon the Meteorology of London in relation to the Cholera Epidemic of 1853-4,' printed in the appendix to the report of the Medical Council, he gives, as far as practicable, a comparative view of the meteorology of London during the three chief epidemics of 1832, 1849, and 1854. The report, to which we must refer for details, will well repay a careful perusal, and is advantageously read in conjunction with Dr. Barton's, to which we have already adverted. Mr. Glaisher, at the conclusion of his very minute and elaborate report, thus sums up the meteorological phenomena of the three visitations:

> "In the year 1832, the barometer reading was high, that of the thermometer low;‡ and rain was deficient one-fourth of its average in the year. In the summer, when the disease was raging for the first time in England, the barometer was high; the temperature below the average; the quantity of rain small; the direction of the wind north-east and south-west; the air not in much motion; the sky partially overcast; and there was a seeming deficiency of electricity.
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> "In the year 1849, the pressure of the atmosphere was great; the temperature high; the sky overcast; the direction of the wind north-east and south-west; the atmosphere misty and thick; the velocity of the air less than one-half its average.

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* Cholera in Newcastle, p. 91. By T. M. Greenhow. London, 1832.

† Since this article was in type, Mr. Thornhill, of the Newcastle Literary Society, has kindly favoured us with an abstract of the meteorological observations kept at that Institution for the autumnal quarter of 1853. The point most worthy of note is, that the barometer rose just as the disease reached its climax, and after continuing above 30° during the five most fatal days, gradually fell from the 20th to the 24th of September. The daily mortality declined from 111 on September 19th to 85 on the 20th, and had decreased to 56 on the 24th. The month of August was exceedingly dry. There was light rain on five days between August 26th and September 2nd, but very little after the last of these days until the epidemic had almost ceased. The first deaths were on September 1st. The mere height of the thermometer was not such as to account for the extremely oppressive closeness of the atmosphere described by several of the medical witnesses, and to the correctness of which we can bear personal testimony. The temperature never exceeded 68°, and only passed 64° on four occasions during the continuance of the epidemic.

‡ Mr. Glaisher elsewhere shows that the diurnal range of temperature was small in every month.

When the epidemic was at its height a calm prevailed, with a misty, thick atmosphere at all places, which was sensibly more dense and torpid in low places; the weather was dull, thick, and oppressive; no rain; temperature of the Thames above 60°; weak positive electricity; no electrical disturbances.

"In the year 1854, the pressure of the atmosphere was great; the temperature generally high; sky overcast; direction of the wind north-east and south-west; and the velocity of the air was less by one-half than its average for some time before; and at the time of the greatest mortality from cholera, the barometer reading was remarkably high, and the temperature above its average; a thick atmosphere, though at times clear, everywhere prevailed; weak positive electricity; no rain. In low places, a dense mist and stagnant air, with a temperature in excess; temperature of the Thames water high; a high night London temperature; a small daily range; an absence of ozone, and no electricity.

"The three epidemics were attended with a particular state of atmosphere, characterized by a prevalent mist—thin in high places, dense in low. During the height of the epidemic, in all cases the reading of the barometer was remarkably high, and the atmosphere thick. In 1849 and 1854, the temperature was above its average; and a total absence of rain, and a stillness of air, amounting almost to calm, accompanied the progress of the disease on each occasion. In places near the river, the night temperatures were high, with small diurnal range; a dense torpid mist, and air charged with the many impurities arising from the exhalations of the river and adjoining marshes; a deficiency of electricity; and, as shown in 1854, a total absence of ozone, most probably destroyed by the decomposition of the organic matter with which the air in these situations is strongly charged.

"In 1849 and 1854, the first decline of the disease was marked by a decrease in the readings of the barometer, and in the temperature of air and water. The air, which previously for a long time had continued calm, was succeeded by a strong south-west wind, which soon dissipated the former stagnant and poisonous atmosphere. In both periods, at the end of September, the temperature of the Thames fell below 60°; but in 1854, the barometer again increased, the air became again stagnant, and the decline of the disease was considerably checked. It continued, however, gradually to subside, although the months of November and December were nearly as misty as that of September." (pp. 116, 117.)

The chief results deducible from Mr. Glaisher's observations are, that in cholera years the meteorological conditions are such as have a marked tendency to favour the chemical decomposition of organic substances; "and to render the season defective in those atmospheric changes" which by decomposing or dispersing into space the products of decomposition, "renew the purity of the air." These evils are much aggravated by, indeed in some respects are due to, the extent of river surface and of undrained marshes in and around London; the vaporous exhalations arising from which, detained to a considerable extent by the still atmosphere and the hills which bound the metropolis on two sides, hang like a veil over London, obscuring the sun's rays during the day-time, and retarding the radiation of heat at night. This is especially applicable to times when the temperature of the river being higher than that of the circumjacent air, the former, which can be viewed only as the main common-sewer of London, is converted into a seething, simmering cauldron of foul impurities, the emanations from which consist, not simply of watery vapour, but contain also the products of this unwholesome decomposition. This is just the reverse of what would occur if the river were freed from its vile contaminations, for the water would then absorb and carry off some of the atmospheric impurities necessarily incidental to the existence on its margin of a densely-peopled city.

> "The effect of temperature upon the Thames water, in tainting the surrounding air, is exhibited in the well-known fact that diarrhœa and summer cholera become prevalent after the temperature of the Thames has attained to 60°;"

as well as from the fact, "that as the water declines from this temperature, so also do the above diseases." In these facts, also, lies one of the causes why the population inhabiting the low alluvial lands near the margin of the Thames have in each epidemic suffered unduly from cholera. No doubt, a large share of the evil is due to the filth-saturated soil, the imperfect drainage, and, as we shall hereafter point out, the numerous local sources of atmospheric impurity; but these are all vastly aggravated by the greater stagnancy of the air, prevalence of haze, and excess of night temperature, with "small diurnal range," and the existence of an "air charged with the many impurities arising from the exhalations of the Thames."

No evidence of so precise and accurate a character as that furnished by Dr. Barton and Mr. Glaisher on the meteorology of cholera seasons, is procurable from any other source. Abundant matter corroborative of the same general facts and conclusions is, however, met with in Dr. Milroy's able and interesting report on cholera in Jamaica, and in several reports by medical officers of the Indian army. Cholera appeared in Jamaica in September, 1850; and although its main force was expended before the middle of the following year, it continued to linger in some of the inland districts until the commencement of 1852. During this period it destroyed upwards of a tenth part of the entire population of the island; the deaths being estimated by Dr. Milroy at between 40,000 and 50,000 at the least, and the population at about 400,000. Out of the 40,000 inhabitants of Kingston, about 5000 are reported to have succumbed to the pestilence. In smaller places the proportionate mortality was sometimes even larger. The little town of Port Maria, with its suburbs, Stennett's Town and Manning's Town, lost 553 persons out of a gross population of only 1000, of whom 200 are supposed to have fled immediately on the outbreak of the epidemic. In this unhappy island, then, cholera in its most virulent form prevailed with unexampled violence. It will be presently seen how very similar were the circumstances of the visitation to those which exist with the less violent manifestations of the same pestilence in our own more favoured climate. In reference to the meteorology antecedent to and during the epidemic, Dr. M'Illree, writing to Dr. Milroy from Newcastle barracks, says: "The weather at the commencement of the year was very dry;" "there was much heat in June, July, and August;" and "for several weeks previous to the appearance of cholera it was exceedingly warm, still, and disagreeable, even at Newcastle, which is nearly four thousand feet above the level of the sea." "The atmosphere was very oppressive, and surcharged with electricity." Mr. Taylor, of Good Hope, a plantation in the Port Royal mountains three thousand feet high, remarked, "that for several weeks not a breath of air was to be felt, nor a leaf seen to move, even at that elevation." Similar evidence is furnished from the north or opposite side of the island by the Hon. Mr. Roberts; from Port Royal by the military and naval medical staff; and by Drs. Clacher and Henderson, of Port Antonio. The weather in St. Elizabeth was intensely hot and oppressive for several weeks before the outbreak: "the atmosphere felt as if it was too thick to breathe." Dr. Reid, of the 2nd West India Regiment, stationed in Spanish Town during the period, says in his report:

> "I noticed that every day, for some time prior to the (epidemic) attack, about two o'clock the air became very sultry, and that immense masses of heavy-looking clouds, with very defined edges, hung over the town, and gave to every one a most unpleasant feeling of tightness across the chest. This, I think, continued for about a week before the epidemic broke out." (p. 118, note.)

The whole of the facts relative to the meteorology, so far as they came to his knowledge, are thus summed up by Dr. Milroy:

> "After a very dry spring, the early rains set in as usual in May. In ordinary seasons these last for two or three weeks, and then dry weather follows for some months, when the late or October rains are looked for. But in 1850, rainy weather occurred at frequent intervals throughout the whole summer. It was altogether a very wet season, not so much from occasional immense falls of rain, as from the constant recurrence of showers; and also, which is not common in tropical countries, from their frequently occurring during the night. The heat, too, was more than usually oppressive from July onwards, in consequence of the remarkably calm, stagnant state of the atmosphere. The sea-breezes—which are so refreshing within the tropics, setting in about nine or ten o'clock in the morning, and lasting till the afternoon—frequently failed, or were entirely absent for several days in succession. At other times they were irregular as to the direction from which they came. The regular sea-breeze in Jamaica is from the east, being south-east on the south side, and north-east on the north side of the island. Whenever it deviates much from its accustomed quarter, blowing more due north or south, it becomes much less refreshing, and this change is the more remarkable if it veers round at all to the westward. It is then, instead of being the 'doctor,' or health-bringer, very generally not only uninvigorating, but positively unwholesome.
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> "Now the change in question was continually occurring during the summer and autumn of 1850. . . . . The peculiarities adverted to were experienced, not in one part only of the island, but over its entire length and breadth; on the coast and in the interior; on high mountains, which are usually cool and healthy, as well as in the plains, where oppressive sickly weather is more common." (pp. 7—8.)

In his account of an outbreak of cholera in H.M. 86th Regiment at Kurrachee, in India, in June, 1846, Mr. Thom mentions that—

> "The climate of Kurrachee during the weeks preceding the appearance of cholera among the troops was characterized by several peculiarities different from those which generally belong to all hot countries and seasons, perhaps mainly so by their presence being in an excessive degree. First, the temperature was unusually high, being 90° to 92° in the day-time, and 86° at night in good houses; and in the tents of our soldiers it rose to 96°, 98°, and 104°, as indicated by a thermometer suspended on a central pole five feet from the ground, and in the thorough draught between the doors. Secondly, the quantity of moisture in the atmosphere was greater than I ever saw it before in any part of the world, or at any season, the dew point being at 83°, and the thermometer in the shade at 90°, the lowest range; even this gives 12·19 grains of vapour in each cubic foot of air. The mean heat in the twenty-four hours was such as to suspend an unusually large proportion of vapour in the air, always near, but rarely or never reaching the point of deposition. Even at the Equator, with the sun overhead, I never saw the point of deposition above 78°. The third, and perhaps most important, circumstance worthy of notice in connexion with the other two, was the light, weak, unsteady winds or calms which prevailed in the early part of June. Now this is exactly the reverse of what ordinarily happens. In the last two years, the months of June and July were remarkable for the strong, steady, and cool winds,

and overcast sky, which has given so favourable a character to the climate of Kurrachee during the hot months. It also appears that the quantity of rain which fell during the prevalence of cholera was much beyond anything that had occurred for a long time before; at least it surprised the European officers who have been here for three or four years. The state of the barometer I cannot give, but it must have been very low."*

The effect of this state of the weather upon the bodily feelings and functions is described as having been most overpowering and oppressive.

> "There was a sense of languor and oppression, a stifling feeling about the respiration, and inability to undergo the slightest fatigue without extreme exhaustion. . . . . In fact, for ten days before the predictions were unhappily fulfilled, it was a common remark among 'old hands' that it was regular 'cholera weather.' . . . . At this moment (14th October) the thermometer is as high as it was during the cholera, being 90° to 92° in houses, and 100° in tents, in the middle of the day; yet we feel fresh, elastic, and free from that horrible undefinable sense of oppression that prevailed in June. . . . . But we have a palpable cause of this agreeable change; the dew point is at 72° instead of 83°, and the evaporation is now, even in a calm, more rapid than it was in a fresh breeze in June."

From the meteorological table we learn that after some days of cloudy hot weather it became oppressive and calm, or with but little light wind, on June 11th. This state of things continuing, after six admissions for cholera on the preceding days, there were 47 on the 15th, and 316 in the six following days, 209 of which were on the 16th and 17th. On the afternoon of the 17th the wind, which had previously been south-west, veered to the north-west, there was a fresh breeze, and the pestilence began to decline. On the 21st there was heavy rain, with squalls at night, and the admissions, which had on that day been 20, fell on the following day, in the morning of which there was a gale. The disease, in fact, ceased as an epidemic, the subsequent cases being straggling ones. Out of 1091 rank and file, 410 were attacked by, and 238 died of, the disease between June 11th and July 20th, being at the frightful rate of upwards of 218 in the 1000. This is exclusive of the other troops in garrison, natives as well as Europeans, and of officers, women, and children.

Although, according to Mr. Scot, cholera has been doubly more frequent in India in dry than in wet weather, and usually begins to manifest itself in April and May, at the setting in of the hot season, many instances are recorded of its having followed rain, and a change from an exalted to a lower state of the thermometer. It is not difficult to reconcile these facts with its more usual prevalence in dry weather; for in situations in tropical countries naturally dry, there is in very hot and dry weather an absence of the degree of moisture indispensable for its production, which a fall of rain supplies, but of which there is always enough without rain in moister climates, like England, and in marshy districts, or by the margins of rivers. This is in accordance with the history of that other pestilence, plague, between the history of which and that of cholera there are so many striking analogies. The Arabian physicians assert that pestilences are brought by unseasonable moistures, heats, and the absence of atmospherical movement. In Ethiopia it is particularly observed that rain during the sultry heats of July and August is usually followed by plague. On the other hand, plague often ceases with a rise of temperature, provided there be an absence of moisture.[*] Cholera often prevailed in India with an east and north-east wind. At Arcot it appeared on the day on which the wind changed from south-west to the north-east, or monsoon quarter, and considerably abated on the occurrence of a strong south-west gale with rain.[†] At Vizagapatam it appeared about the 13th of May. The weather was oppressively hot, and the air loaded with humidity. Cholera broke out in its most aggravated form in the 2nd Madras European Light Infantry, at Arnee, on May 23rd, 1840. The weather had previously been intensely hot, the thermometer during the month having varied from 80° to 93°. At two P.M. on May 22nd, the day preceding the outbreak, a heavy squall of wind, with rain, occurred, reducing the temperature from 91° to 87° in the house, and to 81° outside. Twenty patients were admitted into hospital between the evening of the 23rd and the following morning, and the disease continued to prevail with more or less violence until the end of the month. After very sultry weather, the station was refreshed with heavy rain on the evening of the 31st and June 1st, when cholera disappeared as suddenly as it arose.[‡] "Cholera was not prevailing in the neighbourhood, and no one was supposed to have arrived amongst the troops from an infected place." Another of the Indian medical officers reports, that at the time of and anterior to an outbreak of cholera "there was a dead stillness in the atmosphere, not a twig nor a blade of grass moved, and many complained of a suffocating sensation. On the 18th, between two and nine A.M., 31 cases of cholera were admitted," and 11 more during the day, making in all 42 out of a strength of 617.[§] Dr. French, of H.M. 49th Regiment, in reporting upon an outbreak of cholera in that regiment in February, 1835, after stating that on the occurrence of a high wind, accompanied by a fall of temperature, the disease abated, and the cases became more controllable, adds—"It has frequently come under my observation in India, that violent storms of wind and rain have for a time either entirely arrested or greatly mitigated all the symptoms of the disease."[‖] Bellary, a military station in India, is notorious for the liability of the troops stationed there to cholera. Between 1818 and 1839, the year 1819 alone shows no mortality from this cause. The average strength of the European troops has been 654, and the average admission of cases of cholera into hospital 39, being at the rate of about 6 per cent., of which nearly a third have proved fatal. The climate of Bellary is characterized by an intense heat, a cloudless sky, great glare, with strong gusty hot winds during the day, and considerable reduction of temperature at night. Very little rain falls in that part of India. The rock of Bellary is of granite, five hundred feet high, and the soil immediately around is formed of the granitic débris. On at least one occasion of an epidemic outbreak at Bellary, of which a record has been preserved, this was preceded by light showers of rain following great heat.*

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* Medical Report on the Causes, Character, and Treatment of Spasmodic Cholera in H.M. 86th Regiment at Kurrachee, in June, 1846, pp. 13, 14. (Parliamentary Paper.)

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[*] Mead on Plague: Mead's Medical Works, pp. 247, 259. London, 1762. 4to.

[†] Report on Epidemic Cholera, p. 9. By W. Scot.

[‡] Reports on Asiatic Cholera in Regiments in the Madras Army from 1828 to 1844, pp. 10, 134. By J. Rogers, F.R.C.S. London 1848.

[§] Loc. cit., p. 205.

[‖] Loc. cit., p. 115.

The cloudy sky so frequently observed to prevail during cholera visitations in this climate is not without its parallel in India. Mr. Scot thus describes it:

> "Amongst the atmospheric phenomena supposed to be connected with the appearance of cholera in India, it may be interesting to mention a certain aspect of the sky which proved too often to be a harbinger of an outbreak. This was a dull leaden-coloured suffusion, obscuring the sun, yet totally without any distinct form of cloud—an ominous canopy, without motion, and attended with a certain chilly feel in the air. If this continued for several days, we were certain to hear of cholera; and the disease would cease on this appearance of the sky breaking up, especially if ending in a storm."†

The report on the cholera in the Black Sea fleet is a very interesting document, and well illustrates the effect of a close, still atmosphere, and of defective ventilation, whether this arise from the structure of streets and houses, or from the position of a ship, in favouring the disease. In several vessels the mortality was considerable; it was pre-eminently so on board the *Britannia*, *Albion*, and *Trafalgar*. Cholera broke out simultaneously among the crews of these vessels on August 9th, when moored off Baljick. The preceding days "were oppressively hot, and a dead calm generally prevailed throughout the bay." The thermometer on board the *Trafalgar* averaged 75°, the barometer 29° 87′, from the 1st to the 8th of August. On the evening of the latter day, the wind, which had previously been south, and partly off land, shifted, and blew a very hot blast from the shore, over the encampment lately occupied by the French. Upwards of one-half the crew of the *Britannia* suffered either from diarrhœa or cholera between August 9th and 27th, of whom 139 died. The ship quitted Baljick on the morning of the 12th. As she drew out of the bay, she got the advantage of the prevailing north-east breeze, and on that day and the following the patients did well. On the night of the 13th, the wind freshened, rendering the closure of the lower-deck ports necessary, and the only ventilation between decks was by the "imperfect means" of wind-sails. The great outbreak commenced about ten A.M. on the following morning, and "for the suddenness of its advent, the tempest-violence with which it raged, and the wreck it left behind," was almost unprecedented. The deaths on the 14th and four following days were—13, 45, 21, 14, and 14. On the 17th, the ship returned to port, and the greater part of the crew, sick and healthy, were transferred into empty transports, from which time the disease rapidly abated. It is perhaps not unworthy of note, that some of the officers and seamen have since spoken of a peculiar dense cloud which passed over this ship and the *Albion* on the 13th.

As the result of our inquiry into the meteorological circumstances that precede or accompany outbreaks of cholera, it would appear that whilst this pestilence may prevail within a considerable range of temperature, a moderately elevated one is most suitable for its development; and this accompanied by a still, stagnant condition of the atmosphere, and a moderate amount of moisture.

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* Reports on Asiatic Cholera, p. 14. By J. Rogers, F.R.C.S.

† Report on Epidemic Cholera as it has appeared in the Presidency of Fort St. George, p. xviii. By W. Scot. 1849.

It may be objected to this view, that cholera prevailed during the winter months of 1831 in the north of England, and at Moscow during the winter of 1830-1. Mr. Losh's 'Meteorological Journal,' already quoted, proves the winter of 1831 to have been of exceptional character in the neighbourhood of Newcastle where the pestilence first appeared. The summer of 1830 was oppressively hot in Russia, and cholera appeared in Moscow in September, whilst the city was still under the influence of the preceding heat. Its continuance during the cold of winter is probably more apparent than real, for the internal atmosphere of Russian houses is maintained at a high elevation during the winter months by means of stoves.

The precise effect of the comparative absence of ozone and of electrical disturbances in this country during an epidemic is at present unknown, as well as that of the high state of the barometer. It is certain that thunder storms have in this climate usually been succeeded by at least a temporary lull in the ravages of cholera; but, on the other hand, this disease is said to have been often heralded in tropical countries by disturbance of the atmospheric electricity. This was certainly the case at Quebec, towards the close of the epidemic there in the summer of 1832, but whilst the mortality continued large. Although the weather was hot and dry, there was an abundance of lightning every evening for ten days together. The frequent occurrence of mist and of a cloudy sky during a choleraic visitation is remarkable, especially as it has been observed elsewhere as well as in London. Mr. Glaisher, indeed, says "he can by sight estimate certain differences of mist, which he identifies with corresponding differences of epidemic sanitary condition; that he can connect one tint of mist with the prevalence of cholera, another with the prevalence of influenza; yet that, except for this rude test of colour, he cannot discriminate those mists, and has no hygrometric or other meteorological knowledge of their existence."

It is not unworthy of note here, how often mist has been observed to accompany pestilence. The first outbreak of sweating sickness in England was ushered in by a damp, misty atmosphere; and a similar history attaches to most of the subsequent visitations. The last epidemic of this disease, which broke out in April, 1551, was, according to Caius, preceded by impenetrable fogs of bad odour, arising from the banks of the Severn, from whence a true impestation of the atmosphere was diffused in every direction, so that whithersoever the winds wafted the stinking mist, the inhabitants were attacked by the pestilence.* Something not unlike the meteorological conditions observed in years of cholera outbreak was observed in the last plague year, 1665. J. Bell, in his 'London Remembrancer,' thus expresses his opinion relative to the influence of the season, and thus indirectly tells us the kind of weather prevalent during that momentous year:

> "And I conceive that the contagion of the air doth arise from the unseasonableness of the weather; for the weather hath been very seldom since the begin-

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* Hecker's Epidemics of the Middle Ages, translated by Dr. Babington for the Sydenham Society, pp. 290, 291, et seq.

ning of the plague suitable to the season of the year; but the air hath been close and obnumbulated, insomuch that the sun hath not had power to do its office, which is to exhale all fogs and malign vapours."*

There is one result of Mr. Glaisher's investigations to which we have scarcely alluded, but which well shows the importance of making careful meteorological observations in more than one locality of each district. It appears that during the autumn of 1854 a very great diversity of temperature was found to exist between the outlying and central stations ot London; and this especially in regard to the daily range, which is much less in the city itself than the suburbs; much less near the river's margin than in more distant or more elevated localities. The excess of night temperature affected the weekly mean between particular stations to 7°, 8°, 9°, and 10°; and even for a brief period, to 15° and 20°. With the whole of these facts before us, we feel that Mr. Glaisher is fully justified in asserting that "were the meteorology of our towns carefully ascertained, and collated with that of the metropolis, and both together with that of the country generally," we should soon "be in a condition to elucidate a clear insight into the meteorological causes of cholera, influenza, and many phases of disease which now burst upon us with the suddenness and devastating power of a divine and wrathful visitation."

It would thus seem that there was much justice in the opinion of the older physicians relative to the influence exercised by what they termed the constitution of the year in the causation of epidemic diseases. Perhaps the climatic conditions which have been so commonly found associated with cholera epidemics, and which, so far as we are able to gather from the meagre records furnished by contemporary authorities, were likewise associated with the several pestilences now extinct in this country, might justly be termed the pestilential constitution. Further observation is required before we can safely assign to this constitution its due share in the production of pestilence, but we are almost justified in asserting that the existence, in more or less intensity, of these seasonal and meteorological conditions is necessary for the development of pestilence in this climate, whether in the form of cholera or of plague, although the co-existence of at least one other factor is likewise necessary. In other words, a certain distemperature of season favours, if it is not necessary for, the production of pestilence, the precise character of which will depend upon the existence of some social or local condition, without the co-operation of which an epidemic pestilence cannot arise. Many circumstances seem to strengthen this supposition. Amongst others, the unusual amount of mortality in London from the several kinds of alvine flux in the year of the last plague at Marseilles, which very considerably exceeded the average of the seven preceding and the seven following years; as though the same character of season which had given strength to the plague at Marseilles, exhibited itself here in a less deadly form by the increase of the only disease approximating to a pestilence then existing in this country. The absence of plague from Britain since the great plague of 1665, and its disappearance from Egypt for upwards of one thousand one hundred years during the Persian, Grecian, and Roman occupation, together with its re-appearance in the latter country at a subsequent period, both point to the existence of some local conditions as necessary for its production. The many analogies between the history of plague and cholera also tend to show that they are in some respects governed by similar laws, although the proximate cause of each must be different. Both have prevailed chiefly in the same season of the year, and in similar localities. The greatest prevalence and mortality of both has usually been in September, and it has commonly happened with both that each visitation has extended over two seasons. The same districts of London in which plague was most destructive, are those in which cholera has been most fatal. A still more striking analogy was the occurrence of sporadic cases of plague almost annually, just as in our time is the case with cholera; so that, although there were then, as now, many years in which the number of deaths from pestilence, being inconsiderable, were not set down as epidemic seasons, yet scarcely one passed over in which at least a few scattered deaths from plague in the hotter months are not recorded.*

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* London's Remembrancer; or, a true Account of every particular week's Christenings and Mortality in all the years of Pestilence, &c. By John Bell, Clerk to the Company of Parish Clerks. 1665.

That meteorological conditions have a great influence over the development and spread of cholera, no one who has followed us throughout this investigation will hesitate to admit. How important is it, then, that the inquiries set on foot in 1854, and then only when the pestilence had attained its acme, should be systematically continued;—that the climatic phenomena of different towns be compared, a careful register of disease, as well as of mortality, be instituted, and a comparison of the meteorological phenomena of districts and towns visited by the same classes of disease be made;—lastly, that the atmospheric, electrical, and thermometric phenomena of those parts of towns which are found to be notoriously insalubrious be placed in juxtaposition with those of the suburbs and healthier portions of the same. Care should be especially bestowed upon an investigation into the presence or absence of ozone, our knowledge of which has not extended in proportion to the valuable results which, from the little we know, may be expected to flow from an increased acquaintance with its properties and effects.

Hitherto meteorological inquiries have been made almost exclusively with a view to the discovery of the laws which regulate the weather and climatic character of seasons. The results obtained from the limited and partial inquiries of last year in the direction of meteorology as applied to the investigation of epidemic disease, are so important; the promises of still more valuable information as regards the causation of disease held out by meteorology, if these inquiries be pushed into the normal as well as the unusual influences of season, so large, that we cannot resist expressing a confident hope that some system will be adopted for their continuance. Soon there will be a skilled staff of inquirers into the local sanitary condition of London, who might combine meteorological with medical and sanitary inquiries. By a slight change of plan, the valuable records kept by the medical officers of districts under the poor law, which, so far as we know, have not hitherto been made available for the prosecution of either statistical or medical investigations, might supply information in regard to the prevalent kinds of sickness, and their comparative mortality, similar to that now furnished by the Registrar-General as regards death. Probably an arrangement might be made to obtain similar information from public hospitals, dispensaries, and other large institutions. From a careful classification of such facts, placed side by side with the meteorological phenomena of the time and place in which they have been observed, we should gradually obtain a more precise knowledge of the effects of weather, season, and climate upon the human constitution. The effect of weather on the human constitution is a common topic of conversation in this changeable climate of ours. It is by common consent allowed to be great, yet we absolutely possess no accurate acquaintance with the result produced on man's organization by a rise or fall of the barometer or thermometer, or the electrical state of the atmosphere.

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* Those years in which 1000 deaths did not occur were not esteemed plague years. Maitland declares in his History of London, that for twenty-five years before the fire of 1666, the city had never been clear of plague. From 1603 to 1679, the bills of mortality only exhibit three years entirely free.

We have thus investigated, as fully as the information within our reach would admit, the influence of season in the production of cholera. The presence of another co-efficient, at least, is, however, necessary to give character and energy to this influence. This, as we have before said, is to be sought in the existence of certain occasional, and therefore remediable, conditions, which by common consent are termed localizing causes. This constitutes what Dr. Barton has termed "the other blade of the shears." Whether this common opinion be true, and if so, what is the real nature of such localizing causes, we must leave to be determined on a future occasion.

E. Headlam Greenhow.

REVIEW II.

Guy's Hospital Reports. Third Series. Vol. I. — *London*, 1855. pp. 381.

IT is with much pleasure that we have received the first volume of a new series of the 'Guy's Hospital Reports.' These reports are essentially what hospital reports ought to be, and what the present state of medicine requires—viz., a series of carefully observed and recorded facts. Notwithstanding that the original contributors to the Reports "have been sadly reduced in number," we would congratulate the editors on the merits of the present volume, and would assure our readers that it fully maintains the high character which its predecessors had earned for it.

We proceed to give an analysis of the various papers, which amount to twenty:—

I. We have first a paper by Dr. W. W. GULL, entitled *Notes on Tænia, with fifty Cases, treated by the Oil of Male Fern.*—This indigenous plant, the *Lastræa Filix mas* of modern botanists, has been known as a vermifuge since the days of Dioscorides. About five-and-twenty years ago its efficacy was confirmed by Peschier, of Geneva, in several hundred cases; but it does not appear to have attracted much attention in this country until noticed by Dr. Christison in the 'Edinburgh Monthly Journal' for

1852. In the following year a paper appeared by Dr. Christison in that journal, giving the results of his experience in upwards of twenty cases, in all of which, without exception, the worm was discharged after a single dose. Dr. Gull's cases amply confirm the previous statements of Peschier and Dr. Christison, and show that an indigenous weed equals, if it does not surpass in efficacy as a vermifuge, the vaunted kousso of Abyssinia, the turpentine of America, or the pomegranate of the Continent. The preparation employed was the ethereal tincture of the rhizome, in doses of from one and a half to two drachms,* in a mucilaginous draught, occasionally followed by a saline laxative; which last, however, Dr. Gull does not consider necessary. In all Dr. Gull's 50 cases, as well as in many others treated by his colleagues, the remedy proved effectual. One patient had been labouring under tapeworm for eleven years, and had taken kousso six times. In 25, or one-half of the cases, we find the period mentioned which intervened between the administration of the drug and the expulsion of the worm. The average of all the cases was five hours and three-fifths. In 8 out of 20 cases the head of the worm was found. There can be no doubt, then, of the efficacy of the male fern as an anthelminthic; but we still want information as regards the permanence of the cure which it effects. In 5 only of Dr. Gull's cases are we informed that the cure was permanent, whereas in 6 cases we are told that the worm returned within a short period.

With regard to the locality in which the patients lived, all came from low-lying districts on the south banks of the Thames. As to sex, 27 were females, 14 males, and 9 doubtful, but probably females; this would make 36 females to 14 males. As to age, 11 were under ten years, 6 from ten to twenty, 14 from twenty to thirty, 11 from thirty to forty, and 8 above forty.

II. Dr. W. Gull records an interesting case of *Atrophic Softening of the Brain*, dependent upon occlusion of the innominata and left carotid arteries where they came off from the arch of the aorta. The origin of these vessels was quite obliterated by a fibrous structure, and the arch of the aorta was thickened and dilated. These morbid conditions appeared to have originated in an attack of aortitis, brought on by violent muscular exertion, nearly two years before death. During life, the patient, a female, aged forty-one, had had two apoplectic seizures; the first, shortly after the commencement of her illness, being followed by hemiplegia on the left side, and the second proving fatal. Extensive recent softening was found in the anterior and middle lobes of the left hemisphere, and the right corpus striatum was much wasted, and contained two irregular cysts.

The case is full of interest, as showing the production of cerebral softening from a want of due nutrition. Softening dependent upon disease of the arteries at the base of the brain is very common, but in this case the arteries at the base were "everywhere free from atheromatous deposit." The case is also interesting from the fact, that for upwards of a year the left subclavian artery appears to have been the only trunk which maintained the circulation in the head and neck and both upper extremities. It would have been very desirable to have ascertained the sources of the collateral circulation.

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* The dose appears large. In Dr. Christison's cases it did not exceed twenty-four grains.

III. *Researches on the Nature of the normal Destruction of Sugar in the Animal System.* By W. F. PAVY, M.D.—The sources of sugar in the circulation are now generally admitted to be twofold:—1. The saccharine matter originally contained in the food, or which has been formed by the action of the saliva and pancreatic juice upon its amylaceous constituents, is directly absorbed by the lacteals; and 2. The researches of M. Claude Bernard have shown that the liver possesses the power of forming sugar by chemical processes in its circulation, even when the food is destitute of both starch and sugar. The sugar from both these sources is conveyed to the right side of the heart—in the former case, by the thoracic duct and superior vena cava, and in the latter, by the hepatic veins and inferior cava. Dr. Pavy records the results of a number of experiments upon animals, which he performed in order to ascertain the place and manner in which the saccharine matter thus thrown into the circulation is destroyed. As to the place, he confirms the observations of previous experimenters, that the lungs are the chief seat of destruction of saccharine matter in the animal economy, though the process is also carried on in the systemic capillaries, especially those of the chylopoietic viscera. As to the manner, he combats the combustion theory of Liebig and other chemists, according to which the sugar is resolved, by the direct action of the oxygen absorbed during respiration, into water and carbonic acid; he endeavours to show that it becomes converted into lactic acid by the catalytic action of an azotized principle whose particles are in a state of change—viz., the fibrine of the blood.

This conclusion he supports by the following facts and arguments:—1. He found, by repeated experiments, that during the aëration of blood which contained sugar, and still retained its fibrine, the sugar largely disappeared; whilst in blood that had separated from its fibrine, and lost its vitality, no such destruction of sugar was observed. 2. Sugar, in its ordinary chemical bearings, manifests little susceptibility of direct oxidation, while it is with extreme facility metamorphosed when in contact with an azotized principle whose particles are in a state of change, becoming converted into lactic acid. 3. Lactic acid is shown to exist in arterial blood by the fact, that the former is separated from it by the follicles of the stomach and the muscular tissue. 4. It is well known that the presence of an acid will check the lactic acid fermentation; and Dr. Pavy ascertained, that by injecting diluted phosphoric acid into the jugular veins, so as to overcome the normal alkalinity of the blood, the metamorphosis of the sugar ceased to be accomplished as before. 5. Dr. Pavy notes a fact, also observed by Bernard, that if blood containing sugar be allowed to decompose, the sugar disappears, and the blood becomes acid.

IV. *On the Treatment of Purulent Ophthalmia, with Cases.* By J. F. FRANCE.—In this paper, Mr. France records the history of 9 cases, and gives the result of his treatment in 20 more. The cases varied in their nature, some being dependent on gonorrhœa, others on exposure to cold and wet, &c. The object of the paper is to show, first, "that this disease, left to its natural course, imperils in the very highest degree the faculty of vision;" and second, that "up to a given period it admits of remedy, and is divested of danger, by the plan of treatment adopted." The following is a summary of the treatment recommended. 1. In acute cases, local depletion by leeches, and scarifying the inner surfaces of the eyelids every twenty-four hours; also scarification of the ocular conjunctiva, where there is much chemosis. 2. The repeated application between the eyelids of a collyrium of nitrate of silver, containing from 1½ to 8 grains to the ounce. 3. Constant fomentation and ablution with decoction of poppies, containing a drachm of alum dissolved in each pint. 4. When there is much chemosis, after a purgative, calomel, until the chemosis is subdued, or the mouth begins to be affected. 5. Quinine, when there is debility. 6. Moderately nutritious diet. 7. During convalescence, a tonic diet and regimen, with local astringents and counter-irritants. A tabular view is given of the various cases, from which it appears that of thirty-six eyes subjected to the above treatment, four were lost, one remained under treatment, and thirty-one were saved, retaining perfect vision.

V. Mr. France also records a *Case of Pulsating Swelling in the Orbit*, which originated from a thrust with the point of an umbrella. The swelling appeared to be aneurismal in its nature, and is remarkable for having subsided spontaneously after some months, the only treatment adopted being quietude, and attention to the general health.

VI. Mr. France also mentions three cases of *Sub-conjunctival Dislocation of the Crystalline Lens.*—In one, the dislocation was inwards; in another, upwards; and in a third, outwards.

VII. *Cases selected from the Records of the Lying-in Charity of Guy's Hospital, with Remarks.* By Dr. J. C. W. Lever.—Of these, we would merely mention two cases of ovarian dropsy. In one, ovariotomy was performed, but, owing to the existence of adhesions, whose presence had not been expected, the whole of the cyst could not be removed, and the patient died of peritonitis thirty-nine hours after the operation. In the other case, after paracentesis had been performed three times, death took place from rupture of one of the cysts, and consequent peritonitis. In this case, though formidable adhesions had been expected, those which were found were very slight, and easily broken up by the finger. These two cases indicate the great difficulty of diagnosing the existence of adhesions in the case of ovarian tumours—a difficulty which, as urged by the late Dr. Hamilton, of Edinburgh, must always constitute an objection to the operation.

VIII. *Cases illustrating the Pathology of the Stomach.* By S. O. Habershon, M.D.—These cases, twenty-nine in number, are referred to the following pathological conditions:—atrophy of the mucous membrane, catarrh, superficial ulceration, follicular ulceration, chronic ulceration, perforating ulcer, diphtheritic inflammation, suppuration in the coats of the stomach, sloughing of the mucous membrane, and cancer. The subject of the pathological conditions of the mucous membrane of the stomach is one which is only beginning to attract the attention which it deserves, and which will amply repay such investigations as those of Dr. Habershon.

We cannot, however, avoid thinking that Dr. Habershon's numerous divisions are somewhat premature, and hardly justified by the present state of our knowledge. Thus Dr. Habershon himself admits that each of the various forms of ulcer which he describes may terminate in perforation, and hence we are led to the belief that some of the forms may be merely different stages of the same morbid condition. Dr. Habershon states, that he has repeatedly observed appearances similar to those described by Dr. Handfield Jones, as produced by atrophy and hypertrophy of the solitary glands of the stomach, and the development of cysts, but seems to think that these changes may often be produced by the mode of making the preparation, or by changes after death. Dr. Habershon also doubts the explanation given by Dr. Jones of the appearance of mammillation—viz., that it is produced by local atrophy of the mucous membrane, or the breaking up of the hypertrophied solitary glands, on the grounds that mammillation is more common than the existence or evidence of solitary glands, and that the appearance may be often produced artificially in a healthy mucous membrane. He does not appear to be aware that Dr. Jones has described two forms of mammillation, one of which "may be called healthy, and appears to depend on some unusual contraction of the corium of the mucous membrane."* There is one statement of Dr. Habershon's which requires confirmation, and as to which we must in the mean time express our doubts—viz., that "the sympathetic nerve may be observed in microscopical sections, at the base of the mucous membrane, sometimes *upon the capillary vessels*, and at other times leaving them." Dr. Habershon has some interesting observations on fatty degeneration of the stomach, in which the follicles are found to contain only granules of oil, in place of secreting cells. The symptoms in these cases he has found to be "a sense of great prostration and exhaustion, with complete loss of appetite, the tongue clean, no pain, nor thirst, nor vomiting." Case 17 is an interesting one of fæcal abscess, which established a communication between the greater curvature of the stomach and the transverse colon, and also with the right pleura. It is to be regretted that the symptoms are not given, and more especially that it is not stated whether or not there was fæcal vomiting, or vomiting of food during life, or what was the condition of the pylorus. Dr. Gairdner's observations of cases in which there was a communication between the stomach and transverse colon, would lead to the belief that fæcal vomiting is to be looked for chiefly in those instances in which the pyloric orifice is free, and vomiting of food when there is some constriction of the pylorus.†

Dr. Habershon seems to think that we may derive some assistance in the diagnosis between chronic ulcer of the stomach and cancerous disease, from the age of the patient. The average age of six cases of the former was thirty-two years, that of five of the latter, fifty.

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* Dr. C. H. Jones, Observations of Morbid Changes of the Mucous Membrane of the Stomach, p. 8.

† Edinburgh Medical Journal, p. 80. July, 1855.

IX. *Adenocele* forms the subject of a very interesting and important paper by Mr. BIRKETT. Since the Jacksonian prize of the Royal College of Surgeons was awarded to Mr. Birkett in 1848, for his essay on 'Diseases of the Breast,' he has been regarded, and most deservedly, an authority in these matters. Under the head of "adenocele," our author includes those tumours of the mamma which contain structures similar to, if not identical with, the normal secreting tissue of the gland, and more especially the cysto-sarcoma and chronic mammary tumours of surgical authors. M. Lebert,[*] of Paris, and Mr. Birkett, were the first to show by the microscope that these tumours, besides resembling the mammary tissue in general appearance, contained elementary tissues precisely similar; and we ourselves, as well as other observers, have had frequent opportunity of confirming the correctness of their observations. Recent researches have shown that the same law holds good in the case of tumours developed in the substance or neighbourhood of other glands, more especially the thyroid, salivary, and prostatic glands.[†] The fact that a new morbid deposit in the body should assume the complicated structure of glandular tissue, is one of high interest to the physiologist, as showing the great influence which the tissues themselves exercise over the nature of the materials separated from the blood, and the forms which these materials assume. Mr. Birkett makes three divisions of adenocele. In the first, he classes those new growths which are dense, firm, fibrous, and lobulated, and which correspond to the "chronic mammary tumours" of Sir A. Cooper. In the second, he includes those cystoid formations in the mamma, having growths within them, which appear to spring from their walls—the "cysto-sarcoma" of Müller; and to the third division he refers those cystoid formations first pointed out by Sir B. C. Brodie, as referable to a dilatation of portions of some of the lactiferous tubes. Of the first two classes he makes numerous subdivisions, according as the tumours contain the glandular ducts, sinuses, and secretion, or merely the cæcal terminations of the ducts, or according as these are united or not by connective tissue, &c. Now, while admitting the correctness and importance of Mr. Birkett's observations, we doubt if much advantage is to be derived from such a systematic classification; and we would venture to suggest that the three classes of adenocele, or at all events the two first, may be merely different stages in the development of one and the same tumour; or in other words, that the same tumour, examined at different stages of its growth, might present the characters of each of the classes. A simple cyst arising from dilatation of one of the lactiferous ducts, or more frequently from a transformation of some of the elementary tissues of the gland, may have glandular growths developed from its lining membrane, so as to constitute an example of the second class; and again, these glandular growths increasing in size more rapidly than their containing cyst, ultimately fill this, and come to belong to the first class—viz., a solid glandular tumour, enveloped by a fibro-cellular capsule. We have ourselves had occasion to examine specimens, apparently exhibiting these three stages in one tumour, and our opinion is further confirmed by two of the cases recorded by Mr. Birkett, in which there was an "alternation of the so-called chronic mammary tumour and the cysto-sarcoma in the same individual, and at different periods of life."

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[*] Physiologie Pathologique, tom. ii. p. 201.

[†] Paget's Lectures on Surgical Pathology, vol. ii. pp. 8, 263, 264, et seq.

Mr. Birkett also makes some interesting observations on the age and social condition of the patients in whom these tumours are developed, as also on the prognosis, diagnosis, and necessity for operation in such cases, for which we must refer the reader to the original paper.

X. *Two Cases of Chronic Inversion of the Uterus, successfully Removed by Ligature.* By HENRY OLDHAM, M.D.—In the one case, the inversion followed a first parturition, and the inverted portion, which comprised the whole of the body of the organ, was removed, nearly six years after, by means of the ligature. The ligature separated on the twenty-second day, and a fortnight after the patient was able to leave the hospital. In the other case, the ligature was applied for the removal of a fibrous polypus, which was afterwards found to have dragged along with it a considerable portion of the uterus. The tumour, including the portion of the uterus, was cut away on the twentieth day, and a fortnight after the patient left the hospital, recovered.

Dr. Oldham makes some valuable observations concerning the causes of inversion of the uterus, and endeavours to disprove the common opinion that it arises from a mismanagement of the third stage of labour, by traction of the placenta, still adherent to a flaccid, yielding uterus. He mentions that, out of 16,000 cases of labour attended by the *pupils* of Guy's Hospital, not a single case of inversion had occurred; and in the only instance of inversion following parturition which he had himself met with, traction of the placenta had certainly not been the cause. He is disposed to ascribe the first direction inwards of the uterus to an irregular contraction of its fibres, the remaining part of the inversion being completed by contraction of the non-inverted part or the bulging inverted portion, "which it grasps, and extrudes as a foreign body."

XI. *Lacerated Perinæum: Operation, nine days after Delivery.* By HENRY OLDHAM, M.D.—In this case, the operation was performed in the manner recommended by Mr. I. B. Brown, and was attended with perfect success. It does not appear, however, that the sphincter ani was divided, as recommended by Mr. Brown.

XII. *Cases of Tracheotomy; with Observations.* By THOMAS CALLAWAY.—The author records three instances in which he performed the operation of tracheotomy in infants not exceeding three years of age. In three of the cases, the operation was performed on account of suffocation following the swallowing of scalding liquids from the mouth of a tea-pot or tea-kettle. These cases, occurring within a period of twelve months, show that this accident is by no means unfrequent. Two of the cases recovered; one died. In the remaining case, the operation was performed on account of suffocation produced by impaction of a portion of food in the œsophagus, compressing the trachea, the gullet itself being compressed and narrowed by a scrofulous tumour of the vertebræ. This case terminated fatally.

XIII. *On the Intrinsic Calcification of the Permanent Tooth-pulp, as constantly associated with Dental Caries.* By S. James A. Salter, M.D. —Calcification of the tooth-pulp, Dr. Salter states he has found, after numerous examinations, constantly associated with dental caries. The calcification he describes as commencing in the centre of the tooth-pulp, near the extremity of the fang, by the development of minute calcareous nodules, which he designates "calcification islands," and in which he has not been able to make out either laminæ, tubes, or other histological forms. These islands increase in size and number until they become fused together into one calcified mass of "osteo-dentine," consisting of systems of dentine around isolated bloodvessels. The surface of the pulp nearest the decayed portion of the tooth is the last to calcify, and it is on this fact that Dr. Salter principally founds his distinction of the change which he has described, from the "dentine of repair," as described by Mr. Tomes, in which the dentine is developed on the exposed surface of the tooth-pulp by outgrowths of the superficial vessels. Dr. Salter also shows that, in some cases, the tooth-pulp is converted into *crusta petrosa;* and that, in such cases, there has generally been a preternaturally abundant communication between the tooth-pulp and periosteum. We have next—

XIV. *A Digest of Two Hundred and Nine additional Cases of Chorea, occurring in the Hospital.* By H. M. Hughes, M.D., and E. Burton Brown.—In a previous volume of the 'Guy's Hospital Reports' (1846, p. 360), Dr. Hughes gave a similar digest of 100 cases of chorea. We shall enumerate a few of the more important results deducible from the 309 cases of the combined Reports.

1. As to the *age* of the patients; we find—

85 cases did not exceed ten years.
137 cases were above ten and up to fifteen (inclusive).
65 cases were above fifteen and up to twenty (inclusive).
11 cases were above twenty.
In 11 cases the age was not stated.

2. As regards the *sex*, 241 of the cases, or almost 78 per cent., were females, and only 68, or 22 per cent., males. Curiously enough, however, under the age of nine years the proportion was very different—viz., 15 females to 12 males, or only 5 to 4; a fact which is also shown by the tables given by Romberg, in his work 'On Nervous Diseases.'

3. The *exciting cause* was determined in only 143 of the cases; of these—

In 87 cases the cause was fright.
12 " " injuries to the head.
13 " " rheumatism.
10 " " uterine affections.
2 " " avowed masturbation.
8 " " debility.
11 " " other diseases.

It is to be observed, that though rheumatism is mentioned as the exciting cause in only 13 cases, in very many instances it is stated to have previously existed. Thus, of 58 cases in which this was made a special subject of inquiry, it was found to have existed in 30, not to have existed in 28; and out of 104 cases in which special inquiries were made respecting rheumatism or the existence of a cardiac murmur, there were only 15 cases in which the patients were both free from the cardiac murmur and had not suffered from a previous attack of rheumatism.

4. As regards the *extent of the body* over which the choreic movements occurred, we find this specially referred to in 103 cases of the last Report:

In 42 cases they were general, or all over the body.
7 cases they were limited to both arms or upper part of body.
30 " " right side of body.
24 " " left side of body.

It appears that when chorea affects the whole body, a longer time is necessary to effect a cure, and that it is more frequently fatal than when it affects one side only. Dr. Hughes, indeed, observes, that he has never witnessed a case of chorea prove fatal in which the whole body was not affected.

5. As regards the *results* in both Reports, we find—

201 cases noted as cured.
30 " " relieved.
9 " " unrelieved.
10 " " died (2 of other complaints).
In 59 result not known.

6. Of the cases which were cured, the average *duration*, from the commencement of the treatment up to the period when the cure was effected, we find to be a fraction above five weeks.

7. *Treatment.*—Various remedies were employed, such as sulphate of zinc, iron, syrup of the iodide of zinc, mercury, lemon juice, &c.; but the one most generally employed, and the most successful, was the sulphate of zinc. Electricity was employed in 14 cases, of which 5 were cured, 1 was relieved, 3 were unrelieved, and in 5 the result was not stated.

8. *Fatal cases.*—Of the 309 cases mentioned in both Reports, 10 proved fatal. In addition to these, 8 other fatal cases are mentioned in the first Report as having occurred in Guy's Hospital; making in all 18 cases. Of these, excluding three children, 9 were females, and 6 males. In two of the fatal cases, death resulted from other diseases during the chorea; of the 16 remaining cases, 12 were those of persons verging on puberty, or who had actually passed that period. The duration of the disease in the fatal cases was in one only six days; and exceeds ten weeks only in one instance, in which it was eight years.

9. *Post-mortem appearances.*—14 of the fatal cases were examined after death. In 10 cases, disease of some kind was observed within the

cranium, such as congestion of the membranes, or sub-arachnoid effusions, or increased vascularity with softening of the cerebral substance. No particular notice is taken of the cerebellum. In 4 cases the brain and its membranes were quite healthy. In 6 cases the spinal cord was examined, and was found healthy in 2, softened in 2, and either it or its membranes otherwise diseased in 4. Of 14 cases in which the chest was examined, in 11 there existed a diseased condition of the sigmoid or auriculo-ventricular valves, and in 6 or 7 of these cases the disease consisted of vegetations upon the valves.

XV. Mr. Edward Cock next details 13 cases of Fracture of both Bones of the Leg, and 7 of Dislocations of the Ankle, *in which it appeared expedient to divide the tendo-Achillis, for the purpose of replacing the separated bones and retaining them in position.*—These cases occurred at Guy's Hospital, partly under his own care and partly under that of Mr. Poland and Mr. Birkett, and this treatment appears to have been adopted since the publication of two cases by Mr. De Morgan in 1850, in which it had been first employed. Few surgeons, we believe, have met with cases in which such an expedient appeared necessary, yet we can conceive that a case might sometimes occur in which division of the tendo-Achillis would facilitate reduction. We are, however, rather surprised that at one hospital it was found necessary to have recourse to such a procedure twenty times in the course of four years, and we cannot but consider such a practice as evincing somewhat of *nimia diligentia* on the part of the surgeon.

Mr. Edward Cock also records—

XVI. *Rare Cases of Fracture and Dislocation.*—Of these, four in number, perhaps the most interesting is one in which there was dislocation of the head of the humerus into the axilla, with fracture through the neck of the bone. The patient was a man, aged forty-two, who had fallen from a considerable height, and pitched on to his right shoulder. This accident is noticed by Chelius, and three cases have been recorded by Sir A. Cooper, and one by Mr. Aston Key. In Mr. Cock's case, as in all the others, it was found impossible to effect the reduction of the head of the bone, and the case was treated so as to encourage the formation of an artificial joint between the broken end of the shaft and the glenoid cavity.

XVII. *Observations on Reinsch's Test for Arsenic.* By W. Odling, M.B.—These observations are of value, and tend to increase the already good opinion in which Reinsch's test for arsenic is held by British toxicologists. As regards the *delicacy* of the test, Mr. Rainey's observations had previously shown that by means of it $\frac{1}{1000}$th part of a grain of arsenious acid might be detected in solution; but Dr. Odling has succeeded with Reinsch's test in positively determining the presence of $\frac{1}{5000}$th part of a grain. Dr. Odling next proceeds to observe, that the objection which has been raised to Reinsch's test, that during ebullition a portion of the arsenic is lost, is one of no value; for, though a loss does take place, he shows, by an ingenious experiment, that when the quantity of arsenic is small this is quite inappreciable. Lastly, Dr. Odling has found that the tersulphuret of arsenic is not, as it is always represented, quite insoluble in diluted hydrochloric acid, but quite the reverse, and that therefore Reinsch's test is equally applicable for the detection of this form of the metal. He has also ascertained that, provided oxidizing agents, such as nitre and chlorate of potass, or the perchloride of iron, be not present in large quantity, Reinsch's test is applicable to the detection of arsenic when in the state of arsenic acid.

XVIII. *On the Pathology and Treatment of Alkaline Conditions of the Urine.* By G. OWEN REES, M.D., F.R.S.—This paper is of great interest, both in a scientific and practical point of view, and coming from Dr. Rees, merits the attentive consideration of the profession. Ten years ago,* Dr. Rees had expressed his doubts as to the existence of the "phosphatic diathesis" of Dr. Prout; and he now adds, that his further experience is quite opposed to the belief that alkaline urine, except when produced by diet or medicines, is ever secreted as such by the kidneys; but that he believes that the urine secreted of a healthy acid character becomes alkaline from disease of the mucous surfaces over which it has to pass. Dr. Rees well observes, that a deposit of earthy phosphates in alkaline urine by no means indicates an excess of earthy salts, as may be proved by precipitating the portion which remains dissolved in the urine, and adding it to that already formed. A real excess of phosphates in the urine he has only found in cases of mollities ossium, in scrofulous and rickety children, and in some rare forms of dyspepsia. He supports his views by some important observations on the actions of alkaline remedies: "If the urine," he remarks, "be secreted of alkaline reaction, owing to the administration of alkaline remedies, there is no tendency whatever to deposit of phosphates." Again, he has found that, in cases of the so-called "phosphatic diathesis," an *alkaline* plan of treatment "answers excellently, and is the surest method of obtaining the secretion of an acid urine." By this, he says, the urine is rendered alkaline as it is secreted by the kidney, and less irritating to the inflamed mucous surfaces; and, as the patient convalesces and the alkaline remedies are gradually discontinued, the urine is discharged from the urethra of its healthy acid character. With regard to the fact that, in these cases, *acid* remedies are sometimes productive of benefit, Dr. Rees observes that, in the slighter cases, they may be so by improving the general health; while, at the same time, as they do not increase the acidity of the urine secreted, they do not render it more irritating. In many cases, however, he has found an alkaline treatment restore the healthy acidity of the urine, after the exhibition of mineral acids had completely failed. One remarkable case is adduced, to show the efficacy of alkaline remedies in cases of alkaline urine depositing phosphates. With regard to the form of alkaline treatment he has employed in such cases, he observes:

> "When it is desirable to render the secreted urine alkaline, the best and most speedy mode of effecting the purpose consists in the exhibition of neutral salts composed of vegetable acids (the citric and tartaric), in combination with an alkaline base." (page 307.)

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* On the Analysis of the Blood and Urine, second edition, p. 183. 1845.

XIX. *Report of all the Cases of Fever which occurred in Guy's Hospital during the Year* 1854, *with Remarks having especial Reference to the Typhus and Typhoid Distinctions.* By SAMUEL WILKS, M.D.—In the last volume of the 'Guy's Hospital Reports,' a similar report for the latter half of 1853 was published by Dr. Wilks. Putting these two Reports together, we find in all 249 cases of fever, which are classified thus:

Febricula	73 cases.
Typhus (26 with eruption, 29 with none)	55 „
Typhoid (91 eruption, 25 none)	116 „
Cases of fever which had cholera	5 „
	249

Dr. Wilks's observations bear out, in a most unequivocal manner, Dr. Jenner's distinctions of continued fever into the typhus and typhoid forms. In all the cases (91) which presented a well-marked rose-coloured eruption, diarrhœa was an urgent symptom; and in those cases which proved fatal, there was found to be ulceration of Peyer's patches in the small intestine; while in 25 cases of fever with diarrhœa, in which there was no eruption, the symptoms in every other respect followed the same type. On the other hand, of the 26 cases which presented the mulberry rash of typhus, in none was there urgent diarrhœa; and in none which proved fatal was there found any disease of the small intestines. Speaking of the characters of the eruption in the two forms of fever, he remarks:

> "The clear skin with the pink spots scattered over it, in the typhoid form, and the mottled skin of the typhus, are generally sufficiently distinct. How the two can be confounded in the majority of instances, I cannot well imagine." (page 343.)

With regard to the difference in the duration of the fever, he says, as a rule, typhus "comes to an end on the thirteenth or fourteenth day;" whereas in typhoid fever "it is never until the expiration of three weeks that a change is observed." As to the intercommunicability of the two forms, he observes: "In these instances, where it has been known that other members of a household have had fever, and its nature discovered, it has invariably been of the same character as that of the patient admitted to the hospital."

Dr. Wilks mentions an instance in which four members of one family were attacked with typhus. One of these was a girl, aged eleven years, in whom we might more naturally have expected typhoid fever, according to the theory of those who maintain that there is but one poison, and that the age of the patient and other extraneous causes determine whether the typhus or typhoid form is developed. Dr. Wilks's observations, as we have said, confirm in every respect the views of Dr. Jenner, which are now supported by most members of the profession who have had opportunities of judging of the differences between the two forms of fever. We have lastly—

XX. *Miscellaneous Cases.*—Of these the most remarkable are two cases by Dr. Wilks, in which there was an excess of white corpuscles in the blood, in connexion with enlargement of the spleen, and which are illustrations of the disease originally described by Virchow under the name of leukhæmia,* and afterwards by Dr. Hughes Bennett under that of leucocythæmia. Dr. Wilks had examined the blood of upwards of fifty anæmic patients suffering from scurvy, purpura, &c., as also in twelve cases of ague, with enlarged spleen, without finding the proportion of white corpuscles abnormally increased.

Dr. Gull relates a case of strumous disease of the mesenteric glands, in which the oily ingredients of the food, such as cod-liver oil, passed through the intestines but little changed.

Review III.

1. *Lehrbuch der Pathologischen Anatomie.* Von Carl Rokitansky, &c., &c. Dritte umgearbeitete Auflage. 1ter Band, enthaltend Allgemeine Pathologische Anatomie und Anomalien des Blutes.—*Wien*, 1855. (Mit 130 Holzschnitten.)

   A Manual of Pathological Anatomy. By Carl Rokitansky, &c., &c. Third edition, completely revised. Vol. 1, containing General Pathological Anatomy and Abnormal Conditions of the Blood.—*Vienna*, 1855. (130 woodcuts.)

2. *A Manual of General Pathological Anatomy.* Translated from the German by W. E. Swaine, M.D., F.R.C.P., &c. Vol. 1. Sydenham Society, 1854.

3. *Rudiments of Pathological Histology.* By Carl Wedl, M.D., &c. With 172 illustrations on wood. Translated and edited for the Sydenham Society by George Busk, F.R.S.—*London*, 1855.

4. *Handbuch der Allgemeinen Pathologischen Anatomie.* Von Dr. August Foerster, Professor an der Universitaet zu Goettingen. Vol. II.—*Leipzig*, 1855.

   Manual of General Pathological Anatomy. By Dr. August Foerster, Professor in the University of Goettingen.

5. *A Manual of Pathological Anatomy.* By C. Handfield Jones, M.B., F.R.S., and E. H. Sieveking, M.D.—*London*, 1854.

The volume placed at the head of the works above-named is the first of the volumes which form Rokitansky's laborious work 'On Pathological Anatomy.' Though the first volume, it was published the last of the series; and naturally enough, for the antecedently published volumes, as the author himself tells us, furnish the groundwork of the views propounded in it. The general conclusions on pathology here brought together are, in truth, to be considered as the summary of the particular facts detailed in the volumes which treat of Special Pathology.

A new edition—a third edition—of this volume has just been published at Vienna, and appears most ill-naturedly almost at the same moment in which the translation of the *first* edition of it is presented to the English reader by the Sydenham Society, under the auspices of Dr. Swaine. This first edition was published in 1846, and thus, in an English form, comes under our notice simultaneously with the newly-revised edition of the original work published in 1855. The council of the Sydenham Society are in no way blameable for this unpleasant *contretemps.* The council, encouraged, Dr. Swaine tells us, by the author himself, did not hesitate to defer from year to year the publication of the first volume, until they felt that it would be improper to tax the patience of the members any further. They therefore published a translation of the edition of 1846 in January, 1855; and in February, 1855, appears from Rokitansky's hands the long-expected renovated volume in a third edition, and in a new form.

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* Virchow and Reinhard's Archiv, p. 570. 1847.

This present volume necessarily differs, Rokitansky says in his few lines of preface, essentially from the former one, for the reason that many important facts have been added to pathology since the publication of the former edition. In this volume his main endeavour has been to confine himself strictly to a detail of facts, and thereby to render the book a practical guide for the student of pathological anatomy.

Here, therefore, we may consider that we shall find detailed the present views and opinions of German pathologists, expressed by the most celebrated among them, on the matters comprised under the head of General Pathology. Some of these views and opinions, thus shaped and modelled by the light of fresh researches and better observation, we shall take the present occasion of bringing before our readers, and somewhat in detail. A condensed account of the facts of general pathology, as most newly propounded by the Viennese school, would, we believe, prove instructive and useful, especially to those who have not the opportunity of studying them in their original dress.

We owe a debt of gratitude to our German brethren in this matter of pathology. With their names, in an especial manner, must be associated the rapid progress which has of late years marked this branch of medical knowledge. Their schools have been the chief workshops, the sources, from whence have been furnished the facts on which rest our modern ideas of pathological anatomy. Nobody, we fancy, will dispute the claims of our Teutonic brothers to such credit; but if a questioner should trouble us for our proofs, we will reply by simply asking him what were the sources whence he derived his knowledge on the subject; and where is the list of *our* standard works on pathological anatomy?

Hence, then, in bringing the subject of pathology before our readers, we do so, and naturally, in connexion chiefly with the names of Rokitansky and of other of his countrymen. The importance of the study, and its direct practical application to therapeutics, have been fully recognised here; and everywhere around us we see the fruits of pathological observation daily ripening and accumulating, through the unwearied energy of numerous inquirers. We need say no one word as to the value of the study. A knowledge of the diseased conditions of the different organs and parts of the human body, as they present themselves to the observer after death and during life, is so manifestly necessary to him who would practise the art of medicine with advantage to his patient and with satisfaction to himself, that it would be mere waste of time to enlarge upon these advantages. When the student has acquired a certain knowledge in different branches of medical science, the necessity of gaining a knowledge in this also is, as it were, forced upon him. At the bed-side of the patient he will have learnt, that for the scientific application of remedies, accuracy in diagnosis is indispensable; and that for accuracy of diagnosis, a knowledge of the morbid changes which accompany or result from disease, is equally so. His accuracy in diagnosis will, in fact, be measured by the constancy with which he compares signs and symptoms of disease observed during life, with facts revealed by the scalpel after death.

And then, again, how can he treat disease more creditably than the mere empiric, unless he have clear and succinct ideas as to the nature of the disease, against which he would employ his art? and how can he ever arrive at any knowledge concerning the nature of disease, except through the study of pathology? Sooner or later he will inevitably discover, that he who is not a pathologist can never be a physician. Let him recollect, that the works of Auenbrugger and Laennec were essentially founded on the results of pathological investigations; and that no medical knowledge (in its highest sense) has a foundation which is not based on such.

What is pathology? What is pathological anatomy? They are not the same things, though often used by us to express the same idea. Pathological anatomy stands in the same relation to pathology, that anatomy holds in reference to physiology; it is the foundation of the physiology of disease. And this will at once show us that pathology is something more than a mere description of the deviations of the organs and parts of the body from their condition of health; something beyond a mere study of the physical facts presented to us in morbid alterations of dead structures. Pathology is to be learnt during life, as well as after death; its study must commence at the bed-side of the patient, and must go hand in hand with clinical medicine. Its final object is to discover the disease, and so assist us in applying the remedy. Well: and here at the bed-side we may often, by observation of disturbances in functions manifested during life, arrive at pathological facts of which anatomy, after death, tells us absolutely nothing.

There are many diseases, in truth, of whose pathology we know nothing, excepting what we learn of it through disturbances of function manifested during life. What, for example, do we know of the pathology of tetanus, or of hydrophobia, or we may even say of that large class of diseases which has, yet with insufficient reasons, been classed under the head of blood diseases? what do we know of these beyond what we learn of them through the disorders of function by which they are characterized during life? Again and again has the most scrupulous sagacity sought in every part of the body for some explanation of the symptoms which represent those disorders in life; but anatomy, aided by the microscope and chemistry, has told us nothing to the purpose concerning them. Hence, then, all we know of the pathology of such disorders is derived from what we have observed during life; and therefore let it be well understood, pathology is a knowledge of disordered functions, as well as of dead anatomical facts.

But pathology, in its full sense, is even still more than this. It takes cognizance of diseases which lie dormant in the body, and which are not yet manifested either by disorder of function or by change of structure.

The diseases, for instance, which we call hereditary, while dormant in the individual who is the subject of them, are of this class; in such there must be some invisible taint, so to speak, inherent in their constitutions, and inherent from the first moment that the germ within the womb commenced its evolutions. The periods, again, of the incubation of diseases are pathological periods in the life of the individual affected by them. Though neither disordered symptoms nor structural changes, appreciable during those periods, indicate the existence of disease, still reason tells us disease is there, quietly working, gradually unfolding, and coming to maturity and complete development.

It may, perhaps, not be out of place at the present moment to express this word of caution, as to the proper signification which the student should attach to the word pathology; at this moment, when we are all so keenly engaged in the observation and recording of material facts. Thus occupied, we are naturally inclined to forget that pathological anatomy has but limited information to give us concerning the nature of disease; that in reality neither the scalpel, nor the microscope, nor chemical re-agents, can open to us the whole domain of pathology. We must strictly understand that observation thus aided can reach only a certain point, that it cannot embrace the whole; for, as we have said, reason steps in, and tells us there are diseases, or deranged conditions of organs, which totally elude our present powers of observation. The facts thus afforded us by reason are manifestly parts of pathology, but of pathology where neither the sight nor the touch can take cognizance.

Pathology, then, is the study of anatomical changes of organization; of the disorders of functions; and of latent and hidden diseases, and diseased conditions of the body. Through its aid we endeavour to trace out the nature of disease.

And what is the meaning which we are to attach to that word, disease? A correct answer to this question is of every importance to the practitioner of medicine, and we may therefore be permitted to dwell upon the matter for a moment; we say advisedly, to the practitioner of medicine, for what, after all, is the last and final object of these pathological studies, but the cure of disease? How poor and indifferent they become in value, stripped of their special purpose! What avails a knowledge of the study, if it come not to use practically in the physician's hand?

What, then, is disease? Now, there is no one fact of greater importance, inasmuch as it bears immediately on the treatment of disease, which has resulted from modern pathological researches, than this—viz., that local changes of structure are, in the very great majority of instances, where they become the proper subjects of medical observation, connected with some general disorder of the whole system; the structural alteration not being essentially the disease, but only one, and as it were the partial, expression of some widely extended influence, some poisoning of the blood—to use the language of metaphor—some deranged state of the nervous centres, some faultiness in the nutritive powers, some depraved condition of the absorbing or secreting forces. The further our knowledge of pathology advances, the more clearly does the fact become established. The local accidents which represent the disease to us are not *the disease*;

we may study, and must study, as pathologists, the individual peculiarities and characters of these accidents: we must learn all that anatomy and chemistry can tell us of them; but then, as physicians, we should remember how little, in almost all cases, these accidents constitute the real disease which is afflicting or destroying the body. The ulcerated intestines and the spots on the skin in fever; the abnormal deposits found in the lungs in phthisis; the watery evacuations and the blue skin in cholera; the livid ecchymoses of purpura; the coma of uræmic disease; the pustules of small-pox—these are not the diseases; they are positively neither more nor less than its consequences; the sequents of certain antecedents whose essential nature eludes our grasp. They are not, therefore, the special objects of our treatment; our efforts are employed in the endeavour to neutralize the causes which produce them.

Now, of these morbid changes of parts, pathological anatomy gives us clear and succinct ideas; it tells us, truly, but a part of their entire history, but it defines accurately what it tells us, and marks clearly the line where our knowledge ceases: it brings us to a certain point in the history of the diseases of which they are the representatives, and there it leaves us; it shows us structural changes, abnormal conditions, and diseased products. But why these structural changes, why these abnormal conditions, why these diseased products, it tells us not, or it tells us very darkly. It shows us where our knowledge is safe and certain, it tells us where the history is complicated with doubts and difficulties, and lets us see clearly enough where it is shrouded in complete mystery. And it is just this very knowledge of his own ignorance which distinguishes the philosophic physician from the barren empiric.

We linger on this subject, for a clear comprehension of it, as before said, intimately affects the fulfilment of our last and highest duties as physicians—the treatment of disease; and, moreover, the very nature of pathological studies naturally inclines us too much to absorb our attention in investigations of the diseased product, and so to forget that the product is in reality the mere representative of the disease, and not the disease itself. Tubercular deposits, for instance, we may examine with the utmost care,—all their physical and chemical relations thoroughly make ourselves masters of; but when we have done all this, what, as physicians, have we learnt of the ultimate nature of the cause which provokes the deposit? what thereby have we learnt which shall guide us to a correct method of its treatment? This we have done: we have made ourselves masters of the method of the *development* of the disease at a particular stage of its progress, but we learn thereby nothing of the action which presides over the development and production of the abnormal deposit.

No; he who would know such a history of tubercle as shall avail him in his practical art, must look for his knowledge not here alone, in the minute details of its pathological anatomy, but in the whole history of the disease likewise, as manifested by symptoms in the living, and by reasonings on its hereditary and other special characters. This entire history of it is, indeed, its true pathology.

But let us not be misunderstood. In all we have here said we have not implied anything to undervalue the importance of pathological anatomy. What we desire is to give the study its real and true value as an aid to the knowledge of the physician's art. It is, in truth, the only sure stepping-stone we possess to a right understanding of the treatment of diseases. It enables us, by appeals to material facts, to correct the erroneous views which fancy engenders; it gives us a solid basis whereon to establish a path of observation by which we may reasonably hope to arrive at their better comprehension. And from pathology also has arisen the physical diagnosis of internal diseases; that method of diagnosis by which we have been enabled to attain an unhoped-for degree of certainty in our discrimination of those diseases.

What we desire is to see its proper and legitimate value given to the facts of pathology, and that this value should neither be undervalued nor unfairly exalted. No one can pretend, as we have said, to the title of physician who is not conversant with structural diseases; but, on the other hand, the most intimate knowledge of structural diseases will of itself never make a man a physician. Pathology must be the chief means which shall direct us to a correct treatment of disease, and it will do so by telling us *how much* we know of disease. Where our knowledge of disease, of its intimate, its original nature is dark and obscure, there will our treatment be experimental, empirical, and doubtful; this useful lesson an enlightened pathology alone could teach us.

And indeed it is not hard so to turn the facts which pathology places to our hand, as to misuse them; rather it is difficult to restrain ourselves from putting them to unfit purposes. In our natural eagerness to seize upon any rational semblance of an aid which may help us in the cure of disease, how often do we fix our hopes on things that fail us. Every page of the history of medicine has something to teach us on this score; and the tale may be told perhaps of to-day, as we might tell it of the days of Broussais. Let the reader recal to his mind the modern history of blood diseases (so-called). A few years ago, and humoral pathology was paramount; in the blood existed, was to be sought, and would be found, the disease which by symptoms was demonstrating its presence in the body; animal chemistry would declare all this to us. Well, and how stands the matter now that time and experience have tested the value of this modern humorism? What are the facts which it has added to our intimate acquaintance with disease?

On this head we eagerly turned for information to that part of Rokitansky's new volume which should teach of the *Dyscrasiæ;* and, to our surprise, sought in vain for that painfully familiar term. Can we doubt that he has acted wisely and judiciously in thus ceasing to define by special terms hypothetical conditions of the body?

No one can deny, or refuse to admit, that, associated with certain diseases, particular alterations occur both in the quantitative and qualitative relations of the constituents of the blood; but surely there is nothing in the history of the anomalous conditions which the fluid presents under such circumstances, nothing in the changed states of its fibrine, its colouring matter, its cells, its salts, its albumen, &c., which in any way warrants us in seeking in such conditions the distinctive and peculiar nature of the disease, or justify us in associating particular diseases with particular conditions of the blood. We deny it not; the time may come when these alterations of the blood's composition shall lead us to definite results; but most assuredly they are far from having done so at present. Our knowledge of these alterations is as yet most indistinct and imperfect, and what can be said concerning them is vague and indefinite.

Take what is called the tuberculous crasis, and let us see how far facts give us legitimate grounds for the assumption of such a particular condition as the expression of tubercular disease manifested in the blood. We will take the description given of it in the excellent manual on Pathology of Drs. H. Jones and Sieveking:—"With respect to the real nature of the tuberculous crasis," says Dr. H. Jones, "we have scarce any exact knowledge. It is evidently a special dyscrasia, intimately connected, as we know, with causes of debility, and leading to the effusion of a matter which shows only the feeblest traces of organization." Now, we must object here at once, and the objection applies more or less to the description of all the other so-called crases of the blood, that what is not proved is here taken for granted; and that there is manifestly a confusion in that part of the history which tells of the crasis and which tells of the exudation. You assume that there is a "special dyscrasia which leads to the effusion of a matter," &c.; but have we not, from observation of the facts of the case, equally as good a right to say, that the effusion of the matter leads to the dyscrasia? What proof have you that some dyscrasial state of the blood precedes the exudation of the tubercle; that is to say, such a dyscrasial state which shall admit of chemical or anatomical definition? Describe the special state, if you are able, in proper terms; or if not, then we think we may very fairly ask you to exclude from your pathology the assumption of such a condition until the time arrives that experiment or observation has given us the right grounds for doing so, by enabling you to define it in set terms. Let there be dyscrasial states of the blood; we can well believe that there are; but so long as such states are founded almost exclusively on hypothesis—legitimate hypothesis, if you please—let us not commit the fallacy of assuming and using such hypothesis in our practical dealing with disease, as though it were a demonstrated fact.

What, we should desire to ask, do you who believe in a tuberculous crasis of the blood pretend to know concerning the condition of the blood, which you say gives rise to the exudation—what do you know of it independently of the actual existence of tubercle somewhere in the body? Do you, can you, ever speak of such a crasis in a body where no tubercle is present in some of its parts or organs? An esteemed writer on consumption speaks of the disease as existing prior to the deposition of tubercle in the lungs; he therefore, we suppose, assumes in practice the truth of the hypothetical crasis; but we would ask, appealing to his own and the experience of every auscultator, how does he prove the truth of the negative he assumes—viz., the non-existence of the tubercular matter in the lungs? Certainly he can only do so by the aid of physical diagnosis, and most certainly physical diagnosis cannot give him that aid; for it is a fact placed beyond any kind of doubt, that a certain amount of tubercular matter *may* exist in the lungs, and at the same time not betray its existence by any alterations in the natural sounds appreciable by percussion or auscultation.

The fact seems really to be, that we know nothing of the nature of these so-called crases of the blood; all we can say is that, in conjunction with certain diseased conditions of the body, the normal constitution of the blood undergoes changes. How can we speak of a typhus crasis, or an exanthematous crasis, or a croupous crasis, apart from those series of phenomena which demonstrate the existence of those diseases to us in each instance? The crasis, the peculiar condition of the blood, is, we have every right to assume, a partial phenomenon of the disease, associated with some particular *period* of its development. We know far too little of the act which presides over the exudation of the tubercle, for instance, to fix the true and actual period of the development of the peculiar blood-condition which accompanies tubercular disease. As the facts stand at present, we may very fairly maintain that in most cases it is just as probable that the exudation precedes and provokes the crasis, as that the crasis gives occasion to the exudation.

One most important element, as an agent possibly influencing the production of disease, is herein altogether overlooked, and that is the nervous force. That the nervous system must play a part in the guidance and generation of disease is certain; wherever there exists in an organ or part, the display of a physiological function, an office to discharge, or a duty to preside over, we may be certain that the presiding agent which displays the healthy function is capable of being subjected to diseased activity. Applying this self-evident truth to the nervous system, and reflecting for a moment on the all-pervading nature of its influence, the necessity for its continual and instant exercise, as regards nutrition at least, and the mystery which shrouds the manner of its communication with the objects it influences—does it not follow, as a self-evident proposition, that until this hidden region of physiology has been visited, and some of its secret tales told by their discoverer, we have one, and perhaps the greatest, part of the problem of disease unravelled? Nervous influence guides the parts to their healthy nutrition; how shall it not, when perverted, guide them to an unhealthy display of the nutritive function?

This doctrine of crases involves us in an exclusive humorism; but the days of the battles between the Solidists and Humorists should be forgotten in Medicine, just as Neptunism and Plutonism have vanished in Geology—they who supported the igneous, and they who fought in defence of the aqueous, origin of rocks, were both right in part, and wrong only in the exclusiveness with which they defended their particular doctrines. Equally may we believe that they who place the original seat of disease either in the fluids alone, or in the solids alone, split upon the rock of exclusiveness.

What more simple and rational than the conclusion, that *any* item which forms the body may be originally affected by the disease, and secondarily communicate its disordered state to other items? In this wonderful frame of ours, just as in the mighty macrocosm of the universe, we may be sure that not the smallest deviation from the right direction of the natural forces of any one portion of it remains unresented by the whole; unity of contrivance is everywhere manifest—manifest in the individual parts, and manifest in the perfected being, which is the resultant of the individual parts combined; and as in its healthy, so in its diseased conditions, that which touches a part touches the whole, and with greater or less force and effect according to the energy of the agent and the sensitiveness of the part acted on. In health and in disease the solids everywhere and always affect the fluids, and the fluids the solids.

And this is why we object to the doctrine of primary crases of the blood, and because it is wanting in proof, and because the relation of the crases to particular exudations is insufficiently shown. And in this sense it is that we think Rokitansky has done wisely in reducing his account of the anomalous conditions of the blood to a bare exposition of facts, and to a simple description of the particular diseases with which the anomalous conditions are found to be more or less frequently associated.

It is not alone in this particular instance that we approve of Rokitansky's present volume. When we compare its contents generally with those of its predecessor, we may fairly say that the subject matter, sufficiently abstruse in itself, is now made, comparatively speaking, charmingly simple. The wonderful mass of erudition and research somewhat obscurely related, and partially clouded by hypothetical reasonings and a difficult diction, contained in the former edition, is now worked into clear form, definitely arranged, stripped of mysticism, and brought down to the comprehension of the student, and made available for the practitioner of medicine. We say available for the practitioner, for it is only justice to our author to say, that his relation and mode of treatment of the pathology of disease have throughout a distinct practical bearing upon the practice of medicine; herein fulfilling, as we consider, the most important part of the pathologist's task—viz., the so disposing of his materials as to render them readily convertible to their final and proper object—the cure of disease. The work gives all that can be told of positive and available information on the subject it treats of; and the author is he who, from his immense experience, is more capable than any other of instructing the world in these details.

Of Rokitansky's volume we may say generally, that it has been almost entirely re-written. Necessarily the greater number of specific facts relating to the history of diseases are still found unchanged in their particular descriptions; but the arrangement of them, and their theoretical import and their relative connexions, have all been reconsidered, and adapted to the advance of the pathological knowledge of the present moment. This will be particularly seen in the most important part of the work—that which treats of organized new-growths. The history of tumours has undergone complete revision, and has been made much more clear and satisfactory. We may see this at once by comparing his former and present arrangements of cancer growths. Formerly sarcoma and carcinoma were placed together; they are now separated. We subjoin the present and former divisions of cancer.

Present Division.	*Former Division.*
1. Fibrous carcinoma.	Colloid gelatinous cancer.
2. Medullary carcinoma.	Fibro-carcinoma.
a. Villous cancer.	Medullary carcinoma.
b. Cancer melanodes.	Cancer melanodes.
3. Epithelial cancer.	Typhous substance.
4. Gelatinous cancer.	Villous cancer.
5. Carcinoma fasciculatum.	Epithelial growths, cancer. Carcinoma fasciculatum.
Cystic carcinoma.	Cysto-carcinoma.

The same observations will apply, more or less, to nearly every matter treated of in this volume. The chapter on Anomalies in respect of Number of Parts, has been enriched by many woodcuts, representing the chief varieties of fatal abnormities, rendering this part of the work highly interesting. The addition of woodcuts to the volume generally is not one of its least improvements; indeed, it is impossible, by mere verbal description, to give the reader correct ideas of the minute microscopical characters of diseased parts and products. This is so universally felt, that a work on pathology not so illustrated would hardly meet with much attention at the present moment. Truly we may say, when we view the number of excellent works thus illustrated, now lying before us, that the student and practitioner need plead no excuse for being ignorant of even the microscopic characters of diseased structures.

In speaking of the illustrations of the histological facts of pathology, we may here make mention of 'Wedl's Rudiments of Pathology,' a translation of which, by Mr. Busk, has just been published by the Sydenham Society; it contains a series of most faithful representations, in woodcuts, of the elementary characters of diseased structures. It would not be fair to the author, however, if we did not refer those who are acquainted with the German language to the original work, for therein he will find the illustrations most beautifully executed, executed indeed in a style which we rarely find equalled in this country. Of Mr. Busk's translation of this valuable volume we have nothing to say but what is good. It is one of the most useful works which have of late appeared under the auspices of the above Society; and as a guide to a knowledge of the histology of disease, there is certainly none with which we are acquainted wherein the student will find a greater abundance of valuable information.

We cannot, however, help taking exception to the obscurity of language in which Wedl so often shrouds his ideas; and we fear that in this respect the student will often find his patience tried, when attempting to master this author's meaning. We regret this the more because we consider that the opinions given are generally sound and good *au fond.* The descriptions also are frequently too detailed and diffusely set down; they are doubtless true and accurate, but they require unravelling; and in these days we really expect everything to come to hand with clearness and readiness; that the fact should be shortly stated and distinctly defined. In the following extract, for example, there may be good philosophy, but we cannot tolerate such a manner of telling it:

> "It must be allowed that our ideas of tubercle and of cancer are not widely remote, but merely expressions (categories) indispensable in anatomical language,

and requisite for the designation of particular modes of development of certain new-growths. The institution of categories of this kind proceeds from the methods pursued in human thought; at the same time it should not be forgotten that these indispensable categories have such numerous vacancies and deficiencies, that they can only be regarded as ideal, and not as things having an actual existence. Nature shows that in one and the same individual a fibroid tumour may be formed in the uterus, and a medullary cancer in the liver; where, then, is our supposed cancerous *dyscrasia?* It is well known that decided tuberculosis of the lungs, with cavities, &c., occurs, together with cancer in other organs, with intermediate forms. Where, then, is the boundary between cancer and tubercle?" (Transl. p. 579.)

Here, again, we have an opinion of our author, good and sensible as we believe, but nevertheless in language most unfortunately developed:

"In this case it will be necessary further to inquire what criterion we may possess to enable us to judge that a new-growth is of a cancerous nature? These criteria are to be sought, not in the morphological condition of the separate parts of it, but in the entire course of the evolution and involution of the organized new-structure. The study of its evolution teaches us that the *cancer-blastema* (not meaning to imply, under this name, any specificality in it) may remain in its organization as a nuclear, or imperfect cell-formation, of bloodvessels, bone, and cartilage. But however great may be the organizability of the cancer-blastema, it is frequently impeded in its development, inasmuch as the material afforded to the elementary organs of the growth is unfit for their nutrition within the normal limits, owing to the predominance of one substance or another, *fibrine, albumen,* colloid, fat, colouring matter, water, or mineral constituents. The new-formed elementary organ, therefore, falls into a state of involution, and undergoes the corresponding metamorphoses; whilst, in other places, in consequence of hypertrophy, it becomes asymmetrical and deformed. We consider, therefore, that the principal criteria by which we can judge of the cancerous nature of a new-formation must be sought in the multifariousness of its organic development, in the size, shape, and involution of the cells, and of the substance formed by them; the remaining stationary at an embryonic stage; and in the peculiar remarkable inequality in the stages of organization in the various tissues." (Trans. p. 537.)

We again repeat, that Wedl's work is a very valuable addition to our literature; and it is only in respect of occasional embroglios of language, such as here exemplified, that we take exception to it. And while on this subject of language we might remark, that any one who would present the profession with a trustworthy dictionary of modern medical terms would deserve well of his brethren; such an explanatory reference is indeed very much wanted. It is certain that the most literary character who flourished in our profession fifteen years ago, were he suddenly translated from his grave into a modern medical society, would find half what he heard the language of Houyhnhnms to his ears; and we cannot but think that a vast many of these newly-imported terms—naturally enough imported through our free intercourse with German literature—which we daily have in use, do not invariably convey the same ideas to all of us. We may perhaps even go so far as to say, that there are not a few members of the profession who find these expressions altogether beyond them. For all of us, therefore, a clear defining of them would be very acceptable; pre-supposing always that these things are necessary and great additions to the natural poverty of our mother tongue.

A new study, such as microscopy, naturally required a new terminology for the expression of its facts; we have adopted particular modes of expressing those facts, and therefore we consider the time has arrived that a distinct and clear explanation of the exact value and meaning of the terms adopted should be decided upon. We hope some one may avail himself of the hint here thrown out. It is quite certain that there is at present no reference to which the English reader can turn for assistance when he stumbles against these surprising expressions. Zylography, involution, evolution, retrograde development, abnormity, pionæmia, hyperinosis, obsolescence, osteoporosis, decadence, and such-like terms, naturally puzzle the reader when he first meets with them; and after all he only gets at their meaning in a roundabout way, and through much tribulation. And the same may be said of even plain and simple (in a comparative sense) terms, as exsudate, homœoplasiæ, crasis, dyscrasia, blastema, exulceration, an extravasate, dendritic, collateral filial vesicles, an heteroplase, denticulation, qualitative alienation, and like affiliated words, which are to be met with in every page of the pathological works now rendered classical amongst us under the auspices of the Sydenham Society, and otherwise.

Dr. Foerster's name has already been made known to the readers of this review. He is a careful and good observer; and his judgment sound and practical. His present work 'On General Pathology,' which we are now considering, is evidently modelled on the same plan as that of Rokitansky's. The objections which we should make to it are similar to those which seem applicable to Rokitansky's former editions of his 'General Pathology.' It is more diffuse than necessary, and the most important particulars, consequently, less readily tangible than they should be, and, in fact, obscured by unnecessary details. It is also especially defective in the absence of accompanying illustrations; the references which he gives throughout his volume to his 'Atlas'—another separate work—'of Microscopical Pathological Anatomy,' is very inconvenient; and, as we have before remarked, no student can be expected to fathom the depths of the history of fibres and cells, and come to an understanding of them, by mere verbal description.

The opinions and views of Foerster seem, generally, to be those of Rokitansky; but for clearness, conciseness, and the general purposes of the student, we can in no way compare this volume of his to Rokitansky's present edition of General Pathology.

The following general description of carcinoma, given by Foerster, is a fair specimen of his mode of dealing with the subject:

> "Carcinoma is characterized generally, as a new formation, by an unlimited production of cells, which do not belong to any other pathological, or any perfect or embryonic normal, structure, and which never form permanent structures; these cells are generally held together by an alveolar stroma, consisting of areolar tissue and bloodvessels. Every new formation consisting of such a stroma, and of cells which have not the nature of cartilage-, bone-, epithelial-, glandular-, muscle-, and nerve-, cells, nor of the cells which appear in sarcoma, granulations, and pus,—belongs to the class of carcinomata; if some of the above cells do occasionally resemble the cell of carcinoma, still they differ in this, that they have a typical arrangement and a limited growth."

The unlimited growth of the cell, and its not belonging to any typical form, mark the cancerous nature of the growth much more than any particular characters of the cells or its nucleus. Generally speaking, the stroma and its alveolar form are characteristic of cancer; but occasionally these may be entirely wanting.

> "Excepting the above, cancer possesses no essential or characteristic marks by which it can be distinguished with absolute certainty from other new formations. As a rule, however, cancer is remarkable for its mode of propagation, both locally and generally: it spreads locally, by absorbing into itself the surrounding parts; generally, it presents itself primarily in several organs at once, and it spreads into the lymphatic glands into which the vessels from the affected part run. Another peculiarity is the tendency of the tumour to penetrate to the surface, and then to soften and degenerate. These latter peculiarities are not constant, and to be observed in other new formations, though much less frequently."

All authors are indeed now agreed that there is no *one* characteristic sign, either in its anatomical construction or mode of progress, by which cancer can be distinguished. No one can say with certainty that such a cell is a cancer cell, or that such and such a mode of growth is absolutely the mode of growth of cancer only; for to the cell itself, and to every one of the particulars of cancer growth, exceptions might be taken in this respect.

> "A specific character," says Wedl, "has often been ascribed to the cells existing in cancer. Their excessive size, their breadth, the voluminous *nucleus*, the large prominent *nucleolus*, &c., are stated to afford positive characters; so much so, in fact, that Lebert says that even an inexperienced observer will be enabled to recognise them. But we are entirely in accord with Virchow, who denies the specificality of cancer cells. For comparative study, we would recommend the transitional epithelium on the ocular conjunctiva, the epithelial cells of the tubuli uriniferi in Bright's disease, many kinds of ganglion cells, the newly-formed cells in the gelatiniform uterine mucus, in many gelatinous exudations, chronic ulcers, soft uterine polypus, in the epidermis of condylomata, warts, &c. Unprejudiced observation will then satisfy any one, that he would but too often be the victim of delusion in laying too great a stress upon the value of the supposed characteristics of the so-termed cancer cells." (Transl. p. 526.)

It may be true that, at the present moment, the line which distinguishes malignant from other growths is not clearly defined; but still it is very certain that, in practice, the difficulty is one which rarely troubles us, and the question may be looked upon rather as one suggested by microscopic subtilty than as arising from any necessity for its absolute solution felt by the practitioner. Our ideas as to the benign or malignant nature of a growth are founded on its mode of progress, not on its anatomical construction alone. Thus, whatever its microscopical characters, every tumour must be set down as malignant which possesses the following characteristics:—constant progress; returning after extirpation, not only in its original seat, but also in distant parts of the body; destroying and absorbing the parts or organs it invades; infiltrating the tissues around; and affecting the glands to which the lymphatic vessels lead from the seat of disease.

The fact is, that in the majority of cases malignant growths offer a peculiar anatomical construction, but that they do not do so invariably; and, on the other hand, growths which have in a marked degree that peculiar construction, do yet not present the characters of malignancy in every instance. From this it follows, to use the language of Dr. H. Jones, that "the characteristics of cancer result from the invisible qualities of the new formation, its mode of vegetation, dissemination, and reproduction, not from any peculiarity of form or arrangement of its particles."

To return to Rokitansky. We have said that his first volume has undergone a general revision; and as a proof of this, we will here introduce a condensed account of the history which he gives of tubercle, requesting our readers to compare it with that to be found in the previous edition of his General Pathology. We shall also touch upon some other interesting facts of general pathology here treated of, and shall trust, by so doing, to call the attention of our readers to the excellence of this new volume of Rokitansky's work on Pathology; his improved manner of dealing with his subject; and the many modifications which his opinions have undergone.

The chapter on Tubercle is entirely remodelled, and many of the ideas concerning it which he formerly asserted have undergone revision. Words like fibro-croupous tubercle, tubercle-pus, fibrinous tubercle, tuberculizing new-growths, dyscrasial character of the blood—these all, and the ideas associated with them, have happily entirely disappeared; and in their place we have a plain unvarnished history of tubercle, clear, and adapted to ordinary comprehensions. Tubercle here is simply tubercle, and not half-a-dozen other things besides. It takes its name from its external form, and is found as small nodules, isolated or grouped together, and as large irregular masses widely spread over and through the textures of an organ. Its essential character is that it is incapable of further development, that it tends to degenerate, and thereby to the destruction of the tissues around it. However alike to tubercle in its external form and original constitution any abnormal product may be, if it exhibits a trace of fibrous development it is not tubercle.

Tubercle appears under the form of grey or yellow tubercle-masses. The nature of these, as we all know, has been much discussed. In form, the two kinds of tubercle resemble each other: roundish, granulations, of about the size of millet seeds, isolated or grouped together in irregular nodular masses. *Grey tubercle* is of uniform consistence, toughish or softish, compressible, and of pearly-grey colour. It is composed essentially of nuclei, $\frac{1}{125}$ to $\frac{1}{100}$ of mill. diam., held together in an adhesive binding medium; besides these there are present, cells containing one or more nuclei, which indicate an endogenous development and growth of the before-mentioned elements. *Yellow tubercle* is of various shades of colour, opaque, friable, and of cheesy lardaceous consistence. It contains a large abundance of fine protein molecules; among which are present the elements of grey tubercle, shrivelled, indented, and wrinkled, and of a yellowish lustre.

In what relation do these kinds of tubercle stand to each? Is yellow tubercle originally distinct from, or a metamorphosed condition of, grey tubercle? To this Rokitansky answers: Grey tubercle is without doubt converted into yellow tubercle, as Laennec taught; certain other abnormal products, not tubercular, may, it is true, undergo the same metamorphosis, but still the metamorphosis is most peculiarly related to that formation which originally appears as grey tubercle. In so far as this metamorphosis occurs at an early period of the disease, and advances rapidly, it might seem as though the tubercle was, in its origin, yellow and opaque.

Tubercle is developed in the following manner:—A clear, or turbid, greyish-red, adhesive, synovial-like exudation is poured out into the parenchyma, for example, of the lungs. This is the *albuminous tubercular infiltration* of Laennec. In this exudation spring up at separate points grey granulations, congregated together in little groups; these, at first soft and moist, gradually become condensed and resistent, or the texture in the part corresponding to the infiltration becomes uniformly swollen, and gradually assumes a fine granular structure, representing *tubercular infiltration of the lungs.* Minute investigation shows us, that in this infiltration the elements of tubercle are developed. On serous membranes we find the tubercle deposited as fine, vesicle-like, softish, greyish or opaque, discrete granulations, forming, when very numerous, a continuous nodular-like layer of granulations. These are to be distinguished from tubercle developed in pseudo-membranes. Whether the tubercular elements arise in the exudation as a free blastema, or whether they are developed in the parenchymatous blastema, is uncertain.

Grey tubercle sooner or later is metamorphosed into yellow tubercle; in some rare cases the granulations become obsolete—that is, are converted into hard, horny nodules. The metamorphosis generally commences at the centre of the tubercle, perhaps because there the growth is of most ancient date. The yellow tubercle undergoes further changes. Its solid parts become softened down into a tolerably uniform, creamy, purulent-like fluid, in which are found an immense number of fine granular points, and some scattered elements of yellow tubercle. This is to be distinguished from the thin, whey-like, flocculent fluid, known as tubercular pus. This softening is a further spontaneous degeneration of yellow tubercle, and is different from the mere disintegration of the tubercle mass under the influence of the surrounding effusion; for it commences in the centre even of large masses. Tubercle has no vessels proper to it; but the remains of vessels and other elements of the texture invaded by the tubercle may be found in the softened mass.

Inflammation plays an important part here, in so far as it occasions an exudation into the parts around the tubercle mass, which exudation serves as a basis for the further regeneration of tubercle. Likewise inflammation supplies the materials which form the fibrous thickenings and capsules around the tubercular cavity or ulceration. The separation of the tubercle-mass from the textures around it, is also often occasioned by inflammation; the elimination of the separated mass produces a cavity or ulceration: tubercular phthisis.

The softened tubercle sometimes undergoes *Cretification*—that is, it is converted into a greasy, fatty, chalky mass, which is gradually hardened into a cretaceous substance.

In its mode of development, tubercle offers important differences. 1. It often arises in a slow and insidious manner, in the form of distinct granulations confined to one portion of an organ; these gradually increase in size and number, destroying the textures they invade; or they suddenly and rapidly increase, as in the following mode of growth. 2. In this case, the tubercular granulations are rapidly and extensively deposited, and are not confined to one organ. The tubercles thus deposited are of uniform size (miliary tubercles), and regularly distributed through the tissues. They are found, in different stages of development, at different parts of the same organ: those of recent growth soft and gelatinous, those of older date firm. The tissues thus invaded are softened and infiltrated with a thick serous effusion. Tubercular disease of this kind appears as an acute disease, with typhoid phenomena: *acute tuberculosis*. It is rarely a primitive form of tubercle. 3. The third form is *infiltration of the tissues;* it appears, as seen in infiltration of the lungs, under the form of a thick, synovial-like fluid (*albuminous infiltration*), which gradually degenerates into a firm, greyish-red, granular, broken-down mass, containing portions of tissue within it. The nature of this effusion, the rapid development of the tubercle in it, its lobular form, and the growth of areolar tissue in it, are facts which favour the opinion of the inflammatory origin of this form of tubercle. This tubercle is generally metamorphosed into yellow tubercle; it runs a rapid and acute course, softening and destroying the textures which it surrounds.

The peculiarities of tubercle indicate that it is an especial anomaly of nutrition, which is frequently associated with a particular organization of the individual. It excludes the co-existence of other diseases. Cancer very rarely co-exists with it, and the especial parts attacked are different in the two diseases. Typhus, heart diseases, certain lung diseases also—emphysema, for instance—rarely co-exist with it.

Tubercle attacks nearly every part and organ of the body, and even new growths, but with marked difference in degrees of frequency: the lungs most often, then the intestinal canal, lymphatic glands, larynx, serous membranes, brain, spleen, kidneys, liver, bones and periosteum, uterus, testicles, and spinal cord. Tubercle very rarely, if ever, affects the salivary glands, the ovaries, the bloodvessels, the œsophagus, and the vagina. Moreover, tubercular disease of the intestines, of the larynx and trachea, of serous membranes, of the spleen and liver, is seldom other than secondary, and therefore of minor import.

In every organ, there is one particular part especially prone to the attacks of the tubercle: for example, the apex of the lungs, the pia mater about the pons Varolii, the grey substance of the brain, the spongy part of the bones, the lower part of the ileum, &c. The tubercular disease rarely spreads from the larynx into the pharynx. It attacks the old and young, but especially adolescents. It is generally fatal, and destroys life by injuring the functions of organs, through ulceration, marasmus, and destruction of tissues; and when acute, with symptoms of blood disease. Tubercle may undergo cure by obsolescence, cretification, and elimination; but such cure avails not the patient, if new tubercle is continually deposited. Cure is effected by means of a firm capsular scar.

Such is the account, in a condensed form, given of tubercle by Rokitansky. It is plain and simple. Many of the opinions the author formerly held concerning tubercle will be found altered or modified. Any one who will compare this with the account given in the former edition will see how completely the subject has undergone revision.

The entire subject of organized new growths has been new-cast, and modified in many important particulars, by Rokitansky. We shall therefore take a rapid view of the author's present views concerning these new growths and tumours, and point out the modified arrangements which they have undergone in his hands. The interest which attaches to the subject is great, and well warrants us in throwing all the valuable light that is available into the obscurity which, in many particulars, still surrounds it.

Fibrous tumour is that which Rokitansky formerly called *gluten-yielding fibroid tumour*. It is distinguished from other areolar-tissue formations by its well-defined isolation in the tissues where it is seated, and from which it may be peeled out. Its chief seat is the uterus and its appendages.

Sarcomata differ from fibrous tumours through not being well-defined and circumscribed. Though frequently surrounded by a species of areolar capsule, they are nevertheless generally intertwined into the textures of the organ, so as not to be removed without injuring these. They are found in the areolar tissue; in fibrous membranes, as on the dura mater, peritoneum, periosteum; between muscles and their tendons; in bones, particularly the facial bones; in the mammary and parotid glands; in the testicle, ovary, and the brain. They frequently appear at an early period of life, and are generally solitary, and, as a rule, completely curable by extirpation. They often decay, through the inflammation and sloughing of the membrane around them, whereby they are left denuded. They never ossify. They are usually formed both of embryonic-gelatinous and of fibroid-areolar tissue; but sometimes one and sometimes the other of these elements prevails, and then the tumours are distinguished as *gelatinous sarcomata* or *fibroid sarcomata*.

Gelatinous sarcoma is represented—1. By the collonema of Müller: a soft, gelatinous, tremulous, semi-transparent, greyish-yellow, non-vascular mass, containing fibres; and 2. By growths more firm and resistent; the gelatinous part being more consolidated, undergoing either conversion into enchondroma or into fibrous texture, whereby the gelatinous sarcoma is converted into fibrous tumours. Bloodvessels often abound in this sarcoma. Fibrous sarcoma is distinguished from fibrous tumours, and especially from those of the uterus, by its succulence, and the diminished density of its stroma. Its seat is especially fixed in sub-mucous, fibrous, and muscular textures, forming so-called fibrous sarcomatous polypi, which in form take the shape of the cavity into which they intrude. These are very vascular. Under the sarcomata may also be included the *neuromata*, the *irritable tumour* of Sir A. Cooper, and *epulis*.

Papilloma is a papillary form of growth, of gelatinous areolar substance; it is seen particularly on the skin and some of the mucous membranes; and also in textures where there exists no natural papillary formation. It is simple or compound, having a cauliflower form. Its nature is either benign or cancerous.

New formations of elastic tissue are found, either alone or mingled with the former-mentioned growths. Pleuritic false membranes not unfrequently represent the pure elastic tissue.

Cartilage and bone-formations follow next, and the account given of them is much the same as that given formerly.

Fat-formations.—Small lipomata not unfrequently disappear, and leave no trace behind them. In larger ones, there sometimes occurs an absorption of the fat-cells, the areolar tissue being left; and in old fat-tumours, chalky mortar-like masses are often found in the place of the fat-cells. Fat-formations are no longer arranged under the titles of normal and abnormal fat-textures, and free-fats; but they are now described as obesity, lipoma, and free-fats. Cholesteatoma is removed from this division, and is now placed under the head of *epidermic formations.*

New formation of muscular tissue.—Cell-fibres, which resemble more or less organic muscular fibres, may be observed as constituents of different abnormal growths; but true organic muscular fibre of new formation can exist only in the form of muscular hypertrophy. New growths of striped muscular fibres have been often described by observers; but the fact is still matter of doubt.

New formation of nerves.—Virchow has seen nerves in false membranes of the pleura and peritoneum; and Rokitansky has observed an independent nervous apparatus springing from a ganglion in an ovarian cyst. Both he and Virchow have frequently noticed a new growth of grey cerebral matter extending into the ventricles in the form of small tumours.

New formation of bloodvessels.—This chapter is entirely re-written, and much enlarged. Our author's views have undergone considerable change concerning the mode of formation of the vessels; obscurity, however, still hangs over the subject, although much information has been added to our knowledge since the former edition of this work was published. Several interesting memoirs, named at the head of the chapter, have lately appeared, and by these Rokitansky illustrates anew the subject:

> "It is matter of certainty," he formerly wrote, "that such new vessels by no means originate through any prolongation of pre-existing vessels in the contiguous textures, but that the new process of development is altogether an independent one, and that only at a later epoch do the new-formed vessels enter into anastomosis with the older ones."

He now says that it is most probable the bloodvessels of new growths arise through the prolongation into them of the original vessels of the membranes, &c., from whence the new growths are derived. Thus, in the false membranes of serous sacs, the bloodvessels extend from the original vessels of the sac, and increase and grow towards each other either as solid fibres or as tubes with club-shaped extremities, which eventually open into each other, and probably by dehiscence; and thus vessels developed in the visceral and parietal layers of the new growth of serous sacs, when the surfaces of the sac become united, anastomose with each other. In this manner also, in all probability, arise the vessels of other new growths, such as fibrous tumours; and in part, also, those of carcinoma. Vessels also may be formed by the union of cells in the new growth, whereby a tube is formed; but the mode of union has yet to be explained. These vessels differ much in their structure, size, and mode of distribution.

The development of *blood* in new growths seems more improbable to us now, inasmuch as we consider the chief source of the new growth of bloodvessels to be derived from pre-existing vessels; but nevertheless, the elementary development of blood-corpuscles in certain structureless hollow growths, must be admitted. Numerous observations have shown that there are certain cell-like forms which contain unmistakeable blood-corpuscles: thus, a group of such corpuscles has been observed, surrounded by a structureless membrane; a development of them has likewise been seen within certain nucleated cells.

The pros and cons of this subject are stated at length by Rokitansky and illustrated by original observations; to these we must draw the attention of the reader, warning him not to quote the English form of Rokitansky's volume as that which contains his present opinions. Under this same chapter are included *vascular tumours, telangiektasis, cavernous blood-tumours*, concerning which new and original details are given, which will prove of especial interest to the surgeon. Our author also refers to a *development of vascular canals* which takes place through absorption of the new formation deposited on the inner surface of an artery, whereby a system of canals is formed, which open by fine mouths into the artery, and are filled therefrom.

Passing over the chapter on pigment-formations, which is re-written, and the chapter on new growths of the external skin, mucous and serous membranes, and of epidermic and hair-formations, and on glandular growths, we come to *cyst-formations*, a subject full of practical interest. Rokitansky here also tells his tale anew; he has condensed his matter, re-arranged it, and brought it into relation with our present knowledge of the subject. What is a cyst? It consists essentially of a closed membranous sac, generally lined with epithelium, and containing semi-fluid matters. Capsules surrounding tubercle, exudations, &c., are excluded from this category. The contents of the sac vary much. 1. We have cysts with serous, synovial-like contents; hydatids, ganglionic cysts, hygroma; such are the commonest kind. 2. Then colloid cysts, melliceris. 3. Fat-cysts next, cysts containing fat in the form of margarine, elaine, buterine, &c. 4. Cysts filled with nuclei and nucleated cells, epithelial cells; with a dendritic vegetation, with areolar tissue, or with an abnormal or normal parenchymatous tissue. Milky fluids and milk are also found in cysts situated in the mammary gland; spermatozoa in cysts about the spermatic cord, &c. The original contents, whatever their nature, moreover, frequently undergo considerable changes in the cyst.

Cysts, again, are simple-one-cysted, or compound-many-cysted: of the many-cysted sort there are several varieties. Next follows a description of the mode of growth and anatomical constitution of the dendritic vegetations, the excrescences, flattened, branched, pedunculated, cauliflower-like, villous, which frequently grow on the inner surface of cysts.

We now come to a most important consideration—viz., the origin of these cysts. 1. They may result from a process going on around serous exudations, &c., in consequence of which the pre-existing or a new-formed areolar tissue is converted into cyst-walls surrounding the exudations. Of this sort are the cysts formed at the parts of limbs exposed to pressure subsequent to amputation, synovial cysts, &c. 2. They may be produced from pre-existing physiological structures, as from incomplete closure of the processus vaginalis upon the spermatic cord: as diverticula also of serous and mucous membranes. They also arise through dilation and hypertrophy of the tissue around a follicle resulting from accumulation of secretions within it, in consequence of obliteration of its excretory opening; inflammation sometimes being the exciting cause. Of this kind we have examples in the follicles of the ovary, the vesicles of the thyroid gland, the Malpighian capsules of the kidney, the mucous glands, as at the back of the larynx, &c. The contents of such cysts frequently afford no trace of their original constitution; the bile or urine, &c., are absorbed, and are replaced by mucus or serum, or metamorphosed into colloid or cholesterine matters.

3. Cysts, again, arise as new formations. They grow as vesicles, with a fibrous alveolar structure. They may be simple or compound; and in relation to their vesicular origin, often attain a considerable size, seldom, however, exceeding that of a walnut: generally speaking, they are about the size of a hemp-seed. They occur frequently, congregated together as microscopic forms or otherwise, in the cortical parts of the kidneys, in the broad ligaments of the uterus, in mucous and serous membranes, in the brain, in the mammary gland, in sarcoma, carcinoma, and dendritic vegetations. They contain a serous, synovial fluid, which often degenerates into colloid and resinous-like masses; in the fluid are found granules, nuclei and nucleated cells, simple structureless vesicles, colloid particles, fat-cells, cholesterine, &c.

These cysts also arise out of a new-formed fenestrated growth; the walls of the loculi of the compound fenestrated growth uniting together, and forming enclosed spaces. True cystoid tumours represent such formations. Like fenestrated growths in general, they vary in size, reaching even that of a walnut; originally, however, they are, as a rule, microscopic objects. Thus they are situated at one time in a fine microscopic areolar tissue; and at another in a fully-developed fenestrated growth; very commonly they are placed in the middle of a large fenestrated growth, which spreads out with an uneven nodular surface in all directions.

In the fenestrated growth a loculus may be developed, so as to form a parent cyst; or again, a loculus in the inner layers of the fenestrated growth may so expand as to intrude into the space occupied by the parent cyst, forming thus a secondary cyst, which process again may be repeated by a tertiary cyst. More frequently, however, the pre-existing fenestrated growth around grows in mass into the cyst, forming a tumour having a neck or a broad basis; which tumour, again, may give rise to a development of closed loculi and cysts. Sometimes the number of these is so great, that they completely occupy the cyst, even when of considerable size, pressing against and flattening each other.

There is another kind of cystoid growth found, particularly in the ovary, and there often attaining a very great size; it contains in its loculi and cysts a thin serous fluid, mucus, a gelatinous or a medullary mass—then representing the largely-developed stroma of a gelatinous or a medullary cancer. Some of the loculi are fat-cysts also. Another growth of this kind is developed in the sub-mucous areolar tissue, or in the parenchyma of mucous membranes, in the form of a broad roundish swelling, or pedunculated, distended with a tough mucus, dehiscing at its periphery, and displaying a fenestrated growth, containing numerous various-sized loculi. It is found especially in the uterus and in the stomach. Its contents frequently degenerate into colloid matter.

Then there is a pedunculated, hanging, bag-shaped, simple or pouched cyst. It is found on the walls of cysts, on the mucous membrane of the bladder, and is generally filled out with areolar tissue, of the fine trellis-work form. It seldom exceeds in size a pea or bean.

4. The last division of cystic forms is a combination of cysts with other new formations, as with sarcoma or carcinoma.

As a summary, we find that—The number of cysts in an individual or in an organ varies indefinitely; there may be one cyst in the organ, or the organ may contain an aggregation of cysts. The form of a single cyst is ordinarily roundish; that of aggregated cysts very various. Their structure may be gathered from what has been said above. In the cystic diseases of mucous canals, the mucous membrane and muscular elements disappear as the walls of the cyst expand. There is scarcely an organ or a tissue in which cysts have not been observed. Most commonly, we find them about the peritoneal coverings of the female sexual organs, in the ovaries, then in the kidneys, thyroid and mammary glands, in bones, mucous membranes; less seldom, indeed rarely, in other organs—liver, spleen, lungs, &c. They are rarely congenital. Their nature is benign and malignant. Hæmorrhage frequently occurs in them. Hyperæmia and inflammation also attack them. Sometimes they ossify.

What we have here given of the history of cysts will suffice to show how totally the subject has been revised by Rokitansky, and is a tolerable specimen of what has been done in many other parts of this volume; demonstrating, we think, pretty clearly, that the present English form of his work must no longer be taken as the exponent of our author's opinions.

Let us now take a condensed view of our author's account of carcinomatous tumours.

By malignant, Rokitansky understands those growths which, either of themselves or consequent upon extirpation, multiply and are reproduced in the neighbourhood of their original seat, and also in distant and different organs; giving rise to marasmus through their actual increase and nutrition, not through any hindrance which they occasion to the function of the organ they invade. Their constitution is heterologous; they are infiltrated masses which spread out into and destroy the textures around them; having a marked tendency to suppuration, whereby the growth is not only increased in the textures around, but is generated in distant parts, particularly in the lymphatic glands. It must be remembered, however, that every malignant growth does not present these peculiarities; and that, on the other hand, certain benign growths exhibit many of them.

Cancer is a malignant growth, in the sense here given. It consists of two parts—viz.: 1. The peculiar *cancer mass*, which is formed of nuclei and nucleated cells, of the most varied natures, distributed through an inter-cellular medium; and 2. Of areolar tissue, which constitutes the *stroma*. The first is the most essential, and the heterologous part of the cancer. The relative quantity of these parts present in a cancer gives it its character; when the cancer mass abounds, the tumour is *medullary*, &c.; and when the stroma abounds, it is *fibrous*.

The *stroma* takes very varied forms; it is arranged as net- or trellis-work, having larger or smaller interspaces; and also as papillary, villous, or more or less ramified, vegetations. The fibrous stroma sometimes undergoes ossification, forming a network of true bony texture. Occasionally the stroma is absent altogether, and then the cancer is formed wholly of nuclei, nucleated cells, and their connecting medium. The stroma must be carefully distinguished from the areolar tissue proper to the organ in which the cancer is developed; and so also ossified stroma from particles of original bone present in the cancer-growth of bones.

The cells of the *cancer mass* vary much. Some are homœoplastic, as in epithelial cancer. The cells frequently grow to mother cells; the nuclei to structureless, sterile, or prolific vesicles (bruterzeugende Blasen). Although the cells of the cancer mass offer in themselves nothing characteristic, they are often remarkable for their size, and especially for the size of their nucleus and its nucleolus. The cancer mass, however, sometimes consists merely of small, pus-like cells, and often, indeed, of nothing but nuclei. Some cancers are very rich in bloodvessels, some very poor.

As the blastema of cancer, we not unfrequently find in cancerous formations an albuminous, synovial like, colourless or pale-yellowish fluid, contained either in the interspaces of the stroma, or collected in a foyer. It gives rise both to the materials requisite for the development of the elements of the cancer juice, and to the essential cancer-mass, in which the stroma grows. The blastema of cancer is, for the most part, insensibly produced, but its production is occasionally attended by inflammation and stasis. The development of cancer, therefore, generally takes place as an exudation into the interstices of textures, and on to the free surfaces of membranes; it may probably also arise from endogenous growth within certain cells, as bone-cartilage, &c., cells.

Cancer is also developed within the bloodvessels, out of fibrinous coagula. Also we find widely-branching cancer masses in different veins, cauliflower masses in the vena cava, in the right side of the heart, either loosely attached to the internal membrane or firmly connected with it, being perhaps produced out of fibrinous coagula. It is very probable, also, that cancer arises, in an acute form, in parenchymata, through infarction of their capillaries.

Hypertrophy of neighbouring organs generally accompanies the formation of cancer. The size of the cancer varies much, as likewise the number of parts affected. The size and the number of tumours present seem to stand in an inverse ratio to each other; solitary cancers often attain a very large size.

Cancer is rarely cured by extirpation, generally returning either in the part or in other organs of the body. There are certain organs, such as the lungs, spleen, salivary glands, small intestines, and serous membranes, which are rarely ever attacked primitively by cancer. The most ordinary sites of primary cancer are the uterus and female breast, the stomach, the colon, the liver, bones, and brain, &c.

Particular kinds of cancer affect particular organs; alveolar cancer, the stomach and intestines; epithelial cancer, the skin and mucous membrane. Certain organs are usually affected simultaneously; as the uterus and ovaries, the testicles and kidneys, the stomach and intestines; the spleen with the liver.

Extravasations of blood often accompany cancer through rupture of the bloodvessels.

Cancers, when they come in contact with the air, suppurate; and in some rare cases may thus undergo a cure.

Having said thus much of the general characters, we must pass over any particular notice of the different kinds, of cancer. We have already referred to the different divisions which Rokitansky now makes of these tumours. Generally speaking, as indeed we might conjecture, the alterations introduced into this part of his pathology are not so extensive as in many other parts; though the subjects have been mostly re-written, and many additions made to them. The chapters on Epithelial and Colloid Cancers have, in particular, been thus improved and extended.

A new chapter on the Diseases of Tissues is added to this volume. It gives an account of the changes which occur in the normal constitution of the elements of textures; the resolution of the elements into molecular detritus; fat metamorphoses; colloid and cellulose metamorphoses; ossification and incrustation; obsolescence. The chapter which follows this, on the Anomalies of the Contents of Parts, has undergone but little change.

The last part, containing the anomalies of the blood, has undergone a thorough reform. The long and complicated history of the various crases of the blood detailed in the former edition now no longer exists here. It will surprise, we fancy, those of our readers who are acquainted with Rokitansky's ideas, as portrayed formerly, to hear that the word crasis or dyscrasis is under this head not to be found. He gives, instead of those many pages of blood-crases, a simple account of the qualitative and quantitative abnormal states of the blood, reducing these under the heads of anomalies in the quantity of blood; anomalies in the quantity of blood-corpuscles; anomalies of the fibrine; diseases of the blood-corpuscles; pyæmia; foreign bodies in the blood. One or two particulars we may refer to. Under anomalies of the blood-corpuscles, we have an account of the anomalies of the colourless corpuscles. The colourless corpuscles are increased—1st. With simultaneous increase of the red corpuscles, as in typhus, the exanthemata, in puerperal states, ague, marasmus, and extensive atheromatous disease of arteries. 2ndly. With increase of fibrine, the coagulum of the blood drawn being opaque, whitish, or yellowish; and 3rdly. The colourless corpuscles are so increased that the red colour of the blood is nearly lost, so that it at last becomes an opaque whitish fluid, representing leucocythemia (Bennett). The coagulum then found in the body after death is whitish-yellow, or greenish, like congealed pus, sticky, and softish, here and there studded with streaks of red corpuscles; it reaches generally a long way into the arteries from the heart. The increase of the colourless corpuscles gives rise to an increase of the fibrine of the blood, to stasis and exudation, and particularly to coagula in the vascular system in the heart under the form of numerous vegetations. Rokitansky considers that the colourless corpuscles may be formed in the blood, within the vessels; and does not allow their only source to be in the spleen and lymphatic glands, and particularly in the plasma of inflammatory depôts.

Inflammation and purulent deposits of the lungs, he observes, the skin, cellular tissue, the spleen itself, &c., are often observed in leucocythemia. Many of these are recent, caused by and consequent upon the leucocythemia. The spleen tumour also often shows marks of inflammatory enlargement.

The leucocythemic blood resembles so completely the pus contained in exudation-fibrine, that the more often we meet with it in connexion with purulent deposits, the more inclined are we to regard it as a pyæmic condition. Leucocythemia is, in fact, to be distinguished from ordinary pyæmia arising from absorption of decomposed animal matters; it would rather represent the bland condition of laudable pus.

One other very interesting point our author here also touches on somewhat at length—viz., the coagulation of the blood within the vascular system. In every part of the vascular system coagulation may occur. In the heart—the only part we shall here refer to—we find it as a roundish mass, or as a membranous expansion, winding around in all directions among the trabecula of the ventricles; or again, as a villous appendage to the endocardium. The coagulum varies in colour according to the amount of red-corpuscles present; it may be formed in layers, or consist of an uniform mass, and increase often in the direction of the current of blood; forming fibrinous tumours of the heart.

The causes of these coagula are many; they reside partly in the blood, and partly external to it; frequently the causes, both within and without, work together in producing them. 1. *Mechanical causes* act by causing stagnation of the blood, as in the thrombus arising from ligature, and coagula through weakened action of the heart, &c. Under this head also must be included roughnesses and false membranes on the inner surface of the heart and the vessels, rupture of the valves, &c.; it is of great importance, however, not to mistake these growths in the endocardium and the valves, &c., for coagulated fibrin. Then, again, small particles of fibrin, or of the valves themselves, or of other textures, carried into the capillaries from the cavities of the heart, cause capillary infarctus. 2. *Inflammatory exudations* taken up in the blood, as in inflammation of the endocardium, of the arteries and veins, also occasion obstructions primarily and secondarily. 3. Lastly, there are conditions of the blood itself which give it a tendency to coagulate, conditions which probably arise in the colourless elements of the blood.

Before closing this notice of Rokitansky, we must say a few words on the subject of Inflammation, as now described by him. Here our author again has made great changes. His description of the phenomena attending the inflammatory process is reduced from several pages to half a dozen lines. Of *dyscrasial* inflammations he has no word to say.

Inflammation, we find, is an abnormal process of nutrition, in which an exudation takes place consequent to *stasis* of the blood. The following phenomena characterize the particular stages of it. 1. *Hyperæmia;* accumulation of the blood-corpuscles in the capillaries, retardation of, and an oscillatory movement in, the blood-current. 2. *Stasis;* stagnation of the blood, cleaving together of the red corpuscles, and increase of the colourless corpuscles. 3. *Exudation of blood plasma.*—Our author shortly relates the different theories which have been given as explanatory of the phenomena, but seems himself disinclined to become entangled in the wide and labyrinth-like discussions to which their investigation inevitably leads. He wisely confines himself to the detail of what observation teaches.

The *stasis* is the most important part of the inflammatory process. It consists essentially in a cleaving together and accumulating of the red corpuscles in a plasma which has become thickened through a previous transudation of the blood serum from out of the vessels, and in an aggregation of the white corpuscles, the blood assuming at the same time a dark brownish-red hue. The exudation may be explained (independent of all molecular attraction) by the pressure to which the plasma in the vessels is exposed through the stasis; and it is favoured also by the co-existing distension of the coats of the vessels, and consequent increase of their permeability. *Œdema*, serous effusion, takes place around and to a certain distance from the central point of inflammation; the further the effusion is from the central point, the poorer is it in plastic constituents.

The next stage of the process succeeding the exudation is restoration of the blood-current, whether in consequence of their returning contractility, or through the cessation of the temporarily excited contractility and narrowing of the arteries leading to them, and therefore through the impulse transmitted from them.

Inflammation varies in many particulars. 1. It varies in its degree of intensity; a most important fact, directing us to a right knowledge of the rank of the inflammation in the anomalies of nutrition. By the quantity and quality of the exudation we best measure the intensity. Some kinds of (so-called) inflammations reach not the stage of true stasis; they are rather protracted hyperæmiæ tending to stasis. Such are mostly chronic in their course; their exudations being serous, poor in plastic materials, and irregularly poured out. Other inflammations there are, acute in progress, rich in plastic exudations, and marked by a highly developed degree of *stasis*.

2. Every hyperæmia may end in stasis. Here, we may mention *hypostatic inflammations* occurring in dependent parts of the body in the course of adynamic diseases—the so-called asthenic inflammations. The hyperæmia and stasis are characterized by a dark livid redness, the result in part of imbibition, and in part of vascular injection; the exudation is coloured by blood-colouring, and poor in plastic matters. Mortification is their not unfrequent consequence. Stasis may also be caused by *mechanical hyperæmia*, and then is characterized by great swelling and dark-red colour. It readily ends in mortification, and is generally accompanied with large serous effusions.

3. In its mode of *spreading*, inflammation differs much. It may attack a part or organ at one or many points (foyers), may extend over a large surface, or invade every portion of an organ.

4. The exudation offers important distinctions both in itself and in relation to the tissues it invades.

We rarely have an opportunity of observing the exudation in its pure and original form; for directly it is poured out, a process of development, as well as other changes, commence within it. It is probable that the inflammatory stasis is not a mere simple stoppage of the blood, leading to an increased effusion of the nutritive plasma; but that it gives rise to qualitative changes in the plasma, causing it thus to differ from the plasma of healthy nutrition.

What is meant, then, by exudation? What is that which, in an inflamed part, constitutes the exudation? In answering this we must carefully distinguish *two essentially different ingredients* of the inflammation, which may be seen well marked in the case of inflamed serous membranes—viz., (1) the pure *Inflammatory exudation;* and (2) the *Tissue-formation,* which, consequent to it, arises in and out of the substratum of the inflammatory process; the pseudo-membrane of serous membranes.

In the inflammatory exudation (which, both in respect to its composition and development, stands in the most intimate relation to the process going on in the capillaries during the stasis), forms corresponding to the colourless elements of the plasma are developed. These elements are—*Elementary granules, Nuclei,* and *Nucleated cells;* and besides these, a greater or less amount of *Exudation corpuscles.* These are held either in a fibrinous network (coagulated fibrin), or in a shapeless, striped, hyaline mass resulting therefrom; or they are distributed through a fluid, intercellular matter, and occasion, when in large quantity, the whitish, yellow, or yellowish-green colour, the opacity, and the thickish-creamy, fatty-like consistence of the exudation.

These elements may be present in very small and in very large quantities, and in all intermediate degrees. When very abundant, they give to the exudation the character of pus. They are ever alike, so that what has been said as characteristic of the elementary forms of pus (hitherto unnaturally separated from other exudations), refers equally to all.

The *Cells* are round, colourless, or yellowish, containing generally granular matter; and enclosing two, three, or more nuclei grouped together, $\frac{1}{100}$ to $\frac{1}{66}$ mill. diam. The *Nuclei* are roundish, shining vesicles, $\frac{1}{200}$ to $\frac{1}{166}$ mill. diam., having a dark contour; in their granular contents, one or more nucleoli are seen. Besides these round, there are also oblong, &c. nuclei.

The *Exudation corpuscles* are vesicles (Bläschen) nearly of the same size as the cells. They have no nuclei, but enclose a fine granular matter, in which one or more nucleolar-like, shining corpuscles, with a dark contour, may be often remarked. They may be regarded as nuclei of inordinate size and growth; just as cells are seen here and there in exudation matter, which exceed the ordinary measure.

These different elementary forms are incapable of further development. On the contrary, they undergo a retrograde metamorphosis; fatty conversion, absorption, granular degeneration, &c. &c.

Pus, though hitherto improperly separated from other exudations, deserves a special consideration. It is a yellowish-greenish, thick, fatty-like, alkaline fluid; it is distinguished by the abundance of the above-mentioned elements which it contains. The relative amount of these elements differs greatly; normal pus is constituted almost entirely of cells; and then, again, pus may consist almost wholly of pyoid (exudation) corpuscles. Very commonly, also, fatty granules, ammoniacal phosphates, and sometimes infusoria, exist in it; so, also, there may be blood, mucus, epithelium, and the remains of broken-up tissues. These elementary forms float in a *pus-serum,* in which they sink when at rest. Pus undergoes several metamorphoses; fatty and calcareous changes; resolution into fine granular detritus, &c.; and conversion into a mucous-like and colloid fluid.

Inflammatory exudations are *fibrinous, albuminous,* and *serous.* Fibrinous exudations appear as coagulations (Gerinnungen), containing the above-mentioned elements in various proportions spread through a fibrinous network, or hyaline mass. Such exudations on membranous surfaces are called croupous, and are remarkable for their extent, the rapidity of their deposition, their abundance, and the exhaustion of vital power which they occasion. The *albuminous* is a clear, colourless, synovial-like exudation; or when granules, nuclei, and cells are present in it, an opaque, troubled, thickish, whitish-yellow, creamy fluid. Under this head belong purulent exudations, the blastema of tubercle, and of typhus- and cancer-masses. *Serous* exudations are either simply serous, containing little albumen; or albumino-serous, thick, synovial-like, rich in albumen, or fibrino-serous.

We have already said that the *membranous formation* which takes place subsequent to the exudation in the substratum of the exudation process, is to be distinguished from the exudation proper. *Pseudo-membranes* have been hitherto improperly considered as a part of the exudation. They result from the development of the areolar substratum (Bindegewebe Substratum) into cellular growths. In serous membranes, for example, appear round, oval, spindle-shaped cells $\frac{1}{50}$ mill. diam., with nuclei $\frac{1}{100}$ mill. diam., which grow into soft, villous layers, papillary granulations, and channelled, branched, anastomosing folds, giving to the inflamed membrane its well known dull, flocculent appearance. The serous membrane also at the part loses its fibrous texture, and assumes an hyaline, gelatinoid condition. The new vegetations at first take the form of a simple or areolar lamella, or of a trellis-work; which again gives birth to new villous and papillary cell-masses; and thus the layers are formed one over the other. The materials for this continued growth are undoubtedly essentially derived from the serous membranes through the simultaneous development of a new vascular system in it; they may, however, in part be derived from the exudation in contact with the innermost layers of the new growth.

Hence this pseudo-membranous constituent is not a plastic, organizable part derived from the exudation, but it grows out of the same substratum as the exudation, is one with it.

The same process of cell-growth takes place in connexion with purulent exudations, giving rise, as in healing ulcers and in wounds, &c., to *fleshy granulations.* These are formed of cell-layers, which grow into papillary, wart-like masses, out of which again spring new cell-masses, the deeper layers in the meantime being converted into vascular areolar tissue. The pus-formation ever present on such granulating surfaces takes its source in the exudation, which is poured out of the newly-formed vessels of the granulations. It corresponds to the exudation which so often occurs on the inner surface of serous sacs during the growth of pseudo-membranes on them, and which combines with, and increases the quantity of, the original exudation.

Hence, then, the exudation proper of inflammation, its cells, &c., are incapable of further development; pus, therefore, is an excrementitious product. New texture-forms, the results of inflammation, arise through the growth of the areolar substratum into cells, and by the fusion of these into hyaline and fibrous-areolar textures. The inflammation, therefore, can only be looked upon as an exciting cause of the new-growth, not as a necessary ingredient of it, for the new-growth may take place without any perceptible signs of concurrent inflammation. Moreover, the new-formation and the exudation hold neither quantitative nor qualitative relations to each other; the blastematous contents of the exudation, for instance, may be very abundant and the new-growth but slight, and *vice versâ;* and the quantity of the exudation is no measure of the amount of new-growth.

From the outline here given, our readers may form an idea of the new doctrines now promulgated by our author regarding inflammation, and of his new method of treating the subject. We wish that our space would permit us in like manner to run through his history of blastema, the growth of cells, and their development into tissues. This chapter is one of the most interesting in the work, but it is not readily mastered without the aid of woodcuts, and we fear would scarcely be made comprehensible by mere verbal description.

The extent to which we have already allowed our remarks to run warns us that we must bring them to a close; but we cannot do so without once more expressing our high approbation of the contents of this volume of Rokitansky's on General Pathology. We have no hesitation in proclaiming it, in our opinion, to be the best elementary treatise on general pathology with which we are acquainted; the more we have examined it, the more firmly have we been confirmed in the opinion. It is just the work which we should have desired to see produced by a man of Rokitansky's vast experience, and we are free to confess it is a work which we scarcely expected to see him produce. High science is here rendered comprehensible to, and made available for, the purposes of the practical man; the subject matter, difficult enough in itself, is not made, as too often happens in like cases, incomprehensible to the general reader, by being smothered in an overwhelming amount of learning. The author has stuck truly to his text: "Ich bemühte mich, strenge an dem Faktischen zu halten;" he has kept himself strictly to a detail of facts. To attain a knowledge of the *facts* of general pathology, the student will find no easy task.

From what we have said, we need perhaps not add, that we hope to see this work in an English dress. The publication of the former editions of Rokitansky's work exercised very great influence over the pathological literature of Germany and of this country; the publication of this edition will, we trust, exercise a like influence, an influence from which we cannot but anticipate happy and beneficial results.

W. O. Markham.

REVIEW IV.

1. *Principles of Human Physiology, with their chief applications to Psychology, Pathology, Therapeutics, Hygiene, and Forensic Medicine.* By W. B. CARPENTER, M.D., F.R.S., F.G.S., &c. Fifth Edition.—*London,* 1855. pp. 960.
2. *Elements of Psychological Medicine.* Being an Introduction to the Practical Study of Insanity. By DANIEL NOBLE, M.D.—*London,* 1855. pp. 356.
3. *On the Influence of Education and Training in Preventing Diseases of the Nervous System.* By ROBERT BRUDENELL CARTER.—*London,* 1855. pp. 438.
4. *Psychological Inquiries.* By Sir B. BRODIE, Bart.—*London,* 1854. pp. 264.
5. *The Senses and the Intellect.* By ALEXANDER BAIN.—*London,* 1855.
6. *The Principles of Psychology.* By HERBERT SPENCER. — *London,* 1855.

IT is an opinion we hear often expressed, both at home and abroad, that the English mind, absorbed in practical pursuits, has become singularly unfit for any kind of philosophical speculation. This opinion, it is necessary to admit, is not wholly devoid of reason. That there is any natural deficiency, indeed, in our mental constitution for purely speculative researches, we feel justified, even on mere historical grounds, in denying. It was from England that the scholastic philosophy drew its acutest disputants,—in England, that the inductive method first enunciated, theoretically, its great and fruitful principles,—in England, that the most systematic attempt was made (in the writings of Hobbes) to build up a complete body of scientific truth upon a materialistic foundation,—in England, that the science of ethics and the spirit of criticism received, in the seventeenth century, at once its deepest and most enduring characteristics,—and in England, finally, that an epoch was made (by the writings of Locke) in the whole procedure of psychological and metaphysical investigation.

Notwithstanding all this, however, it must be confessed that *since* the time of Locke the philosophic spirit has been, to a large extent, dormant amongst us. Individuals there have been, well fitted to remind us that the power of speculation is only slumbering; but no great national school of philosophical thought, either of one kind or another, has even begun to show itself, amongst all the mental activity of the last and the present century. Perhaps the fruitfulness of physical investigation in this country has contrasted but too strikingly with the general unfruitfulness of metaphysics; or perhaps there may have been a sort of unconscious, intuitive perception in the practical English mind, that the one real method has not yet been evolved which could give to mental researches the certitude, and consequently the value, which we have now learned to demand in all the walks of pure science.

In Scotland, indeed, a school of philosophy—based, in the main, on healthy convictions, and nursed by a sound antagonism to scepticism—sprang up, and flourished for a time as though it had found a truly congenial soil. But even this school—always timid in its character, confined in its range, and greatly wanting in the power of practical application—has now well nigh passed by. Amongst all the professors of that country, there is only one name that appears likely to go down to posterity as having added anything to the range of human thought. And even Sir William Hamilton (the author to whom we allude), though unrivalled as a critic, hardly emulated as a logician, and seldom surpassed in the knowledge of the history of speculation, has confined his efforts so much to the *formal* side of philosophic methodology, that his writings have not availed, in any great degree, to rouse the mind of his country up to any decisive effort in the way of broad and earnest reflection upon the great moral problems of the age. Scotland is still largely engaged, nationally speaking, in verbal disputations, logically as acute, but morally as fruitless, as were the battles of the middle ages.

Psychology, or the science of mind, has thus for some time occupied a wholly anomalous position amongst us. On the one hand it is felt, that a clear and comprehensive knowledge of mental operations is a great and pressing necessity. All the kindred sciences, in fact, appeal to psychology for first principles. The moralist looks here to gain light on the nature and authority of conscience; the legislator, to find some guide for estimating the degrees of criminality in doubtful cases; the educationist, for data on which to base an intelligible scheme of mental culture; the physician, for guidance in the proper treatment of insanity; and lastly, the theologian looks here for aid in tracing the essential characteristics of man's religious nature, and the avenue by which his mind can soar to the contemplation of the infinite.

With all these inquiries, however, directed to the subject—with all these eyes waiting upon it for light—the science of mind is felt to have been, for some time at least, singularly unproductive. It does *not* solve the problems of the age, or appear to give much aid in solving them. It borrows from other sources more light than it lends to them. And even when, as we sometimes see, a formal attempt is made to consult it on subjects with which it ought to stand in the closest connexion,* every one is compelled to confess that the result comes to hardly more than a series of commonplaces, which leave the whole question exactly where it was before.

Now, to judge by past experience, we should naturally conclude, that the unfruitfulness of any science (if it be truly a science) must arise from a false method of investigation. The science of nature was unproductive while pursued on the principles of the ancient philosophy; investigated by the light of the *Novum Organum*, it has become the handmaid well nigh to every human desire. It is important for us, therefore, to look for a moment at this one point, and see what precise methods of psychological research have been most in vogue during the last century.

1. *And, first, we find the purely speculative method largely followed by various of our older English writers, and still more largely by modern*

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* The Relations of Psychology and Theology. By Dr. Alliott.

German ones. The attempt which rational psychology, as it is usually called, has constantly made, is, first of all *to think ones-self,* by the pure force of reason and reflection, into a clear conception of the nature of the human soul; and then to deduce from its fundamental characteristics thus determined, the whole catalogue of its powers, capabilities, and attributes. This attempt, we may certainly say, has hitherto been quite as fruitless as was the attempt made by the ancient philosophers to *think themselves into the secrets of nature,* and then apply their thoughts to the construction of a complete natural philosophy.

We do not mean it to be inferred from this, that all speculative thinking is alike worthless and unproductive. All we mean to say is, that where a direct objective material of research exists—one to which we can have immediate access, which presents actual facts and phenomena to our view, and which can be interrogated by inquisitive observation,—it is folly to neglect these patent facts, and then to retire into the recesses of our own minds to find the laws we seek for *there.* If the subject of research be one which presents no tangible material at all; if it be something which can only be grasped and realized *in thought;* then thought may undertake to investigate it. Take any purely metaphysical question—such as the nature of time, or space, or power, or causality—and all we can do is to show the relation of such questions to the laws and possibilities of thought itself. So it is with regard to most moral and religious questions—such as the existence of a God, or the immortality of the soul, speculatively considered. Here are subjects of research which present no direct and accessible phenomena apart from the process of thought by which we realize them. Thought, therefore, must do its best to rise from the visible realities around us, or the felt realities within us, to a rational or moral certitude on such matters. But it is not so with regard to the human mind. Laying aside all hypothesis on the nature or essence of the soul, we have a series of phenomena presented in history, in our observation of others, in the facts of our own internal consciousness, that are perfectly inexhaustible as illustrations of mind, its laws, and its characteristics. The facts of sensation, of emotion, of thought, of imagination, of human action, wherever or however they present themselves, all form materials of investigation to which we are as well able to direct our powers of observation and analysis as we are to the outward phenomena of nature herself.

Convinced of this truth, many other psychologists have adopted—

2. *The purely empirical method of research.*—Dugald Stewart, for example, laid great stress upon the idea of reducing psychology to an *inductive* science. Many of the French philosophers, and their followers in England, have done the same. But in the case of nearly all these writers, the inductive method, properly so called, has been but very imperfectly realized:

> "They thought" (to use the words of Fortlage, in his 'System der Psychologie') "that to write a psychology, it was only necessary to look into the soul as into a peep-show, and to put down simply what showed itself. It was just the same kind of procedure as if, in investigating a theory of storms, we were to give as near a description of them as possible; designate all the forms of the lightning flashes, put all the rolls of the thunder to musical notes, and, above all things, add plenty

of information about remarkable and curious storms which have taken place here and there."

In these few words, Fortlage has just touched the evil and imperfection under which our empirical psychology has laboured. *Observation* there has been in abundance, but very little *explication;* classifications have abounded, some more and some less complete, but we have had very little disintegration of individual phenomena into their simple elements. Lists of mental faculties have been carefully made out and labelled; and then the knowledge of these lists has been allowed to stand in numberless instances for the knowledge of the thing itself. To take an illustration of this method: let us suppose that any one endeavouring to investigate philosophically the nature of vegetable productions, were first to divide them into their component parts—the root, the stem, the leaf, the flower, the seed, &c.; and then, having described the varieties of all these separate parts, were to consider the work of investigation finished, and the knowledge derived from it complete. How imperfect, we at once see, would be the insight gained by this procedure into the real nature of the object under research. To gain any philosophic insight into it, it would be necessary to acquire an accurate knowledge of the chemical elements of the vegetable world; to have true ideas concerning the process of cell-formation; to know something in brief of all the wondrous operations which science has revealed in connexion with the physiology of plants. It would be necessary, moreover, to have some acquaintance with the plant as an organic unity; to trace its development from the seed-germ upwards; to understand its metamorphoses; in a word, to *analyse the whole thing into its primary elements and simple functions*, instead of giving a mere enumeration of its constituent parts.

The case of psychology is precisely parallel. A mere descriptive psychology is just as imperfect, scientifically considered, as a mere descriptive botany. It comes after all to little more than a mere catalogue of terms; and tends in the long run, when put in the place of true science, to become as dry, as formal, as dogmatic in its nature, and as circumscribed in its applications, as are the barren deductions of mere *à priori* speculation.

Neither is the matter helped forward by the aid of phrenology. With exception of the one fact, that phrenology has called attention to the physiology of the brain and the nervous system, its whole procedure has exhibited the crudest possible attempt at unravelling the web of our complex mental phenomena; while its effort to read the faculties on the surface of the cranium has never risen above a species of practical tact, which, in the hands of a keen observer, might give rise here and there to a few plausible conjectures, but could never reach the very lowest idea of a true science of the human mind. As phrenology was based originally upon an imperfect and immature knowledge of the nervous system, so the advancement of modern physiology in this direction has left it standing like an old landmark, which points out the ignorance rather than the knowledge of the times in which it flourished.

While these isolated efforts have been made to penetrate into the region of mind, and discover the laws of its operations, the nature of science itself, and the order in which the various branches of it develop themselves, have been gradually becoming more clear to the human reason.

It has now been distinctly apprehended, that no single science whatever is or can be isolated from all the rest; that those which have the fewest elements to work upon, and which start from the simplest ideas (such as number, space, force, &c.), arrive soonest at their perfection; and that the results of each, as they are unfolded, form the starting-point for that science which stands next in the scale of development.

Thus the results of arithmetic form the basis of geometry; while the results of geometry give us the starting-point for a science of dynamics. In the same way, chemistry, basing itself upon the sciences already investigated, gives the primary data for a systematic research into the laws of organized life—that is, for a scientific physiology; and physiology, when it reaches its higher walks, leads us upwards to the human soul, as the goal of its noblest efforts.

This truth of the co-ordination of the sciences, which has been for some time tacitly gaining ground amongst the fixed convictions of philosophic minds, has naturally thrown psychological research back upon physiological principles; proving to us beyond a doubt, that we need the data which the lower science can supply before we can give a proper foundation to the higher. Accordingly, in looking over the more recent attempts which have been made to advance and to fructify psychological research, we find that a very large proportion of them come either from the side of physiology, or from the practical necessity of seeking more definite knowledge in the treatment of insanity. A series of facts have thus come to light in connexion with the structure, functions, and diseases of the nervous system, which have already begun to carry the precision of the positive sciences into the region of psychological research. In estimating therefore, as we propose to do, the position and prospects of modern psychology, it will be necessary for us, first of all, to give a succinct and popular abstract of the most important conclusions thus arrived at.

The effective study of the nervous system takes its starting-point from the grand discovery of Sir C. Bell, that the whole of the nerves spread in infinite ramifications through the body, are of two different orders, and perform two distinct but related functions. One portion of them, he showed, convey impressions from all points in the circumference of the human frame to the centre; the other portion convey impulses from the centre to the various parts of the circumference. The one portion, therefore, are properly termed nerves of sensation; the other, nerves of motion: the one, afferent; the other, efferent. In this discovery we have the first idea presented to us of the nervous system as one great organ of action and reaction; as the link between the soul and the world; the instrument by which outward realities around us affect the mind; and by which the mind, as force or will, reacts in its turn upon the world without.

For some time it was imagined that every nervous impression necessarily reached the sensorium, and that every external movement, therefore, was made *consciously* in obedience to the will. Against this view, however, many well known facts began to raise well-founded doubts. It was observed, for example, that in cold-blooded animals, such as frogs and turtles, the operations of hopping, crawling, &c., could be very well performed for a time, after the severance of the head from the body. This was a sufficient proof that, in their case at least, the motor impulse could not come from the brain, but must reside in the nervous apparatus of the spinal cord. The researches, thus commenced, were carried systematically forward by Dr. Marshall Hall, until it was completely established, that in the human subject, as well as in the lower animals, there is a distinct and separate centre of nervous action in the spinal cord; and that numerous movements take place, the origin of which is not in the brain, nor in any of the sensory ganglia beneath it; but simply and solely in the spinal cord itself. These movements, of course, are performed wholly unconsciously (like the act of breathing in sleep or in apoplexy); they are the organic response, as it were, to certain physical stimuli, necessary equally for the preservation and the well-being of the human individual: in other words, they are *thrown back* from that part of the nervous system to which the stimulus especially applies; and on account of this particular characteristic, have received the name of REFLEX ACTIONS. Here, then, we see already how decidedly the nervous system in its unconscious operations has begun to claim for itself the origination of many phenomena which were before attributed to the direct effort of the mind, or the will; and we can judge from this fact alone how many false observations in psychology are corrected by the simple comprehension of the laws of reflex activity.

The phenomena of reflex action, however, were not allowed to rest here. It was seen that the principle, once established, in relation to the spinal cord, might be carried out still further, and throw light upon many other phenomena hitherto sufficiently perplexing. Dr. Carpenter took up the investigation where it was left by Dr. Marshall Hall, and has given us, particularly in the two last editions of his 'Human Physiology,' a very full and detailed account of the further conclusions at which he has since arrived. It is frequently supposed that the spinal cord, if traced upward, communicates immediately with the cerebrum, so that actions and re-actions pass directly from the one into the other. This is shown, by mere anatomy, to be erroneous. Instead of communicating with the cerebrum, the spinal cord is found to merge into a series of ganglionic masses, which form the centres of the nerves of sensation. These sensory ganglia are not, as was formerly supposed, mere appendages to the cerebrum; they are *distinct centres* of action and feeling, analogous to the entire "brain" of insects and mollusks, and form therefore, in regard to their functions, the subject of a distinct investigation, apart from the spinal cord on the one side, and the cerebral hemispheres on the other. It is in the fuller investigation of this second and intermediate centre of innervation, that Dr. Carpenter has added so materially to the elucidation of the whole subject of cerebral physiology.

The "sensory ganglia," as Dr. Carpenter has shown, hold an intermediate position as regards their functions, as well as their location, between the spinal cord and the cerebrum. Like the intellectual and voluntary activity of the latter, their operations are connected with *consciousness;* but, like the reflex activity of the former, they take place without forethought, purpose, or any control of the will. From this peculiarity they have received the appellation of CONSENSUAL *actions.*

Many simple examples of these actions might be readily cited. The start produced by a loud and sudden noise; the contraction of the eyelids, to prevent a too dazzling light; the act of sneezing, and the sensation of tickling; the process of sucking, in the young of the mammalia; these are a few of the most familiar instances of the consensual actions—actions, that is, bearing the double character of being attended with consciousness on the one hand, and yet being wholly involuntary on the other. They bear, it will be seen, a very close resemblance to the purely reflex actions before explained; and indeed may be called reflex, only with the further addition of our being fully conscious of their existence at the moment in which they take place.

The most important conclusions drawn from the phenomena of the consensual actions may be summed up as follows:

We learn from them—1. That many actions are performed by us, and performed *consciously*, which are not in any way the result of purpose, forethought, desire, or adaptation, and which therefore cannot be cited as any illustrations of our voluntary activity.

2. That there exists in the mechanism of the ganglia a pre-arranged system of impulses, which urge us to the performance of various functions, adapted to answer important purposes in the physical economy of our being, independently of anything connected with our own personal will or intelligence.

3. A third conclusion is—that the sensory apparatus is that part of our nervous system which supplies the immediate force, not only for the actions above alluded to, but for *all kinds of voluntary action* as well; that the will itself, in fact, can only act upon the muscular system through its intervention; that every human action, accordingly, viewed at one remove, is really automatic, because the sensory ganglia operate upon the nerves belonging to the muscular system, in a mode of which we are wholly unconscious at the time, and impel them to certain results through an impulse blind in itself, although set in motion by a voluntary effort derived from the brain. This explains the reason why rapid actions are often performed by us with a view to certain ends, the individual movements of which we do not at all follow with the understanding and the will. The will only contemplates the desired end itself; the automatic action of the sensory nerves instinctively supplies the method of accomplishment.

4. Another very important conclusion is—that the sensory apparatus, lying midway between the impulses of the world without, and the action of the intellect and will within, may be set into motion, and that similar motion, either by the one or the other. For example, a sensation, and the idea of a sensation, will often excite the very same consensuous movements. By thinking of a nauseous dish which has disgusted us, we may renew all the inconvenience we experienced from it. Many persons faint away by imagining vividly a surgical operation. The whole working, in fact, of the mind, and of its ideas upon the body, receives a new light as soon as we have well comprehended the independent position and the automatic action of the sensory ganglia. All the phenomena of hypnotism and electro-biology (as it is termed) are manifestly explicable on this principle. They all point to a great automatic centre, which can mould human action with the most perfect adaptation to definite ends, without being controlled by the will, and which may be excited, more-

over, to do so either by impulses directly from without, or by strong ideas operating downwards upon it from within.

5. One more point we may mention is—the light thrown by the consensuous movements upon the nature and philosophy of *instinct*. Those animals (chiefly insects) which are, as it were, all instinct, are known to possess simply a highly perfected sensory apparatus, without the superaddition of any cerebrum whatever, properly so called. Hence the rapidity, the perfection, the beauty, the adaptation of their movements, impelled, as they are, neither by conscious ideas nor by volition, but by a highly-organized automatic machinery. The consensuous movements in man are exactly analogous to this. We perform involuntarily, and without reason or forethought, many actions which are as complex in their nature, and as curiously adapted to certain ends, as does the bee when it constructs its hexagonal cell, or the spider when it weaves its snares for its enemies. Thus nearly all the instincts, both of man and the lower animals, become, in fact, simple examples of the reflex action of the sensory ganglia.

These, then, are some of the conclusions which have been drawn from the physiological investigation of this portion of our nervous system. Much more will undoubtedly be elicited in process of time; but what we have already presented sufficiently proves that, in any analysis of our complex mental phenomena, we should be liable to many errors and false conclusions without the light that flows from cerebral physiology; and that, in investigating a large number of important facts, we derive an essential service from the knowledge now possessed of the independent action of the reflex and the consensual centres of human activity.

Whether a similar light will be thrown upon the working of the emotions, remains to be seen. In the highly intelligent and scientific work of Mr. Noble on Insanity, we are directed to a series of physiological facts bearing very closely upon the elucidation of this part of our nature. He has there stated various grounds for the belief, that the emotional sensibility has a distinct centre amongst the ganglia of the lower brain; and shows that by well adapted experiments and observations we may succeed eventually in isolating, as it were, the activity of the emotions, just in the same way as Dr. Carpenter has done with regard to the sensory apparatus. If this theory be verified, we may expect a new and most welcome light to be shed, through the aid of physiology, accompanied by a series of well-directed experiments, upon that portion of our mental constitution, which has hitherto been marked, even amongst professed psychologists, with the greatest amount of indistinctness and confusion.*

To return, however, from this short digression on the emotions, we must proceed somewhat further with our exposition of the various centres of nervous action. Two have already been pointed out; those which originate purely reflex and unconscious movements; and those which give rise to conscious but yet involuntary actions, prompted by certain guiding sensations. It is hardly necessary now to dwell at any length upon the third great centre of innervation, which exists in the cerebrum proper. All the experiments of modern physiology go distinctly to prove that the physical organ attached to the intellect and the will lies here. This third centre of nervous action, moreover, operates in perfect co-ordination with the other two. Just as an impulse from without passes upwards, first through the spinal cord, then to the sensorium, where it becomes an object of consciousness, and lastly, to the cerebral hemispheres, and is there attended by the genesis of actual ideas; so an idea or volition, beginning its physical career in the brain, passes down again to the sensorium, sets the automatic apparatus in motion, and finally reacts, through the instrumentality of the muscular system, upon the world without; the whole system thus showing the most beautiful and harmonious co-ordination between thought itself, the material organism through which it is conveyed, and the order of universal nature in the midst of which we are placed.*

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* In addition to the work mentioned at the head of this article, the student might also consult 'Three Lectures on the Correlation of Psychology and Phrenology,' delivered by Dr. Noble, at Manchester.

We may regard it,. then, in fine, as a point in our knowledge of humanity which has been definitely gained by the researches of modern physiology, and which can now be set down within the region of positive fact,—that there are *three main centres of nervous action*—the spinal cord, or the excitor-motor system; the sensory ganglia, or the consensuous system; and, lastly, the cerebrum proper, which is now known, as far as any direct evidence can reach the case, to be the physical laboratory in which our notions, ideas, and voluntary efforts first manifest their action on the world without.

In the 'Elements of Psychology,' by J. D. Morell, these conclusions, which have been recently developed on the side of physiology, are employed for *generalizing* the study of mental philosophy, and bringing it more directly into co-ordination with the science of nature. The views there presented are substantially as follows. Comparative physiology has brought to light the fact that, physically speaking, there is a regular progression visible throughout all organized existence. The vegetable world exhibits already the phenomena of life and growth—i. e., of self-development from a primary germ in the way of cell-formation. Proceeding upwards, we find that the limits between the vegetable and the animal kingdom can hardly be defined; and that when we once arrive definitely at the lower forms of animal life, there is still an infinite gradation in the structure and perfection of the nervous system, developing one instinct and one faculty after another, till we come to the very limits of humanity. Once within the region of humanity, we see the law of progress still going forward, and exhibiting a new series of stages, from the mere sensitive life of the infant, up to the loftiest forms of reason and will.

If, then, we can trace a regular progression throughout nature, in carrying out the laws of organic life—from the first effort at cell-formation up to the highest and most complicated cerebral machinery—then, it is argued, there must be some rational connexion running through the whole. This connexion is seen in the fact, that there is a constant tendency throughout all being to advance from the more material form of existence to the more immaterial; from the more instinctive regions of intelligence to the more rational; from the passive to the active; from the dependent to the independent; from complete identification with nature to the higher life of a self-determining individual. This law, then, which we see at work throughout nature universally, holds good equally in the whole process of our mental development. The principle of life, which acts unconsciously, though with perfect adaptation, in the vegetable world—which operates blindly, according to mere instinct and impulse, amongst the lower animals—which gives rise, not only in them, but also in mankind as well, to reflex activities beautifully adapted to subserve the purposes of self-preservation,—this principle of life is at length gifted with self-consciousness in connexion with the superior organism of the brain, and the consequent operation of the higher faculties; and being gifted with self-consciousness, still proceeds onward to the development of the highest reason, the purest emotions, and the most perfectly self-regulating will. The problem of psychology, accordingly, as here viewed, is to show how the laws of nature, assuming the form of the laws of self-conscious mind, accompany the soul onwards through the various regions of instinct, of sensitivity, of intuitive perception, of ideal representation, &c., up to the highest regions of reflective thought and voluntary activity.

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* To those who wish to see many of these results combined in easy and graceful dialogue, together with various practical and moral conclusions drawn from them, we recommend the perusal of Sir B. Brodie's 'Psychological Inquiries,' as containing the fruits of much observation, matured by a familiarity with such topics derived from long and daily experience.

In this way mind comes to be viewed as an organic unity, developing successive powers like every other organism; and *the science of mind*, no longer standing alone, takes its place in the regular series of the natural sciences, depending for its data upon the results of those which have gone before.

This point of view has been taken up by Dr. Laycock, and made the basis of renewed investigations into the functions of the brain. Starting with the now admitted phenomena of reflex action, and granting that such action must take its rise from the vesicular matter of which the ganglia are composed, he goes back one step further in the inquiry, and asks, How or by what active principle is it that this vesicular organism is constructed, and so constructed as to produce such marvellous results? How is it that a material machinery should exist within us which, when set in motion by some stimulus from without, should have all the effect of the most perfect contrivance and forethought? What is the principle of intelligence by which it acts, independent as it is of our own conscious volition? Theories of all kinds, he shows, have been formed in reply. Plato, in his time, maintained the existence of a *plastic power* in nature, which forms everything adaptively for its position and circumstances. The ordinary way of cutting the knot at present is by introducing a *Deus ex machinâ* and attributing the constructive power and intelligence shown both in the mechanism and functions of the ganglia and nerves, to the direct, separate, and individualized operation of the Deity; which comes, in fact, to a sort of modified doctrine of "*occasional causes.*"

With these hypotheses, however, Dr. Laycock thinks *inductive philosophy* has nothing to do. It treats only of palpable phenomena, and the method of their operation. It seeks to find some expression of the laws of nature actually at work around us, independently of any theory respecting the individual exertion of Divine power in carrying them on.

Viewing the subject in this light, one plain fact presents itself to us, that there is inherent in the primordial cell of every organic existence, and through all its subsequent growth, *an immanent or abiding law of development*, which moulds matter into forms of the most exquisite beauty, and constructs out of it machines adapted to all the peculiar wants or possible contingencies of the individual. Speculation may account for this abiding law of plastic activity in a variety of ways: there may be, and have been, numerous theories as to its exact relation to the great First Cause, the all-sustaining Mind of the universe; but apart from speculation, here is the fact palpably presented to us, that a principle of life exists, and exists abidingly, in the primary cell and its subsequent development, which acts at every instant of each creature's organic growth, and acts, too, with all the effect, all the outward manifestation, and all the final results, of intelligence—that is, of complete adaptation of means to the most desirable ends. Whether we choose to call this a principle of intelligence or not, must of course depend upon our definition of the term—i.e., whether or not the word "intelligence" ought to be employed for any kind of activity which is unaccompanied with self-consciousness. That there is a principle at work, however, in every atom of organized matter which produces intelligent *results*, is a fact which, speculation apart, admits of no dispute.

Having considered the plastic principle that resides in every organism, Dr. Laycock next goes on to show that there must be a direct connexion between the construction of organisms, and their use when constructed; and that we have no right, on the principles of the inductive philosophy, to wander away into speculations, and imagine two distinct intelligent agents, to account for these two so closely affiliated purposes. The intelligence, for example, that develops the ganglia in the head of the bee from the primary insect cell-germ, must, he thinks, be fundamentally the same as that which prompts it to construct a comb with the most perfect mathematical proportions. Either we must attribute both processes to an extraneous power, which renders the bee simply a living machine, or we must attribute them both to an immanent principle, that operates without self-consciousness, indeed, yet individually, in each separate organism.

The operations or functions, then, of what we may term the *unconscious principle of intelligence* in organic nature, may be summed up in a few words:—

1. It moulds matter into living organisms according to a fixed, predetermined plan, and adapts them by the most certain intuitive logic to the purposes for which they are constructed. 2. It moves and regulates these living machines, according to fixed and unchanging sequences, in such a way as to promote the welfare and continued existence of the individual. 3. In animals endowed with self-consciousness—that is, in man—it acts upon the vesicular matter of the brain, and excites changes there; the results of which changes, when presented to the consciousness, constitute some of the most important phenomena of thought.

The next step, according to Dr. Laycock (after this elucidation of what he has termed the unconscious principle of intelligence), is to show the relation which this unconscious principle bears to the conscious mind. Are we to regard them as two distinct intelligent agents; or can we identify them as being really and fundamentally one and the same? To this question Dr. Laycock has addressed himself at some length, and shows with great skill that it is impossible to separate into two categories the ganglionic formations, which subserve the *instinctive* life of men and animals, and the cerebral formations, which subserve *conscious intelligence.* He has proved, in fact, that the brain is, strictly speaking, a large ganglionic centre, only superior to the rest; that it is subject to the same laws of development and growth; that it accumulates substrata, and, consequently, power of function, in the same way as the rest of the nervous system; that it responds to stimuli in the same manner, and is strictly subject to reflex actions;—in brief, "that the two forms of mental manifestation—the voluntary and the involuntary—have a common origin and a common substratum, and *that the human mind is none other than the unconsciously working principle of intelligence individualized, become conscious of its own workings in the cerebrum, and deriving its ideas from its own constructive or material changes in the organ of mind.*"

In the views thus put forward by Dr. Laycock, we certainly see the most complete co-ordination established between psychology and physiology in the widest extent. All the facts and laws of physiology, beginning with the simplest notions of cell-formation, are brought into play, and then traced upwards in one direct line of progression, till we are landed amidst the most remarkable phenomena of mind and the general laws of its operation. Whether all the particular conclusions which Dr. Laycock has drawn, in his analysis of the subject, are correct, it can hardly be our object here to examine. The point we are most anxious to notice is, the new direction which this kind of research must inevitably give to psychology as a science, in extending the operations of mind beyond the limits of consciousness, and thus drawing the activity of the soul and the activities of nature into one broad and scientific generalization. Doubtless, it will require both time and labour to work out this generalization, but no system of psychology can henceforth prove satisfactory that does not at least *attempt* to solve the problem thus arrived at, and to interpret the numerous phenomena which bear so directly upon it.

In following out this process of investigation, it is extremely important to separate the whole inquiry viewed as a question of facts, from any speculations we might be tempted to enter into respecting the first creative Power from which every mode of intelligence alike emanates. All we have to do is to investigate—(1) the changes which the immanent principle of life operates in matter, moulding it to certain great and wise purposes; and (2) the changes which take place in our states of consciousness consequent upon these material adaptations. We want to know, in short, whether these changes can be reduced to any distinct and intelligible *laws*, so as to cast new light upon mental phenomena, and the relations of self-consciousness to our physical organization.

No doubt all such attempts will be met, more or less, by the old watch-cries of *materialism* and *pantheism;* but we protest in the outset against any such charges, not only as hasty and ill-judged, but perfectly retortable. It may seem at first sight to be the part of a devout and earnest Theism to maintain the theory of a direct Divine intervention wherever adaptation exists in nature apart from the human will and intelligence; but a very slight amount of further analysis shows us how closely this theory links itself on to a pantheistic result. Pantheism, fundamentally speaking, is the identification of God and nature—sinking the idea of a divine Personality in the category of physical law. And it is hard to say what can more readily prepare the mind for this identification, than the habit of regarding the growth of every plant, the instinct of every animal, the impulse underlying all the unconscious process of human activity, as so many direct modes of the Divine operation, and expressions of the Divine Intelligence. It is assuredly an equally short road to a pantheistic conception of the universe, whether we bring the works of God down to a unity with the laws of nature, or whether we elevate nature upwards to the Divinity. Let *science* follow her path unrestrained; let her cultivate her own region—that of *universal law*—untrammelled by speculations either of one kind or another; and moral reflection will be sure in the end to vindicate for itself the truths of theology, and that upon the only firm and immovable grounds from which a true and all-influencing theology can take its start. The more the regions of physical law and moral order are separated, the more certainly will science advance on the one side, and the more will theology be rendered secure from every pantheistic tendency on the other.

The most recent works of any magnitude which England has produced on the subject of psychology are, 'The Senses and the Intellect,' by Alexander Bain; and 'The Principles of Psychology,' by Herbert Spencer. The former of these works, though starting strictly from the psychological point of view, is conceived in a spirit altogether different from the Essay of Dr. Laycock, above referred to. The object which the author aims at is simply to give a natural history of mental phenomena, as far as the senses and the intellect are concerned. There is no attempt at any generalization between organic life as existing in mind and nature; no distinct co-ordination between the phenomena of instinct and intelligence; no inquiry into the nature of the agency by which the mechanism of the nervous system is constructed and worked. All topics beyond a mere enumeration and exposition of patent phenomena are avoided, as though they had no place in the discussions of mental philosophy. To us this appears to be a serious deficiency in a work evidently intended to be as complete as possible on the positive side of the question. The whole of the region lying between the unconscious and the conscious operation of the soul, is one fraught with the most interesting phenomena —phenomena, too, on which we are anxiously looking for more definite knowledge, as the result of physiological and mental inquiry. If the bee constructs its cell in the highest style of mechanical skill, it is certainly not by any means alien to the spirit of inductive philosophy to inquire into the nature and the laws of the *intelligence* by which such acts are prompted. So, also, if the human individual perform unconscious actions under the promptings of certain nervous centres—actions having the most perfect adaptation to wise and necessary ends, it is not the part of a true mental philosopher simply to accept this as an ultimate fact; but rather to seek some larger generalization, some deeper law, by which such facts may be co-ordinated with the other phenomena of mind and nature. For, even granting that we must stop *somewhere*, before we reach the *causa causarum*, yet so long as myriads of facts like those referred to lie around us unanalysed and unexplained, we cannot be said to have made any real progress in a *philosophy of mind*, although we may have made a very extensive classification of its more obvious phenomena.

Mr. Bain's work must be regarded, in fact, as an industrious, intelligent, and profuse colligation of facts, physiological and mental; one of those "amassing of instances" which always prove highly valuable as the precursors of inductive science, but which certainly only come up just to the threshold of science itself. Of the 600 pages, of which the work consists, above one-third already exists, in one shape or another, in the various treatises on physiology; so that it may be regarded as containing a very fair digest of what is already known, *as fact*, of the relations between the mind and the body. With regard, however, to the conclusions drawn from these facts in the explication of mental phenomena, we do not think that they are uniformly trustworthy. Many things are explained on physiological principles, which have the appearance of being direct deductions from known facts, but which are, in reality, pure speculations. Such are all the accounts given of the rise of the ideas of extension, form, size, &c., by the aid of muscular movements; such, also, is the physiological theory propounded respecting the revived impressions of the senses. With regard to this latter point, especially, physiology is as yet in its infancy; we know next to nothing of the physical substrata of our ideas, or how they pass in and out of consciousness. This is a field of research on which very little can as yet be said with any certainty; and anything that is said, should certainly be qualified with expressions that denote anything rather than confidence or certitude.

With all this, however, to set off on the other side, Mr. Bain's work is a decided advance upon what has preceded it in the natural history of mental phenomena. He has abandoned the old method of dividing the mind into so many definite faculties; has seized the unity of its operation; elucidated by the aid of physiology the primary and spontaneous stages of mental activity; shown the germs of volition as existing in the instinctive impulses of the nervous system; and gone some way, at least, into the explication of the higher mental phenomena, as resulting from the combinations and associations of our primary ideas according to appreciable laws of suggestion.

The volume recently published by Mr. Herbert Spencer, entitled 'The Principles of Psychology,' forms altogether a contrast to that on which we have just been commenting. While Mr. Bain is conversant chiefly with external facts, and never penetrates, indeed, very far beneath them, Mr. Spencer is equally remarkable for his search after first principles; for his acute attempts to decompose mental phenomena into their primary elements, and for his broad generalizations of mental activity, viewed in connexion with nature, instinct, and all the analogies presented by *life* in its universal aspects. We may say, indeed (without by any means endorsing all the individual results arrived at), that, in our apprehension, this well-studied and deeply philosophic volume presents on the whole one of the most vigorous attempts which has yet been made in our country to place mental philosophy upon a broad and positive basis. In doing this, every branch of metaphysical inquiry has been touched upon, and more or less elucidated; and perhaps we could not more appropriately draw our remarks on modern English psychology to a close, than by giving a very brief abstract of this, the latest work which it has produced, and one, too, which takes into account, more or less, all the results which have as yet been realized on the subject.

The first part of the work takes up the question of *certitude;* that is, it investigates the starting-point of all human knowledge, and the assurance we have of its validity. It is chiefly occupied in expounding and maintaining the "*universal postulate*" upon which a clear system of natural realism may be built; and by the simple application of which both idealism and scepticism may receive their most obvious and unanswerable refutation. This postulate is—"*That every belief which is proved by the inconceivableness of its negation to invariably exist, is necessarily true.*"

The next part takes up the *theory of reasoning.* All reasoning, in its fundamental analysis, is shown to be *a classification of relations;* while every single act of reasoning is defined as "the indirect establishment of a definite relation between two things." This theory is followed up consecutively through every species of inference, whether quantitative or qualitative; and gives us at once a test of the Aristotelian syllogism, and an exposition of all possible methods of argumentation, whether deductive, inductive, hypothetical, or analogical.

But all reasoning is based upon terms, and those terms upon mental perceptions. What, then, is perception itself? Evidently an immediate, direct, intuitive cognition of relations between things. For when I say I perceive a thing, what else do I mean, but that I separate it mentally from the whole mass of my experience, and observe its qualities as similar to, or different from, the qualities of other things by which I am surrounded? Thus all perception, all classification, all naming, all recognition, is simply the intuitive apprehension of given relations; is the very same operation, therefore, in its direct form, which reasoning is in its more explicit and indirect.

By following out this view, the author proceeds to discover the genesis of all our fundamental ideas. All mental experience begins by the observation of some change in the phenomena of consciousness. The simplest imaginable change is the *sequence* of two similar events; and the consciousness we have of their likeness or unlikeness will be the first and most fundamental act of the human intelligence. It is by the repetition of this same process, and the combinations to which it leads, that (according to the views here propounded) all our subsequent knowledge is built up; just as the whole body is constructed by the repetition of the one primary original act of cell-formation. For these fundamental relations once established, the author shows how we form consecutively the ideas of resistance, of motion, of time, and of space, the one flowing necessarily out of the other; and how we may thus ascend by the pathway of experience to all that is involved in the perception of *body* and its various attributes. The attributes of matter the author classifies similarly to Sir W. Hamilton, into the primary, the secondo-primary, and the secondary; only, to make his meaning more clear, he terms them the statical, the statico-dynamical, and the dynamical—attributes, that is, which vary according as the object perceived is more or less active in the production of the mental phenomenon. In the whole of this development, the experience-hypothesis respecting the formation of our fundamental ideas, is consistently and logically carried out; and though it must be confessed that some points are as yet obscure, and some appear, at any rate at the first view, far fetched; yet, as a whole, we must pronounce the entire analysis one of the most acute and logically complete which has yet been presented by English psychologists respecting the genesis of our primary ideas.

The author next proceeds to the subject of psychology, properly so called; and following the path which modern science has opened, takes it up on the physiological side of the question. The first thing requisite, he considers, is to gain some proximate idea of *life;* for all organized existence, from its lowest to its highest forms, is but the varied manifestation of life in the progressive exercise of its appropriate functions. In every step of its manifestation, the author contends, the same general principles and arrangements so distinctly prevail as to justify the broad generalization which includes mind and nature as one vast whole, pervaded by the same ideas, and progressing by the same great laws of universal being.

Life may be defined as *the continuous adjustment of internal and external relations.* Such adjustment is simple enough in the lower ranks of creation; but increases in complexity in proportion as life itself rises higher up the scale of being; while in man it requires all the powers of his mental nature to compass it. *Intelligence,* accordingly, in its fundamental analysis, is the power of adjusting life to the circumstances by which it is surrounded.

The universal law of intelligence flows directly from the co-operation of mind and nature in the genesis of our ideas. It is this—that just in proportion as there is a persistency in the order or relationship of events in nature, so will there be a persistency in the connexion that subsists between the corresponding states of consciousness. The succession or co-existence of external phenomena produces, of course, a like succession or co-existence in our mental perceptions; and when any two psychical states often occur together, there is at length established an internal tendency for those states always to recur in the same order. Starting, therefore, from this law, the author first traces the growth of the human intelligence through the lower phenomena of reflex action and instinct; then, shows how our unconscious life merges into a succession of conscious phenomena; and lastly, carries us upwards, through the regions of memory, &c., to the highest exercise of reason and the normal development of the feelings.

The brief space to which this article is confined, forbids our giving any idea of the method of analysis by which these conclusions are reached. The result of the whole, however, is this—that, in Mr. Spencer's estimation, the experience-hypothesis, taken in its broadest sense, is sufficient to account for all our mental phenomena; that every form of intelligence is regularly evolved out of the harmonious connexion of mind and nature, and that as many facts occur which no possible ingenuity can ever account for, on the principle of individual experience,—we must here have recourse to the development of races, or to that of the human race in its totality, for the explanation. On this principle we shall find at length, he thinks, a complete reconciliation of the rival claims of idealism on the one side, and sensationalism on the other.

In all these results we cannot but see how the old landmarks of mental philosophy are breaking down and disappearing under the steady advance of physiological science; and yet how imperfectly we can complete, with our present knowledge, that vast psychological structure, the foundations of which we see already laid out in the scheme of the sciences. In going through Mr. Spencer's analysis of life and its developments, we cannot help feeling throughout that the present imperfection of physiology as a positive science still throws a shadow upon some of his most important conclusions; or leaves them, to say the least, in the position of mere speculations, which the future may confirm or destroy.

The views he hints at respecting the genesis of the nervous system, and the complicated nature of the human brain, as representing an infinity of experiences gained during the evolution of life from its lower to its higher forms, can only be regarded as theories at present totally unestablished, and which have to await the future light of physiological science ere any solid conclusion can possibly be built upon them.

Moreover, acute, often convincing, as are the author's views on the intellect, so far as it is concerned in the development of primary ideas and the genesis of science, there is still a large region of mental phenomena on which he has as yet indeed thrown very little light. We mean the intuitions of beauty, the development of moral life, and the aspirations of the religious nature. It is true he only proposes to give us the "principles of psychology," and therefore it might be unreasonable in us to look for the results; but, in our apprehension, those principles are not yet deeply grounded enough to compass the whole length and breadth of the psychological problem; nor do they, indeed, lead anywhere *near* to a satisfactory analysis of the *will*.

When all the direct correspondences of the soul and the world shall have been explored, we shall not then be at the complete termination of our research; for our inquiries lead us insensibly onwards to the primary cause as well as the final goal of human reason, and to the problem of human destiny, in connexion with that great all-pervading Intelligence by which the pre-established order of the conscious and unconscious universe has been itself arranged from the beginning. If intelligence grows and expands through the perpetual adjustment of mind with nature, then must it not reach up at last to that primæval Intelligence by which the law of life has been planned and sent forth upon its mission? And if we can identify instinct as the early twilight of reason, cannot we follow out the same law of progress, so as to identify human reason itself in its present form as the twilight only of a still higher form of existence? These are questions to which psychology at present only distantly and indistinctly points us; but strange indeed would it be if the more complete comprehension of mental phenomena should be found ultimately destructive to the best aspirations and the most purifying faith of our spiritual nature. For ourselves, we believe that while it sweeps away many narrow and noxious superstitions, it will leave the great pillars of our moral conviction and our human hopes more firmly than ever rooted in the analogies of reason, and the obvious tendencies of human life.

In conclusion, we shall sum up in a few observations the general results which appear to follow from the foregoing criticisms upon the present position and prospects of psychological science. We have found in the course of our remarks—

1. That the purely rational and abstract systems of mental philosophy uniformly prove unfruitful; very much in the same way as did the science of nature, so long as it was pursued by inward reflection only, without any systematic reference to actual and observable phenomena.

2. That the more empirical systems have contented themselves too much with a mere enunciation of phenomena, giving us rather a natural history of mental facts than any searching analysis, or broad generalizations concerning them.

3. That the researches of physiology, reaching up as they now do to the organic functions of the nervous system, both without and within the region of humanity, have set mental philosophy once again in movement, and drawn it more and more into the circle of natural science.

4. That as physiology itself is only in its infancy in regard to the functions of the brain and nervous system, it can supply at present only very partially the facts that are necessary for a complete psychology.

5. That as physiology advances, the co-ordination of the science of mind with that of nature must become more and more apparent, and the laws operating through the one must throw new light upon the fundamental processes of the other.

6. Lastly, we have good reason for the belief, that psychology, as a science, is now once again on the road of advancement; and that, grounded on positive principles, it will this time become fruitful in all its applications. Instead of leaving us in doubt and difficulty respecting the basis of human knowledge, it will exhibit with new distinctness the origin, the growth, and the validity of our ideas; will teach us to separate the material of truth from its Protean forms; will elucidate the nature and worth of the sentiments and emotions; show us the real power and energy of the human will; guide our interpretation of the religious aspirations; instruct us in the principles and laws of education; define more clearly the limits of moral responsibility; and give us, in fine, that insight into humanity as a whole, which shall promote and regulate all the operations at once of justice and of charity.

J D. Morell.

REVIEW V.

1. *A Manual of the Practice of Medicine.* By GEORGE HILARO BARLOW, M.A. and M.D. Cantab., Fellow of the Royal College of Physicians, Physician to Guy's Hospital, &c.—*London.* pp. 706.
2. *Elements of Medicine: a Compendious View of Pathology and Therapeutics; or, the History and Treatment of Diseases.* By SAMUEL HENRY DICKSON, M.D., LL.D., Professor of the Institutes and Practice of Physic in the Medical College of the State of South Carolina.—*Philadelphia.* pp. 752.

DR. BARLOW'S work is the most recent of a series of excellent manuals the rapid and extensive sale of which must afford satisfactory proof, both to publisher and authors, that their labours are very generally appreciated by the purchasers and readers of medical books. The treatise before us, on the important subject of Practical Medicine, bears internal evidence of having proceeded from the pen of an experienced, laborious, and conscientious physician; and we do not hesitate to express our conviction that "students and junior practitioners," for whose use it is chiefly intended, may safely adopt it as their guide.

We purpose now to give a general sketch of the contents of Dr. Barlow's manual, and while passing in review the chief subjects of which it treats, we shall not fail to direct attention to such errors and omissions as we believe to exist. Nor do we doubt, that in proportion as we succeed in the faithful discharge of this duty, we shall receive the thanks, not only of our readers in general, but of the author himself in particular.

The first seven chapters are devoted chiefly to the subject of general pathology:—1. Causes of Disease; 2. Modes of Death; 3. Elementary Changes; 4. Inflammation; 5. Signs of Inflammation, and of Disease in General; 6. Fatal Termination and Treatment of Inflammation; 7. Typhous, Scrofulous, and Tuberculous Deposits. A sufficiently full though condensed account is given of each of the above subjects. We would direct attention particularly to the fifth chapter—on the Signs of Disease—as affording conclusive testimony to the careful and accurate observation of the author, and containing much that is instructive to the student. From this chapter we extract the following remarks on irregular and intermittent pulse:

> "The conditions requisite for the regularity of the pulse have been explained to be—a uniform current of blood to the left ventricle, and a due supply of nervous influence. Intermittent pulse does not, therefore, as has sometimes been stated, necessarily indicate valvular disease of the heart, but some circumstance tending to interfere with either of the above conditions. Amongst those which impede the due supply of blood to the left ventricle, may be reckoned—disease of the right side of the heart and pulmonary artery—disease of the lungs and their appendages, impeding the pulmonic circulation—and disease of the left auriculo-ventricular orifice. Irregular or intermittent pulse also occurs when the muscular walls of the heart are degenerated or attenuated, though probably as the result of the pulmonic congestion always attendant upon such a condition. When the pulse becomes irregular from any of these causes, it is almost always at the same time very feeble or indistinct. Circumstances may, however, arise, which may

prevent the pulse becoming irregular, notwithstanding the presence of some of the conditions just described. Intermittent pulse may also be the effect of disease of certain portions of the nervous system, or (through the medium of the nerves) of the digestive organs." (p. 76.)

The special diseases which are first treated of are rheumatism and gout. With respect to the nature of these diseases, Dr. Barlow says of rheumatism, that "its internal or essential cause seems to be an abnormal condition of the blood, which contains always an excess of fibrin and of uric acid—the latter is probably the *materies morbi*, or peccant matter." There can be little doubt that this statement with regard to uric acid is an error, for Dr. Garrod[*] has demonstrated, that whereas the blood of a gouty patient contains an excess of uric acid, there is no evidence of an excess of that material in rheumatic blood. With respect to gout, the author not only maintains that the *materies morbi* is nearly allied to uric acid, if it be not identical with it, but he suggests as probable that in consequence of an affinity between the uric acid and the parts affected with gout, "there may be a local excess of this substance without any such excess, or even with a deficiency in the system at large—just as there may be local hyperæmia, although the general state of the system is anæmic;" and he adds, that this explanation "receives confirmation from the recent observation of Dr. Garrod, that uric acid is present in the serum effused when a blister has been applied over a joint affected with gouty inflammation." Now, in the first place, we must correct this account of the observation attributed to Dr. Garrod, who expressly states, that while the serum of a blister on the skin will give evidence of the presence of uric acid, when the blood from the same patient exhibits the phenomena, "the application of the blister should *not* be made to an inflamed part, for it seems that the existence of inflammation has the power of *preventing the appearance of uric acid in the effused serum*."[†] The excess of uric acid in the blood of gouty patients is a demonstrated fact, but it is not therefore to be assumed that uric acid is the specific material cause of gout.

The consideration of the diseases of the lungs and heart is preceded by a general account of the methods of physical diagnosis; and we accord much praise to Dr. Barlow for the simplicity of his nomenclature, and the clearness of his descriptions of auscultatory signs, both in this chapter and in those devoted to the different diseases within the chest.

In the treatment of croup, we observe that the author places more reliance on tartar emetic than on calomel, giving the latter only in moderate doses as an auxiliary; and we entirely agree with him in this preference.

There are few amongst the less serious maladies to which the human body is liable which occasion a greater amount of collective misery than a common cold; and he would deserve well of catarrhal humanity who should devise some means by which *tutè, citò, et jucundè,* this disease might be cut short. Dr. Barlow says—

> "The safest and best plan, perhaps, when we are consulted early, is to advise a foot-bath, and going early to bed; a few grains of compound extract of colocynth, with a sixth of a grain of tartar emetic, and three of extract of hyoscy., with

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[*] Med.-Chir. Trans., vols. xxxi. and xxxvii.

[†] Ibid., vol. xxxvii. p. 58.

a moderate saline aperient in the morning; and a mild diaphoretic draught, as three drachms of liq. amm. acet., with about half a drachm of spirit. æther. nit., and ten or twelve minims of vin. ant. pot. tart., or vin. ipecac., in camphor mixture, three or four times daily." (p. 196.)

Allusion is also made to the dry plan of cure, and to the more grateful *moist* cure. About four glasses of sherry, with sugar, in a large quantity of warm water; light reading on the sofa for the evening; a foot-bath, and early to bed. We would substitute for the sherry a grain or a grain and a half of opium at bed-time, in the case of those patients who can take that drug without inconvenience. According to our experience, however, by far the pleasantest and the most efficient means of cutting short a cold at the very commencement, is the hot-air bath.

Passing on to the subject of bronchitis, we find that no mention is made of pulmonary collapse in connexion with the morbid anatomy of that disease. We observe, too, with some surprise, that the important subject of emphysema of the lung is only referred to incidentally on three or four occasions; and that no account is given of the morbid anatomy and pathology of that very common and distressing malady. The readers of this journal will remember that Dr. Gairdner* has attempted to show that collapse of the lung from obstruction of the bronchial tubes, and emphysema of the lung, stand to each other in the relation of cause and effect; that the dilatation and rupture of air-cells, which constitute emphysema of the lung, are simply the result of a mechanical necessity for occupying space left vacant by the collapse of portions of lung whose air tubes have been obstructed by the viscid secretions in bronchitis. In the words of Dr. Gairdner,† "Emphysema is, according to this theory, an *increase in volume* of those portions of the lung to which the air has access, to supply the place of *diminished volume* in those parts from which it is excluded."

It is very probable that in this way portions of lung surrounding collapsed lobules may become emphysematous; but that this is the whole account of the pathology of emphysema, we are by no means prepared to admit. It is obvious that the great enlargement of the whole lung, and of the entire chest, which is so characteristic a feature of emphysema, cannot be explained by Dr. Gairdner's theory. And, moreover, we have repeatedly met with patients presenting all the physical signs of emphysema in a marked degree—the barrel-shaped chest, with very limited motion of the ribs, great resonance on percussion, and feeble respiratory murmur—who have assured us either that they have never suffered from cough or other signs of bronchitis, or that they have only very recently been troubled with these ailments. We therefore feel persuaded that, in some cases at least, emphysema of the lungs *precedes* the bronchitis with which it is so commonly associated; and that a true and comprehensive pathology of emphysema must take account of this fact, and explain it.

Returning to our author, we find that the subject next treated of is pneumonia. A complete and satisfactory history is given of this important disease and its treatment. We think, however, that we should, in most cases of pneumonia, abstract less blood and give less mercury than the author appears to recommend. This disease is one of those which, in a large proportion of cases, tends to a spontaneous recovery; and it is unquestionably one of the many which have too often been treated with a mischievous degree of activity.

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* British and Foreign Medico-Chirurgical Review, April, 1853.

† Op. cit., p. 472.

Pleurisy is the disease which comes next under consideration. Its symptoms and diagnosis are fully described; but it appears to us that it would have been well to make special mention of *chronic* pleurisy, beginning insidiously without the sharp and pungent pain of the acute disease, attended often with little or no fever or cough, and therefore not uncommonly *latent* until perhaps it has filled one pleura with liquid. Every practitioner should be aware of the insidious progress of this form of pleurisy, so readily detected by its physical signs when once attention is directed to the chest.

With regard to the operation of tapping the chest, Dr. Barlow gives the following judicious advice:

> "If, after the means which have been recommended for bringing about the absorption of fluid in the pleura have been perseveringly used, there is no apparent diminution in its quantity, or if it should suddenly increase, so as to threaten to destroy the patient by suffocation, the important question arises, whether it should be got rid of by making an opening into the pleura—that is to say, by tapping the chest. Although this operation is neither difficult nor, generally speaking, immediately dangerous, it may be well here to protest against its indiscriminate or too early employment, not only on account of the danger of admitting air into the pleura, which would, according to the principles already laid down, convert a serous effusion into a puriform fluid; but also because experience has taught us that, independently of such an accident, its repetition would produce the same result; and we have already seen that, in the majority of cases, such an effusion may be got rid of by other means. As long, then, as we believe the fluid to be serum, the operation ought not to be had recourse to, except to avert impending suffocation." (pp. 267-8.)

The subject of phthisis, which comes next in order, receives very full consideration. The directions for the prevention and the treatment of this disease afford a good illustration of Dr. Barlow's careful attention to minutiæ in the management of his patients.

Passing on to diseases of the heart, we find that the author applies the term carditis to the combination of pericarditis with endocarditis (p. 324); objecting to the restriction of that term to inflammation of the muscular substance of the heart, that this disease, "though it may be theoretically possible, is one of which we have no experience in its simple form." (p. 329.) It is probable that inflammation of the substance of the heart is in most cases, if not in every case, associated with inflammation of its lining or investing membrane; but surely it is better to apply the term "carditis" to cases in which the muscular structure of the heart is obviously the seat of inflammation, whether this be or be not complicated with disease of other textures, than to use this term, "for the sake of brevity," to designate the co-existence of pericarditis with endocarditis without an appreciable degree of inflammation of the muscular walls of the heart?

Carditis, in the strict sense of the term, though not a common disease, is by no means one of the rarest. Two cases of much interest have been recorded by Drs. Burrows and Kirkes;* and a third case, which had been previously published by Mr. Stanley,† is referred to in the same communication. In each of these three cases there were several points of striking resemblance. They all occurred in the male sex, and in boys approaching puberty, from twelve to thirteen years of age. All the cases commenced with, or were accompanied by, severe pains (supposed to be rheumatic) in the muscles of the thighs and arms, but without swelling of any of the joints. In two of the cases there was a papular or pustular rash, as if the blood was contaminated; perhaps a similar eruption may have existed in the third case, though not described. In all these cases the pericardium was extensively inflamed, in addition to the carditis, but in none was there any affection of the endocardium or valves. In all the cases there was delirium, and in one convulsions, with pain referred to the forehead, so that the brain was supposed to be the seat of disease; but in both Dr. Burrows' cases a friction sound and increased dulness sufficed for the diagnosis of pericarditis. The disease was fatal in every case—in one on the fourth, and in two on the eighth day. In all the cases the muscular substance of the heart was soft, congested, friable, and infiltrated with pus. Lastly, with reference to treatment, in both Dr. Burrows' cases the delirium appeared to be increased by the abstraction of blood. Dr. Burrows attributes the cerebral symptoms to sympathy between the brain and heart, and it is probable that this explanation expresses a part of the truth; but it is at least conceivable that the same morbid condition of blood which excited the carditis and the cutaneous rash, may have disturbed the functions of the brain by a direct poisonous influence upon that organ.

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* Medical Times and Gazette, p. 634. Dec. 1853.

† Medico-Chirurgical Transactions, vol. vii. p. 323.

With reference to Fatty Degeneration of the heart, it is probable, as Dr. Barlow suggests, and as Dr. Quain had previously stated, that in some instances this change has supervened upon true hypertrophy of the muscular substance. Now it has sometimes been suggested that mere overaction of the organ may first occasion hypertrophy, and subsequently atrophy and fatty degeneration. It is probable, however, that the imperfect nutrition of the heart in the later stages of these cases, is due to the gradual deterioration of the blood which results from the morbid conditions which first occasioned the hypertrophy. For instance, two of the most frequent causes of hypertrophy of the left ventricle are—1st. Disease of the aortic valves; and 2nd. Chronic Bright's disease of the kidney. These morbid conditions may exist separately or in combination, and it is needless here to describe in detail the processes by which each of these diseases tends to produce a gradual impoverishment and contamination of the blood; such a condition of blood, in short, as must be ill adapted to nourish a muscle which requires a continual supply of new material to supply the waste occasioned by its incessant action.

With respect to the influence of the blood upon the nutrition of the muscular structure of the heart, we would suggest as at least probable, that the softened and fatty condition of heart which is so commonly found in fatal cases of *delirium tremens*, is due to the excess of alcoholic hydro-carbon and the deficiency of protein compounds in the diet of these patients.

Dr. Barlow makes no mention of the *arcus senilis* in connexion with the diagnosis of fatty degeneration of the heart, and it is probable that he agrees with us in thinking that since Mr. Canton first taught us that the *arcus senilis* is a fatty degeneration of the margin of the cornea, it has been too hastily assumed that this appearance of the eye so frequently co-exists with fatty degeneration of the heart, that the change in the cornea affords presumptive evidence that the more formidable cardiac disease is making progress within. There is no analogy of structure between the extra-vascular cornea and the highly vascular muscular tissue of the heart which renders it *à priori* probable that they would be specially liable to a simultaneous degeneration. There is, in respect of structure, a closer analogy between the cornea and the hair; and our own experience leads us to suspect that an extended series of careful observations would show that the *arcus senilis* is much more frequently associated with grey hair and baldness than with degeneration of the heart. It cannot be denied that grey hair, partial baldness, and the *arcus senilis* may co-exist for many years in the persons of men in robust health, who have never manifested a symptom of internal disease, and who may at length attain to extreme old age. We could point to several living illustrations of this truth; and we protest against the hasty assumption of a doctrine which would be a fearful addition to the sources of hypochondriasis, and which would persuade thousands of men just past the meridian of life, and destined to live to a good old age, that death is visibly written in the opaque margin of their cornea. On the other hand, since the absence of the *arcus senilis* affords no kind of presumption that the muscular structure of the heart is free from degenerative changes, it is manifest that the appearances in the cornea afford little aid in the diagnosis of cardiac disease.

With regard to the prognosis of valvular disease of the heart, Dr. Barlow states that it is much less unfavourable in disease of the aortic valves, whether obstructive or regurgitant, than in cases of mitral disease. There can be no question that, as a general rule, cases of merely obstructive disease in the aortic orifice, indicated by a systolic murmur at the base of the heart, are those in which the prognosis as to the probable duration of life may be most favourable; but our own experience would lead us to the conclusion that regurgitant disease of the aortic valves, indicated by a diastolic murmur at the base, is at least as unfavourable a form of disease as mitral regurgitation, with a systolic blowing at the apex. We agree with Dr. Barlow, that when disease of the mitral valve is obstructive as well as regurgitant, it is more rapidly fatal than any other form of valvular disease, the contractile power of the auricle being insufficient to overcome the impediment offered by narrowing of the mitral orifice.

After valvular disease, its consequences, and its treatment, aortitis and aneurism come under consideration; the signs of thoracic aneurism being well described, and the indications for treatment briefly explained. Then follows a chapter on Diseases of the Liver and its Appendages, in which Dr. Budd's well-known work is frequently quoted. Next in order come Diseases of the Œsophagus and Stomach, and we are here tempted to quote the author's description of two remarkable cases of perforating ulcer of the stomach.

> "In one instance, of an elderly man who had been long suffering from apparently intractable dyspepsia, with great pain and distension immediately after taking food, several perforating ulcers were found in the stomach after death; but

most of these opened into the portions of the intestine, between which and the stomach adhesion had been established before the perforation occurred. Thus one communicated in this way with the duodenum, and two with the transverse colon; whilst one opened freely into a large sac or pouch formed by thick fibrinous lymph effused upon the surfaces of the adjacent viscera.

"In another case, which occurred some years ago in Guy's Hospital, there were the pain and tenderness of circumscribed peritonitis in the left hypochondrium, in an elderly female, who had before been in the hospital for what appeared to be chronic gastritis; but what was remarkable, symptoms of pleuritis speedily followed, and shortly after those of considerable pleuritic effusion, combined with which there were metallic tinkling and amphoric cough and voice, so distinctly marked as to lead those who were unacquainted with the previous history of the case to believe that pneumo-thorax existed. Inspection after death showed a large perforating ulcer of the stomach at the (small?) curvature, through which the contents passed freely into a large pouch of false membrane, spread out upon the neighbouring viscera, the roof of which was formed by the diaphragm, through which the inflammation had extended by contiguity to the left pleura, giving rise to serous effusion in that cavity; the pouch before mentioned being distended by flatus from the stomach, produced the amphoric sounds by the concussion caused by the movements of the diaphragm in breathing, coughing, or speaking; affording a curious instance of the auscultatory phenomena which may be produced in the abdomen."* (pp. 422-3.)

Passing over a chapter on Dyspepsia, we come to one on Peritonitis, which contains a very complete account of the causes, signs, and treatment of that formidable disease. Dr. Barlow expresses his belief that disease or irritation of the ovaries is a cause of severe peritonitis more commonly than is generally known, or at least recognised by medical authors. He mentions one case occurring in the person of a young married lady, who imprudently sat upon the grass at a pic-nic party about the time that the catamenia might be expected to appear. A few days afterwards she was seized with rigors, followed by the symptoms of severe peritonitis, which ended fatally. Upon inspection after death, there was found extensive peritonitis, which appeared to have commenced from the serous coat of the left ovary, the ovary itself being large, hyperæmic, and containing a cyst about the size of a pea. Another instance, which was not fatal, commenced at the catamenial period, the catamenia being delayed. We agree with Dr. Barlow in opinion, that if it be true, as cases of this kind appear to render probable, that peritonitis may result from ovarian irritation, "the fact is important, not only in a pathological, but also in a practical and prophylactic point of view."

With reference to the puerperal form of peritonitis, Dr. Barlow with much reason insists upon the necessity for caution on the part of accoucheurs in coming in contact with persons suffering from infectious diseases—particularly erysipelas and scarlatina—lest by means of their clothes or hands they become the vehicles of poison to their parturient patients. Every practitioner of midwifery, too, ought to be wary of making post-mortem examinations. A late friend of ours, engaged in large midwifery practice, received a coroner's order to examine the corpse of a fever patient, which he unhappily did with his own hands. He then went home, had a shower-bath, and changed all his clothes. In the course of the day he attended two women in labour, both of whom were soon seized with puerperal fever, and both died. The husband of one of these women caught erysipelas, which resulted in suppuration within the elbow joint; and her daughter got erysipelas, and died. The influence of cadaverous matter in producing puerperal fever, and the efficacy of chlorine as a disinfectant, are remarkably shown in the experience of the Lying-in Hospital at Vienna.* In this hospital there are two divisions; in one the patients being attended by midwives, and in the other by physicians and their pupils; and it was found that the mortality from puerperal fever was always greater—even four times greater—in the latter division than in the former. Dr. Semmelweis at length recognised the fact that both he and the students frequently made post-mortem examinations; that the cadaverous smell on their hands, in spite of repeated washings, did not disappear until after a considerable time, and that the pupils not unfrequently proceeded to the examination of women in labour immediately after dissecting a dead body. This was the only one of the probable causes of puerperal fever which either did not occur at all, or occurred only in a very limited degree, in the midwives' division. Dr. Semmelweis now acted upon this information. He recommended all students frequenting the division not to handle dead matter, or if they did, he forbade them to make any examination till the following day. And he directed all the students to wash their hands in a *solution oj chlorine, prior to and after every examination.* The result of these precautionary measures was that *the number of deaths at once fell to the usual average of those in the midwives' division.* We trust that few practitioners of midwifery are ignorant of these facts, or unaware of their immense practical importance.

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* Amongst several errors of the press which have escaped the author's correction, we observe that in the page (423) from which the above extract is taken, the word jejunum is in *two* places misprinted *jenum*.

Dr. Barlow has given much attention to the subject of obstruction of the bowels, and particularly to the diagnosis of the seat of obstruction. We are indebted to him for having been the first to direct attention to the assistance which may sometimes be derived from observing the amount of urine secreted in these cases. He says:

> "The condition of the urine is most important as regards the diagnosis of the seat of the obstruction. When the obstruction is high up, as in the duodenum, the quantity of urine is so small, or rather the suppression so complete, that cases of this kind have been mistaken, and that, too, by men of experience, for ischuria renalis; whereas, when the stoppage has been very low down, as in the sigmoid flexure, for instance, the urine is abundant and clear. In the first case the suppression of the urine may in a great measure be accounted for by the sickness; it may be observed, however, that sickness, as ordinarily observed, independent of mechanical secretion near the stomach, never suppresses the secretion so completely. In the case of obstruction in the descending colon, as there is no sickness, and as there is delay in the passage of its contents along the intestinal tube, there is abundant opportunity for absorption; and therefore, from the large quantity of fluid taken up by the veins, the quantity which passes out becomes large." (p. 450.)

Another circumstance which deserves investigation when we would ascertain the character or seat of the obstruction, is the degree of contraction of the lower bowel when carefully explored by a bougie, or the œsophagus tube.

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* See Dr. Routh's interesting and instructive paper: Medico-Chirurgical Transactions, vol. xxxii.; also, Liebig's Familiar Letters on Chemistry, third edition, p. 530.

> "When sudden obstruction takes place in any part of the alimentary canal, it generally happens that the whole of the bowel below this part speedily and even forcibly empties itself; and this perhaps applies more particularly to the small intestines, where obstructions, when they do occur, are generally from twisting, intussusception, or acute inflammation. This produces a forcible contraction of the rectum, such as in one instance to have induced a doubt of the diagnosis of obstruction high up in the small intestines, from the fact that it was almost impossible to introduce the tube, or even the finger, into the rectum, so forcibly did the bowel contract through its whole course below the closure. And the same thing occurs from sudden occlusion, or sudden stoppage from inflammatory affection in the large intestines. . . . . When, however, the stoppage takes place from chronic thickening or contraction, or from malignant disease, occurrences which are more likely to take place in the large intestines, the bowel often loses its contractility below as well as above the seat of obstruction; and therefore the rectum, upon examination by the finger, will often be found dilated; so that upon the introduction of the œsophagus tube it will often coil upon itself in the pouched bowel, which may lead to a belief that it has passed a considerable distance up the canal." (p. 450.)

Passing over a chapter on Muco-enteritis, Tabes mesenterica, Diarrhœa, and Dysentery, we come to one on Diseases of the Kidneys, which, considering the great importance of the subject, we are compelled to pronounce less complete and satisfactory than any other part of the book. Under the general term Nephritis, Dr. Barlow includes all the forms of inflammation of the kidney, without attempting to distinguish them from each other; and although he admits that "the diagnosis of nephritis is not without its difficulties," he makes no allusion to the microscopical appearances in the urine in connexion with this form of disease.

The subject of Bright's disease then comes under consideration. Of this disease the author says:

> "If all the fatal cases of heart disease and disease of the brain originating from albuminuria or Bright's disease, be taken fairly into the account, it will be found that this disease is second only to phthisis in the number of lives which it destroys." (p. 478.)

Dr. Barlow then adverts to the minute structural changes in the kidney in the following remarkable sentence:

> "Into the minute changes which constitute the varieties of Bright's kidney it is not our province very minutely to enter, not because those changes are in themselves devoid of interest in a practical point of view, but because their description, belonging more to pathological anatomy, would refer the reader to the 'Pathological Anatomy' of Drs. Jones and Sieveking." (p. 478.)

Now the power of distinguishing the different forms and stages of Bright's disease by means of a chemical and microscopical examination of the urine, is so intimately dependent on a correct appreciation of the minute structural changes which the kidneys themselves undergo, that it is impossible to separate these subjects in any account of renal disease which is to be available for the practical purposes of diagnosis and prognosis. It is therefore not surprising that Dr. Barlow, being content to refer his readers for a description of the pathological anatomy of the kidney to another source, without himself making use of the excellent materials which are there ready to his hand, should have failed to write in a satisfactory manner on the different forms of Bright's disease, and the means of distinguishing them.

Without attempting to point out all that is incorrect and defective in the account which Dr. Barlow has given of the pathology and diagnosis of Bright's disease, we may refer to the following sentence, as an illustration of his mode of treating this subject. Speaking of the contracted form of Bright's kidneys, he says:

> "There is no deposit in the tubes, but the degeneration consists of a large increase of fibrous tissue, which, by its subsequent contraction, strangulates and atrophies the secreting cells, much as in the case of advanced cirrhosis of the liver." (p. 481.)

Now, not to insist upon the doubt which we entertain as to this being the true account of cirrhosis of the liver, it is entirely inaccurate so far as it relates to the kidney. There *is* a deposit in the tubes in this particular form of disease; as may easily be shown, not only by a microscopical examination of the kidneys after death, but by the almost constant appearance of tube-casts in the urine during life. The primary change in the kidney affected with this form of disease is a desquamation and crumbling of the gland-cells which line the tubes. The disintegrated cells appear in the urine in the form of "granular tube-casts." The tubes being thus deprived of their epithelial lining, waste, and at length many disappear; meanwhile the meshes of the matrix which contain the atrophied tubes become narrowed, and their fibres appear relatively thicker; hence the notion that "a large increase of fibrous tissue" is the essence of the disease. These changes, and others of much interest affecting the bloodvessels of the kidney, which our limits do not permit us now to describe, are all easy of demonstration, and therefore cannot safely be made the subject of vague surmise or random assertion by any author who has a regard for his reputation.* We are happy in being able to state that, with respect to all that relates to the treatment of Bright's disease and its complications, Dr. Barlow has given plain and sensible directions.

In the chapter on Urinary Deposits, which follows that on Renal Diseases, we have marked for the author's correction in his next edition the following passage relating to the means of distinguishing between urine which is alkaline from ammonia, and that which is rendered so by a fixed alkali.

> "Ammoniacal urine does not at first affect blue test-paper, but when the paper has dried it becomes red. This is not the case with urine alkaline from fixed alkali." (p. 514.)

This is a somewhat perplexing statement, but it is probable that the author intended to write to this effect: Reddened litmus paper has its blue colour restored by immersion in ammoniacal urine; but when the paper is again dried, especially by artificial heat, the ammonia escapes, and the red colour returns; whereas red litmus is rendered permanently blue by a fixed alkali, even though the paper be warmed and dried.

The diseases of the nervous system are next treated of; three chapters of considerable length being devoted to the following important diseases: Inflammatory Disease of the Encephalon; Delirium Tremens; Mania; Apoplexy; Paralysis; and Spasmodic Affections. Under this last head are included not only Chorea and Epilepsy, but also Asthma and Colic. With regard to the treatment of those cases of delirium tremens which are the result of mental excitement and anxiety, the author insists upon the necessity for great caution in the use of opium, and intimates that calomel and henbane will be the best internal remedies in such cases. We admit the necessity for caution in prescribing opium for any form of nervous disease, but we doubt whether its use is more hazardous or less beneficial in cases of delirium resulting from mental excitement and anxiety, than in the delirium of drunkards. We have seen the happiest results from opium in the former class of cases, and have not witnessed any ill effects from its cautious use, beyond the temporary nausea and inconvenience which an opiate occasions in a certain proportion of patients, of what kind soever may be the disease for which that drug is prescribed. We believe, however, that the use of opium requires extreme caution in cases of delirium—whether the result of intemperance, or of over-work, or anxiety—which are accompanied by great prostration of strength, with a feeble, fluttering pulse, and a tendency to syncope. In such cases, the heart being perhaps ill-nourished, soft, and flabby, we believe that opium may have an injurious influence—not as a narcotic, but as a sedative acting upon the feeble heart. The wakefulness and delirium continue unabated, the pupils contract, a cold sweat bathes the skin, and the patient rapidly passes into a state of hopeless collapse. In treating a patient threatened with such a group of symptoms, we should withhold opium, and give alcoholic stimulants, with a liberal allowance of beef-tea or other nourishment.

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* We may refer those of our readers who are interested in the question of the forms and subdivisions of Bright's disease, to two articles in previous numbers of this journal, the first, Jan. 1853, p. 56; the second, Jan. 1855, p. 122.

Dr. Barlow gives a very full and complete account of the causes and pathology of apoplexy and paralysis. The following extract is a fair specimen of his mode of treating this important subject:

> "If we endeavour to analyse the causes of apoplexy, we find the immediate ones to consist of extravasation of blood into the substance of the brain, upon its surface or into the ventricles; pressure upon, or compression of, the substance of the brain, by determination of blood, or an undue quantity sent to that organ; disease of the arteries, generally of a large branch, intercepting or diminishing the supply of arterial blood to a large portion of one hemisphere, often producing, or attended by softening of some portion of the nervous substance, from impaired nutrition; poisoning of the blood circulating in the brain, by retained secretion, as in the case of uræmia; and as a doubtful cause, we may add, simple loss of power by the brain, or a portion of it, constituting the true simple apoplexy of Abercrombie; but we regard this cause as doubtful, since it is difficult to find unexceptionable instances from which all the other causes have been eliminated." (p. 551.)

To this catalogue of the causes of apoplexy may be added, a diminished supply of blood, and consequent softening of the brain through the *accidental* obstruction of an artery: as, for instance, in the remarkable case of dissecting aneurism of the aorta, innominata, and right carotid, recorded by Dr. Todd;* and the cases related by Dr. Kirkes,† in which softening of a portion of brain resulted from obstruction of a branch of the middle cerebral artery, apparently by fibrinous coagula which had been detached from the valves of the heart, and thence carried onwards with the circulating current.

With regard to the causes of cerebral hæmorrhage, the author proceeds to say:

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* Medico-Chirurgical Transactions, vol. xxvii. p. 301.

† Ibid., vol. xxxv. p. 281.

> "If we still further pursue the train of causation, we find that, as the extravasation must have proceeded from ruptured vessels, this rupture may have arisen in one of two ways—either the vessels may have been subjected to an unusual amount of distension, or, in other words, the blood may have been too forcibly injected into them, or the vessels themselves may have been diseased." (p. 552.)

Again, the causes of over-distension of the vessels are various. 1. The injecting force of the left ventricle may be excessive, in consequence of hypertrophy of its muscular walls. The author believes that too much importance is attached to this as a direct cause of sanguineous apoplexy. He argues, that the hypertrophy being a conservative change, is only sufficient to overcome the impediment to the flow of blood, which is occasioned perhaps by disease of the aortic valves, or of the large arteries. He admits, however, that in cases of regurgitant disease of the aortic valves, the jerking motion of the blood, which is indicated by the peculiar "water-hammer" or "splashing pulse," may occasion an unusual strain upon some of the remote vessels, and that laceration may result from this.

We would direct attention to another class of cases in which it is probable that one element in the causation of cerebral hæmorrhage is over-distension of the minute vessels by an hypertrophied left ventricle. We allude to cases of chronic renal disease, in which, as is now well known, hypertrophy of the left ventricle is often found to exist unassociated with disease of the valves or large arteries. The explanation of the hypertrophy in these cases is to be found in the interesting fact, that when the blood contains an excess of excrementitious materials, such as urea, carbonic acid, &c., it is impeded in its passage through the minute vessels. The heart therefore being impelled to increased efforts, in order to overcome the resistance thus occasioned, undergoes a corresponding degree of hypertrophy. Now, although the increased power of the left ventricle may only just suffice for the extra labour imposed upon it, and although the blood may pass through the capillaries of the brain with only the usual force and speed, yet it is obvious that there must be unusual pressure and strain upon those portions of the vascular system which lie between the hypertrophied left ventricle and the seat of the above-mentioned impediment—probably the minutest arteries or the capillaries; and that this increased strain upon the vessels will be attended with a risk of rupture and extravasation. The liability to cerebral hæmorrhage in cases of chronic renal disease with hypertrophy of the left ventricle, is still further increased by the frequent occurrence of degeneration of the walls of the minute bloodvessels—a result, probably, of the impoverishment and contamination of the blood, engendered by the renal disease. It follows, from a consideration of the important facts to which we have thus briefly adverted, that no pathological history of a case of apoplexy or paralysis can be complete without a careful examination of the urine, with a view to ascertain the presence or absence of renal disease.

After speaking of increased injecting force in the left ventricle, Dr. Barlow thus describes another mode in which the vessels of the brain may suffer from over-distension:

> "On the other hand, we may have a delay in the return of the blood through the veins, arising from obstruction in the pulmonic circulation, whether produced by disease of the mitral valve, or the lungs, or air-passages, especially the latter,

as in the case of chronic bronchitis; but in such cases the obstructed circulation through the veins, though it must in time be propagated to the arteries, and does in some instances give rise to laceration and extravasation, yet when it is the cause of apoplexy, it is so more commonly by means of pressure from engorgement of the vessels of the brain." (p. 552.)

We would add to these observations the remark, that one element concerned in the production of apoplectic symptoms in the cases referred to, is the narcotic influence of carbonic acid, when an accumulation of that gas occurs as a consequence of defective aëration of the blood—the result of pulmonary disease. The author gives minute directions as to the treatment of apoplexy; and the following sentence—with the purport of which we entirely concur—may be looked upon as the text of his remarks upon this subject:

> "There has been no more prevalent or dangerous error, nor one which has more slowly yielded to the increased accuracy of modern pathology, than, that all cases of apoplexy are to be met with active depletion and other powerful antiphlogistic measures; whereas, from what we have seen, the apoplectic disease may arise from such very different causes, and be connected with such very different conditions of the system, that it is obvious that the same remedies cannot be applicable to all." (p. 558.)

The remaining chapters of Dr. Barlow's book are on Intermittent and Continued Fevers; Eruptive Fevers, with which is included Erysipelas; Epidemic Cholera; Influenza, and Hooping-Cough; and, lastly, under the head of Diseases of Adolescence and Puberty, a brief account is given of Delayed Development, Amenorrhœa, and Hysteria.

With regard to the question of the identity or non-identity of typhus and typhoid fever, a question the interest of which has been greatly enhanced by the laborious and accurate observations of Dr. Jenner, the author expresses his opinion in the following terms:

> "The conclusions which it appears we may most legitimately draw from our present information upon the subject is (*sic*), that in the fevers in which the mulberry-coloured and livid spots are present, there is a greater tendency than in others to assume the low sinking form, and perhaps a greater liability to head affections; but that nevertheless there may, and frequently does, occur severe bowel irritation, with inflammation and ulceration of the lower portion of the ileum. When there is the rose rash, on the other hand, there is almost always great bowel irritation, and not such early depression from the effects of the poison; but the frequency with which one form of the disease has been found to occur side by side with the other in many epidemics, though it may not have done so in all, and the almost imperceptible differences by which they appear to be distinguished, in some instances, seems at present to preclude the belief that they are specifically different." (p. 626.)

The author's account of the pathology and treatment of epidemic cholera may be summed up in the brief statement, that all the symptoms of collapse are due to the drain of water from the blood; and that the primary object of treatment is to check the diarrhœa. We trust that before Dr. Barlow is called upon to prepare a second edition of his work, he may have the time and inclination to study some of the published facts and arguments, which tend to show that his account of the pathology of this disease is inaccurate, and the treatment which he recommends not so certainly beneficial as he appears to suppose.

And now, having endeavoured to give a true and faithful report of Dr. Barlow's labours, and having already expressed our opinion of the general merits of his book, we purpose to notice some of the topics treated of in the second work whose title appears at the head of this article.

Dr. Dickson, the author of this work, having been, as he states, a teacher of medicine for thirty years, and a student more than forty, and having published several text-books and other volumes upon medical subjects, intends the present volume as an aid to young men who have engaged in the study of medicine, to physicians who have recently assumed the responsibilities of practice, and to his fellow-professors of the institutes of medicine, who have felt the difficulty of communicating to the first two classes the knowledge which they are earnestly seeking to acquire. It was necessary, he says, that the book should be compendious, yet that it should contain everything essential for a fair development of the subject, and that it should be written in a simple and easily intelligible style.

Of the two parts into which the work is divided, the first is devoted to the subject of "General Pathology," and the second to "Special Pathology and Therapeutics." Under the head of the incidental causes of disease, the author makes the following allusion to some special sources of dyspepsia amongst his countrymen:

> "In this country it may be affirmed that we have the worst possible cookery, and hence indigestion or dyspepsia prevails almost as a national infliction. But our custom of hasty eating, and our almost universal habit of chewing tobacco, tend also to the same result. Mastication, the instinct to perform which seems to be lost, in the children of civilized parents at least, should be taught to every child when its teeth begin to present themselves." (p. 35.)

In a subsequent page, the injurious effects of tobacco are again referred to.

> "Smoking and snuffing are common both in the New and in the Old World; but it is only in the United States that chewing is a habit of civilized life, extending itself even among refined gentlemen. The mischief done is not perhaps as obvious or direct as might be anticipated; but there cannot be a question that much of the impairment of digestive power and of animal vigour secretly felt, and of the integrity of the nervous system, so often complained of, must be ascribed to these unfortunate national customs." (p. 47.)

Dr. Dickson, remarking that man is the only animal who drinks while eating, suggests the question whether this habit is not hurtful, by diluting the fluids of the stomach, and especially whether the drinking of very cold liquids is not likely to be injurious; and he observes, with much reason, as we think, that those who drink iced water or champagne immediately after hot soup, run a risk the consequences of which they probably will not always evade.

The following remarks on the subtle nature of animal poisons, we submit for the consideration of those gentlemen who, with praiseworthy diligence, have endeavoured, by the aid of chemistry and the microscope, to detect the cholera poison in the air, or in water, or in morbid excretions.

> "In every contagious morbid poison, some new and peculiar result has followed the combination of the elements which go to constitute it, which the highest

magnifying powers of our microscopes have not hitherto shown us, nor our nicest chemical analyses prevailed to detect, any more than they have apprehended or made manifest the odorous particles of musk or rose. If this be true of the fixed and palpable contagions [as, for instance, the variolous poison], what shall we say of the tenacity of those which are denoted as impalpable, which offer to us nothing tangible, but confound us by their invisible potency, and avoid all our means of circumscription and limitation?" (p. 56.)

In a section on the *seats of diseases*, the author makes a passing allusion to the former vehemence of discord between humoralism and solidism; then observing that the actual commencement of morbid change, whether in the solids or fluids, can rarely be made palpably manifest, but that we may be led to a very satisfactory inference concerning this matter by certain familiar considerations, he thus happily illustrates the subject:

"From the great storehouse of the circulating mass of the blood must be built up all the solid tissues of the body—must be formed all the secreted fluids—must be separated all the excretions, properly so called. But the blood itself must, in its turn, depend for the integrity of its composition upon the action of the tissues, whose condition is of necessity a modifying element. The ultimate source of their functional power, their special capacity, we trace to the influence upon them of the great nervous centres. Every part of every tissue depends immediately upon its nerve, and the blood sent to it for its life; and these, the nerves and blood, are mutually dependent upon each other. . . . . . But both the blood and the solid tissues are liable to impressions from without, which materially modify their condition. The former may be directly poisoned by the entrance into, and admixture with it, of many injurious agents, some of which may be detected and exhibited. It is often indirectly poisoned by the influence of contingencies which prevent the elimination of such effete matters as must be got rid of to keep it in a normal condition. We have reason to infer the existence in it of injurious ingredients, whose presence we cannot demonstrate, by the ultimate results. The blood may thus become, so to speak, passively diseased. The solids may be acted on mechanically or chemically, and disintegrated or broken down. But a diseased condition in any solid tissue implies activity, a reaction, as it is called, which may vary indefinitely with the varying nature of the agent that causes it. The capacity for such reaction depends absolutely upon its nervous and vascular connexion and supply continuing uninterrupted; and hence we are led, I think inevitably, to the conclusion that the blood and the nervous tissue are the primary seats of disease." (p. 110.)

The *tendency of disease* forms the subject of one section, the author expressing his conviction that the tendency of all forms of disease is essentially to death,—death, either of a part or of the whole of the body, according as the morbid affection has been general or local. The announcement of this opinion, he says, will surprise those who have received the ancient and plausible doctrine of a *vis medicatrix naturæ*, which he denies. But it is evident that this is a mere dispute about words, for he admits that an organ has the power of recovering its normal condition when the cause which disordered it has ceased to act. As the disease to which we give a name has no existence apart from the body, so neither has the *vis medicatrix* a separate existence. A supposed morbid poison induces that condition of the body to which we apply the term scarlatina; and that a body thus morbidly affected has an active tendency to revert to the condition of health, as a cut finger has to re-unite and to heal, can scarcely be denied by any one whose mind has not been perplexed by a theory which obscures his view of the facts.

Dr. Dickson concludes the first part of his treatise by expressing a wish that civilized and Christian nations would resume the ancient classical practice of burning the bodies of the dead. After referring to the noxious emanations from the crowded graveyards of the Old World, and expressing a fear lest similar evils may ere long exist in the burial-grounds of some of the American cities, he suggests that it would be far better to substitute the polished vase, the marble urn, for the cold and clammy clay and the noisome graveyard; and he intimates that the relics of "all that our souls held dear," might thus become the inmates and ornaments of our habitations.

Passing on to the second part of Dr. Dickson's treatise, we find a definition of *therapeutics* which every student and every practitioner would do well to remember.

> "Therapeutics comprise the whole management of an attack of disease: the regimen, the physical and moral control, nursing, &c., as well as the administration of medicines. Indeed, this general and comprehensive superintendence is often of far more importance than the mere pharmaceutical appliances and means employed. Voltaire's sarcastic definition of the 'practice of physic' as 'the art of pouring drugs, of which we know little, into a body, of which we know less,' is a most unjust reproach when applied to the modern scientific physician, of whom prudence is the peculiar attribute—*nullum numen absit si sit prudentia;* and who believes, with Chomel, and acts upon the belief, that the first duty of the practitioner is to take care that he does his patient no injury in his efforts to benefit him." (p. 172.)

In the classification of diseases the author follows a physiological arrangement, describing—1st. Those which affect the Circulatory, or Vascular System; 2nd. The Digestive System; 3rd. The Respiratory System; 4th. The Sensorial System; 5th. The Motory System; 6th. The Excretory System; and 7th. The Generative System.

The large and important class of fevers which are included under the first head are described at considerable length. The subject of malaria receives full consideration, the author's long practice in a malarious district having afforded him abundant opportunities for studying this source of disease. His account of the various types of fever abounds in quotations and references to authorities, both ancient and modern; but his descriptions sometimes want the precision which would render them valuable for deciding a disputed point—as, for instance, the identity or the difference of typhus and typhoid fever.

The following passage on the use of stimulants in fever shows, as we think, the practical good sense of the author:

> "I avow, for my own part, that when I see the respiration hurried and impeded by debility, the pulse flagging, the skin covered by a cold and clammy exudation, I do not entertain any very fastidious scruples as to the ulterior effects of my stimulants. I am only afraid of finding them inefficient to act upon the little remains of excitability present. The objections which have been so obstinately urged against them originated, surely, in the logical essays of the closet, not in sick rooms or in hospitals, not at the bedside of the debilitated and the dying." (pp. 240-1.)

The extracts which we have given will doubtless, as we intended they should, convey the impression that there is much in Dr. Dickson's work which is excellent. Yet we are bound to express our opinion that the book, as a whole, very imperfectly fulfils the conditions required in a text-book on the practice of medicine at the present day.

In order to justify this opinion, we need only refer to the author's account of the physical signs of disease of the lungs and heart. This is unquestionably a subject of great interest and importance, yet we find not only that it is treated with extreme brevity, but that in several instances the physical signs are so inaccurately described as to have impressed us with the conviction that the author's practical acquaintance with the subject must be very limited. For instance, the physical signs of phthisis are described in eleven lines, and a part of the description is, that "when a vomica is formed, and a cavity more or less emptied, we have *resonance* and pectoriloquy;" the author evidently supposing that resonance on percussion over a tuberculous cavity is the rule, and not, as we believe, a rare exception to the rule. Apparently, too, he has the same idea with respect to the third stage of pneumonia, for in describing the signs of this disease he says—"When purulent matter is spit up, we hear the mucous râle, or a gurgling; there is *restored resonance on percussion*, but not the respiratory murmur," &c. (p. 606.)

We extract entire the author's account of the means of distinguishing chronic bronchitis from phthisis:

> "The distinction between chronic bronchitis and tuberculous phthisis is often difficult. In the latter there is less crepitus, or râle, less soreness of the trachea and thorax, more tendency generally to hæmoptysis, and less expectoration in the early stages. In their advanced progress, we can draw no line between them, except from their previous history." (p. 601.)

The whole description of the physical signs of acute bronchitis is contained in the following sentence:

> "Resonance upon percussion is dull, and the respiratory murmur is impaired very generally over the thorax." (p. 599.)

Nothing, surely, can be more unsatisfactory than this mode of dealing with an important subject which admits of being treated with scientific accuracy.

The author believes that the frequency of Disease of the Heart has been exaggerated; he is aware that it is customary among pathological writers to treat of cardiac diseases as of very common occurrence, but, he says,

> "I am persuaded that, except the symptomatic and transient disturbances with which the profession has always been familiar, they are comparatively rare—at least in our own country, and in its southern portion." (p. 337.)

After quoting the statements of Watson, Latham, and Bouillaud as to the frequent association of cardiac disease with rheumatism, he says:

> "After nearly forty years' practice, I can truly assert that I have seen but two cases of rheumatism terminate in serious palpable disease of the heart." (p. 338.)

And he cites the opinion of Professor Wood, of Pennsylvania, who says that he knows of only one case of incurable heart disease which has resulted from inflammatory rheumatism, under his own care, in persons above the age of puberty. Dr. Dickson suggests, that one cause of the comparative rarity of this complication of rheumatic fever in America may be "the larger and freer use of opium." We confess, however, that we entertain much doubt as to the author's practical skill in auscultation, and therefore as to his power to detect the disease which he believes to be so rare. In describing the normal sounds of the heart, he says that one cause of the *first* sound is "the *opening* of the mitral and tricuspid valves." This may possibly be a misprint, but it is not in the list of *errata*. The following is the entire description of the physical signs of pericarditis:

> "The physical signs are—dulness, at first from turgescence; the friction sound, as of two rough surfaces rubbing together, owing either to dryness or to shreds of membrane on the inner face of the sac; then perhaps creaking, or the *cri de cuir;* effusion of serum soon dulls all noises, and does away the friction sound." (p. 341.)

Our doubts as to the alleged rarity of heart disease in America are strengthened by a perusal of the author's account of dropsy, of which he says:

> "In the bills of mortality published in our country, it will always be found to occupy a conspicuous place, presenting annually an average number of victims inferior to that of few other maladies. In frequency of occurrence and in difficulty of cure it is alike remarkable." (p. 389.)

We turn then with some interest to the chapter on Diseases of the Kidney, and there we find it stated of Bright's disease—the whole account of which occupies less than a page—that "it is happily not often met with among us in the south." (p. 739.) It would appear, therefore, from this statement, that the frequency of dropsy cannot be due to the common occurrence of renal disease. Indeed, the author appears to doubt whether there is any special relation between renal disease and dropsy; for he says, with reference to the coagulability of the urine in connexion with dropsy: "For my own part, I have not been able to draw from it any clear or positive inferences, either as to the nature of the attack, its causes, or its remedies." (p. 389.) Our next resort, then, in searching for the cause of the frequent occurrence of dropsy, is to the Liver, and especially to *cirrhosis* of that organ. Of this disease, again, the author says: "It is not, I think, often seen in our country" (p. 559); and his brief and inaccurate description of its morbid anatomy suggests the conclusion that he has had few opportunities for studying the disease.

It appears, therefore, that dropsy is a very common disease in a country where diseases of the heart and kidney, and that particular disease of the liver which is most commonly associated with dropsy, are reported to be rare. Yet the author, in the course of his long history of the various forms of dropsy, mentions no causes of that disease which can be considered peculiar to America, except it be a habit of dirt-eating amongst the blacks, which, he says, is connected with a species of cachexy on which anasarca is an almost constant attendant. After a careful perusal of Dr. Dickson's account of the causes and pathology of dropsy, we have arrived at the conclusion that it represents much more nearly the knowledge of these subjects which was possessed by the profession thirty years ago, than the knowledge of the present day. It is much to be regretted that this antiquated and inaccurate pathology should have found a place in a text-book intended for the use of students and young practitioners; for assuredly modern pathology is more simple and intelligible, and a better guide for those who are learning that difficult art which has for its object the prevention and cure of disease.

George Johnson.

REVIEW VI.

Chirurgische Klinik. Beobachtungen und Erläuterungen in dem Gebiete der Chirurgie. Von Dr. HERMANN FRIEDBERG, Docenten der Chirurgie und Staatsarzneikunde an der Friedrich Wilhelm's Universität in Berlin. Erster Band, mit 23 Tafeln Abbildungen.—*Jena*, 1855. pp. 320.

Clinical Surgery. Observations and Explanations in the domain of Surgery. By Dr. HERMANN FRIEDBERG. Vol. 1, with 23 Plates.

THE volume before us is the first of a promised series, which the author designs for a repertory of surgical cases and observations. It is pretty exactly of the nature of our 'Hospital Reports,' and is made up of very elaborate records of groups of surgical cases, with such practical observations as happen to be suggested by them. It possesses, however, one advantage over the 'Reports' of our own country,—namely, that of being somewhat profusely illustrated with plates. There are in all 12 cases, and their various circumstances are exhibited in as many as twenty-six drawings. It is plain that the limner's art has yet more assistance to render to the medical naturalist; by increased rapidity and cheapness in the execution of convenient drawings, it may illustrate far more extensively than heretofore, or perhaps supplant, his lengthy verbal descriptions of many objects, and thus materially advance both the domain of science and, as Dr. Friedberg's book shows, the comfort of its cultivators.

Of 5 cases of *Reparative Surgery*, 3 rhinoplastic, and the others blepharoplastic, we select the following:

CASE I. Maria W., aged thirty-two, presented herself in the following condition ten years after having been infected with syphilis.

"The entire nose was destroyed, and an oval opening in the middle of the face existed in its stead. The surrounding soft parts were tumid, and seamed with scars, and they had been drawn into the opening during the healing of the foregoing ulcers. So considerable was this traction, that the upper lip was much shortened, and raised at its middle into an acute angle, and the inner extremities of the eyelids were drawn inwards. The septum was gone, and portions of dead bone were visible in the exposed nostrils as far back as the body of the sphenoid. Small ulcers, still unhealed, remained in various parts of the nasal cavity. The patient had no sense of smell, and never sneezed. Tickling the walls of the cavity with a feather, and the contact of the vapour of strong ammonia, produced no effect. She had epiphora, and was deaf.

"As soon as the ulcerations had healed, Dr. Friedberg transplanted a large flap from the forehead, and constructed a new nose in the usual method. The septum, however, was made of unusual length, in order to secure the replacement of the upper lip, after the complete division of its adhesions with the lower edge of the nasal opening. The flap adhered everywhere, and became a tolerably comely nose; some sense of smell returned, and when the vapour of the caustic ammonia was inhaled, the pupil and eyelids contracted, and the tears flowed. The epiphora also ceased, and the deafness diminished. About five months after the operation the patient ejected a dead piece of the sphenoid bone through the mouth, and had a fresh attack of periostitis in the sternum. Her general health, however, was much improved." (p. 3.)

The issue of this case may serve to show that the rule which precludes reparative operations during the continuance of the constitutional affection, is not absolute. But although important local advantages were no doubt obtained by the reconstruction of the nose, and by the protection thus afforded to the thinly-clad bones within, yet it was at the risk of failure in an operation which would at a later period have been less uncertain of success.

The case further illustrates a point of much importance in tracing the history of cases of syphilis. The genital organs presented no indication of the previous existence of a syphilitic ulcer. Should it then be inferred that no primary syphilis or a mere blennorrhœa had preceded such extensive and severe tertiary disease? Let such instances as the following reply:—A woman had an indurated chancre on the anterior lip of the os uteri; fourteen days after it had healed, its place was occupied by a smooth, slightly red, unseamed scar, with raised edges. In another month, every trace of it was gone. Four or six weeks sufficed to obliterate superficial ulcers of the tonsils and soft palate, whilst those which healed slowly, or which penetrate the sub-mucous tissue, left permanent marks behind them. The scar indeed remains in all cases, but is perceptible only under certain circumstances. Thus a man, aged thirty-eight, had gonorrhœa, a chancre within the prepuce, a second on the left side of the glans, and a third within the urethra. The sore on the glans was well in three weeks, but the others had not all healed until six months after their first appearance. Eighteen months afterward he became again infected, and had phymosis, with considerable swelling. During the swelling, the scar of the original ulcer of the prepuce was distinctly visible, and of a pale, dull-white appearance. The man then died of delirium tremens, and after death the scar on the glans was imperceptible.

Another point of interest in the case is, the restoration of the functions of the olfactory nerves and nasal fibres of the fifth, soon after the closure of the unnatural orifice of the nose. The observation is one not commonly made, and the explanation is consequently uncertain; but it seems most probable that the loss of sensation in those parts of the Schneiderian membrane which had not been ulcerated, arose from their extreme dryness in the large current of air to which they had been exposed. So delicate is the layer of tesselated epithelium which clothes the nasal cavity, that it is apt to be useless even upon slighter occasions than that of the loss of the nose. A faulty mucous secretion, or a cessation of that which naturally keeps the epithelium free and moist, renders the latter incapable of retaining and transmitting to the extremities of the nerves the odorous materials which come into contact with it. Moreover, the current of air is undoubtedly no longer directed towards the upper and olfactory part of the nostrils when the inlet is much misplaced.

The author furnishes a few observations on the subject of the fall of the hair in cases of syphilis. Sometimes the hair is loosened at its root, in consequence of an inflammatory eruption of the scalp, which is attended with the detachment of a quantity of scurf, and with an exudation into the hair sacs; but at other times it falls without any previous affection of the skin, and the immediate cause of its fall is still unknown. The author denies that it is ever due to the use of mercury, since in that case goldbeaters should be particularly affected in this manner; and Rayer observed an abundant growth of hair in a goldbeater who was suffering from mercurial tremor, brought on in the course of his occupation. Dr. Friedberg adheres rather to the opinion, that the hair falls in consequence of being badly nourished by the blood of syphilitic patients; and he supports that opinion by the quotation of instances in which he discovered venous murmurs associated with syphilis and loss of hair, in persons who in other respects appeared to be in good health. We may state, however, that we have failed to discover a murmur under such circumstances.

The mode of operating in two of the reported cases of reparative surgery is worthy of description. It was suggested by Burow, and appears to have succeeded in the following instances.

"CASE II.—A girl had her nose and portions of both cheeks destroyed by lupus exedens. The nose was restored in the usual manner from the forehead, and the gap in the left cheek filled by sliding forward a flap with a broad base; but the part to be extirpated from the right cheek was too large to be replaced in the same manner. The diseased spot was accordingly removed by three incisions, of which the inner, forming the base of the triangle, was inclined downward and a little backward, and was prolonged into the upper part of the neck. On the lowest part of this long incision, beneath the jaw, and on the side opposite to the triangular wound in the cheek, a second triangle was marked and excised; its form and size were the same as those of the upper triangle, but its position was reversed; and its base or outer side was cut on the prolonged line, equal to the base or inner side of the triangle in the cheek. After the excision of the triangles, two flaps were left, which were separated by the long incision, and formed by it and the nearest side of each triangle respectively. These flaps were then loosened from the subjacent structures, and were shifted, the outer upward, over the gap in the cheek, and the inner downward over that in the neck. The whole wound, when united by sutures, resembled the letter Z. With the exception of some ectropium of the lower lid, the case did well." (p. 37.)

The success of this operation appears to have depended on the mobility of the flaps, and on the careful measurement of the parts which were to be applied to each other. The bases of the two triangles being of equal length, the vertical edges of the flaps were necessarily equal; and the remaining sides of the two triangles being respectively equal, the edges of the two horizontal wounds were also well adapted to each other. The following case is an interesting modification of the same principle of operating.

"A child of four years of age having necrosis of a piece of the frontal bone, half-an-inch broad at the superciliary ridge, and extending for an inch up the forehead, had the whole piece removed by operation. In consequence of the disease and the operation together, the tarsal cartilage was completely everted, and its free edge bent up and adherent to the bone; the eyelashes and eyebrow were mingled, and the whole intervening depth of the eyelid was destroyed. The conjunctiva being exposed, was red and tumid, but was not large enough to cover the eye, and some strabismus and opacity of the cornea were the result. By a triangular incision the whole scar was excised, and the lid freed from its connexion with the bone; the base of the triangle was parallel with the free edge of the lid, and its sides passed on either side of the cicatrix, and met above it. To bring the sides of this triangle into apposition being impossible, its base was prolonged by incisions reaching outward to the temporal region, and inward to the nose. A triangular piece of skin was then excised at each end of the prolonged line, but on its inferior side; and the base of each triangle was formed, on the prolonged incision, of *half* the length of the base of the triangle, which was to be closed in the upper lid. The flaps on either side of the latter triangle were then extensively raised from the subjacent parts, and were brought together, whilst the sides of the two inferior triangles were likewise united by suture. After the union of all the parts together, their general appearance somewhat resembled a pair of scales." (p. 125.)

This case also did well.

The chapter on *imperforate anus* possesses considerable interest and importance. The numerous modifications of cases of this nature are systematically arranged, and some useful practical observations are associated with the descriptions.

Besides the ordinary defects of this region, in which either the anus is not formed, or the rectum imperforate to a varying extent above a natural anus, the author has collected a series of cases which illustrate their numerous combinations with other faults in the development of these parts. The defects just mentioned appear to be the natural conditions of the rectum and anus at certain stages of fœtal growth. But instead of these, or in combination with one of them, the rectum may be prolonged as a narrow dense tube, from a dilated part above to the skin behind or before the scrotum, to the urethra, the bladder, or the vagina, and may open by a small orifice at any of these points. Other more or less serious irregularities of development may be associated with these, such as hypospadias and spina bifida. In some cases a cutaneous outgrowth springs from the perineum, or from the usual site of the anus, and it might be supposed to indicate the spot at which the rectum terminates. But there is no necessary connexion between the two, and the presence of such a growth furnishes no assistance in determining the mode or the position in which to operate. A considerable narrowing of the pelvis constitutes a more definite and serious indication of the state of the parts within, for it is usually found that when the pelvic cavity is small, the rectum terminates within the abdomen. The association of a narrow outlet of the intestinal canal with hernia appears to be merely a mechanical result of the straining efforts at defæcation, and not itself a fault of development; for the same result is not unusually observed from a narrow congenital phymosis, and the rupture sometimes speedily disappears after the operation of circumcision.

In estimating the value of different modes of treatment, various circumstances are to be taken into consideration. The cases naturally divide themselves into two kinds. Those in which the anus is well formed may sometimes be relieved by passing a trocar and cannula through the imperforate part of the rectum, and life may afterwards be prolonged by careful and regular dilatations of the punctured orifice. Sometimes, however, this plan may not succeed, and at other times the circumstances of the case may forbid it. Thus, Amussat met with a case in which the vagina and rectum communicated below an imperforate portion of the latter. He was therefore compelled to construct an artificial anus behind the natural one, and to bring down the upper portion of the rectum to the margins of his incision. The girl lived and throve, and in the summer before last, at nineteen years of age, was about to be married.

If there be no anus, the puncture with a trocar may be said to be never advisable, as being insufficient to produce a permanent cure of the defect. When the rectum cannot be found after an incision in the natural situation, it is preferable to open the colon in the manner of Littre or Amussat, rather than to plunge the trocar deeper within the pelvis. There may be, as there has been, a question in these cases, whether the rectum should be sought from below at all, or some part of the colon should not rather at once be opened. Many cases on the Continent have been treated on the latter plan; but in this country, we believe, the artificial anus is invariably made in the natural situation. In some cases, indeed, it may be questionable whether the condition of the patient be really improved by opening the colon; whether, for instance, an anus in the groin or loin be preferable to a communication of the rectum with the vagina, provided the communication be sufficiently large to allow the evacution of the contents of the bowel. But there can be no question that an anus in the natural situation would be preferable to such a condition; and provided the mucous membrane of the rectum be brought into continuity with the skin of the perineum, there would seem to be no greater tendency to subsequent contraction in the perineum than in the groin or lumbar region. In some respects the two operations are of equal value, as, for instance, in their leading to the subsequent closure of the unnatural openings by which the rectum sometimes terminates. The author formed an artificial anus in the natural situation in a boy two months old; and at his death, three months afterwards, a narrow canal, by which the bowel had formerly terminated in the perineum immediately behind the scrotum, was obliterated. Desgranges, on the other hand, opened the colon of a girl of four years of age; and he states, that in eight months, fæculent discharges ceased to pass through a previous communication between the rectum and vagina.

The author adduces one consideration of much weight in favour of thoroughly prosecuting the operation in the perineum—namely, the slight degree in which the constitutions of newly-born children resent operations. He is of opinion that surgeons sometimes give up a really feasible operation in the perineum, from an ill-grounded fear that the infant may not bear its completion; and he cites instances in which the operation was stopped, although the rectum was found after death to have been quite within reach. We will quote his own words. The general rule, as we have said, is a valuable one, and should be borne in mind; but the extent to which it seems to have been acted on, will suggest a doubt of the wisdom of the Prussian law, which renders a surgeon liable to an action at law for not operating enough—for omitting, for instance, to perform in an infant such an operation as that of Littre or Callisen.

"Before proceeding to estimate the value of the operation for constructing an artificial anus in the perineum, I must refer to the slight reaction which takes place in newly-born children after operations. There is a manifest difference in this respect between the two cases which I have described. In the child of twenty-two hours old, in which I made an artificial anus in the perineum, no reaction whatever could be perceived; neither fever occurred, nor an inflammation of the wound; and even the spot from which I removed the cutaneous outgrowth cicatrized after an extremely trifling suppuration. In the boy of two months, however, decided febrile symptoms took place, the edges of the wound inflamed, and suppuration appeared on the second day. This observation confirms the view which many other cases have led me to take, that children bear operations much better just after their birth than at a later period. The explanation of the fact is probably to be found in the low degree in which the independent life of the young creature is yet developed: and it presents an analogy with the condition of inferior animals; for in both there is but little tendency to suppuration, and there is a marked proneness to the form of healing by the first intention. . . . . Acting upon this view, I operated upon a child sixteen hours after its birth. The case was one of double harelip and cleft palate, and the condition of the parts was such that, besides paring the edges of the clefts, I was compelled to separate the soft parts from the whole of the intermaxillary bone, and not only to loosen the upper lip, and a great part of the right cheek, with the ala nasi, from the jaw, but also to make a longitudinal incision in either cheek, in order to relieve the tension. This operation is certainly so severe, that one might be induced to fear for the consequences, and yet the child was quite well after it, slept quietly, and sucked well; and on the sixth day the union was so far firm that I removed the last sutures, and substituted strips of plaster." (p. 199.)

The author's conclusion with regard to the site for the operation is:

"With the exception of those very rare cases in which the whole rectum is wanting, or its upper part, if only so much exist, adheres to the fundus of the urinary bladder, or terminates in a narrow canal, it must extremely seldom be impossible for a patient and careful operator to make the anus in the perineum; but should the operation be really not feasible, without doubt he must resort to that of opening the colon."

The whole of this chapter is valuable, and will amply repay perusal. The book terminates with some cases of tumours of bone and of the parotid region. With the exception of one of the latter, which was of colloid structure, they do not call for any remark; but that case, in the present incomplete state of our knowledge of colloid disease, we think worthy a brief notice.

"Amalie S., aged thirty, a large, well made, and married woman, of fair complexion and lively temperament, presented herself, with a tumour in the region of the parotid, of seven years' standing. She had in her childhood suffered from scrofulous enlargements and suppurations of the cervical absorbent glands, from eruptions on the scalp, and otorrhœa. At twelve years of age she had measles, and then a cough, which was attended with occasional hæmoptysis until she was eighteen years of age, and with copious expectoration, which did not cease until her twenty-second year. The catamenia appeared for the first time at that age, but the discharge was profuse, and always irregular.

"In the following year she married, and the menstruation became worse, the discharge continuing four or six weeks at a time. She was never pregnant. Soon after her marriage she accidentally discovered that a tumour had formed in the right parotid region, and had reached the size of a bean without occasioning her any pain. Four years afterwards it had become larger, and was the seat of smart shooting pains, which increased during the following three years.

"In November, 1852, the tumour was as large as the half of a small apple, hard, elastic, and unattended with pulsation, or with the enlargement of any of the cutaneous veins near it. The skin moved readily over it, but the tumour itself could not be dislodged from or moved in its bed between the jaw and the mastoid process. The surface of the growth was somewhat lobed. It was painful, but not tender.

"The whole tumour was removed by operation, and presented the following appearances. It was composed externally of a smooth, fibrous, and somewhat tough capsule, and was readily distinguishable from a portion of healthy parotid gland which had been removed with it. The general mass was disposed in rounded lobes, which were incompletely parted from one another by septa prolonged inward from the capsule. The principal portion of the interior of the tumour was arranged in granular growths, which corresponded to each of the lobes, and sprouted, cauliflower-like, into them; the remainder of their cavities was filled up by a fluid resembling synovia. Cavities of the same kind, and having similar contents, were found here and there within the substance of the growths themselves. The greater part of these growths was soft, granular, and of a whitish-red or yellowish-white colour; the lowest portion was somewhat like a strawberry, and separated from the capsule by a viscid fluid stained with blood.

"*Microscopic examination.*—Magnified 250 times, the substance which appeared to have been last formed, presented the appearance of a delicate open network of straight and waved connective fibres, intermixed with a few elastic fibres, and an abundance of capillaries. Countless, round, oval, and fusiform cells lay scattered amongst the connective fibres, and their delicate outline being but faintly illuminated, they produced a general gelatinous appearance of the surface, resembling that of a granulating ulcer. Many of the cells had biscuit-shaped and double nuclei, and were undergoing spontaneous fission, whilst others were passing into fibrous tissue. Here and there were spots in which the fibrous and cellular structures were wanting, and nothing but a viscid fluid, and a few solitary cells or flat clusters of cells, occupied the irregular spaces. It seemed as though the intercellular substance of the connective tissue had not become sufficiently firm to form a true tissue, but had occasioned the coalescence of the plastic cells, which swam freely in it.

"All stages of growth presented themselves in different parts of the tumour, from the gelatinous material just described, to the whitish, firmer, and granular tissue which constituted its chief mass. In this latter part the number of firm connective fibres was greater, and that of the recent cells was smaller, than in the softer portion of the growth; whilst the interspaces were smaller and closer. This granular soft tissue became, in some parts, an uniformly fibrous, firm mass, like fibroid, the densest fibrous cords co-existing in it with the smallest interspaces, and with few and small cells. From their general appearance it might be concluded that all the firmer parts of the tumour were the oldest, and that they were formed of substances which had once been similar to the softer portions.

"Some concentric laminated balls of colloid substance were scattered here and there in the tumour, which were peculiar from being, for the most part, cretified, and similar to the sand of the pineal gland. No trace of the parotid gland-substance was detected in the tumour.

"In respect to diagnosis, this tumour might be considered as one of cellular kind, if considered only as to its isolated, encapsuled exterior, and as to its granular lobed structure and network of connective tissue. Its small interspaces, filled with serous fluid and floating clusters of cells, showed some alliance with one of the forms of cancer, though not enough to determine its nature as cancerous. The larger spaces resembled those of cysto-sarcoma; but the construction of the tumour out of gelatinous material indicated its similarity to collonema." (p. 237.)

The patient did well, and remained so a year and a half after the operation. The state of the catamenia, however, continued as before.

Dr. Friedberg has, moreover, noted the effects of operations upon the temperature of the body, and has recorded his readings of the thermometer in the several instances of operation which he has described in the book. Beyond the fact, that during the febrile reaction succeeding an operation the thermometer indicates a rise in the temperature of the body, the only matter of interest is, that that rise is greatest after the extirpation of cancers, and is followed by a greater subsequent diminution of the heat of the surface in these than in other cases. Such a diminution might be expected from the observation of the author, that the temperature of the body is ordinarily greater in persons affected with cancer than in those suffering from other surgical diseases.

Charles H. Moore.

REVIEW VII.

1. *Handbuch der Speciellen Pathologie und Therapie.* Erster Band. *Allgemeine Störungen der Ernährung und des Blutes, Krankheiten des Bewegungsapparates.* Bearbeitet von R. VIRCHOW, J. VOGEL, und STIEBEL.—*Erlangen,* 1854. *Das Fieber.* Von RUD. VIRCHOW, Professor der Medicin in Würzburg.

Handbook of Special Pathology and Therapeutics. First Vol. *General Derangements of Nutrition and of the Blood; Diseases of the Organs of Locomotion.* By R. VIRCHOW, J. VOGEL, and STIEBEL.—*Erlangen,* 1854. *Fever.* By R. VIRCHOW, Professor of Medicine, Würzburg.

2. *On Pyrexia.* By E. A. PARKES, M.D., Professor of Clinical Medicine in University College, and Physician to University College Hospital. ('The Gulstonian Lectures,' 1855.)

A HOT skin, a quick pulse, intense thirst, scanty and high-coloured urine—to how many diseases are these symptoms common? They constitute a large portion of all definitions of specific fevers; they are important symptoms of all local inflammations. Fever—pyrexia—are the terms used indifferently to express such a group of symptoms. In local inflammations, we are accustomed to abstract, so to say, these symptoms from others proper to the special affection, and to speak, for example, of pneumonia and of the fever that accompanies it; while those diseases which are accompanied by these same symptoms, but unaccompanied by constant local lesions of grave moment, we term emphatically, Fevers. The fever or pyrexia is common to all. It is to the consideration of the intimate nature of this abstraction that the essay of Virchow and the lectures of Parkes are devoted.

"I am about to speak," says Dr. Parkes, at the commencement of his first lecture, "of fever in a general, not in a specific sense; I am about to abstract from various diseases, those pyrexial symptoms which are common to all; which, like shadows to substance, are necessary to the very existence of typhus, small-pox, plague, or pneumonia; but yet are not, *per se,* any one of these diseases."

"*Calor præter naturam,*" was Galen's definition of this abstract fever. Later physicians thought the definition insufficient; they knew that fever commenced with rigors; they thought they knew that at such time the temperature was lower than natural; and as they were satisfied that the disease, the fever, had even then commenced, they felt that some other symptoms must be included in the definition of fever—so a quick pulse was added to *calor præter naturam.* Subsequent observers noted that increase in the rapidity of the heart's action was a no more certain test of the presence of fever than was elevation of temperature, and so they added yet other symptoms to the definition. Still the insufficiency of all the definitions was pretty universally acknowledged, until De Haen, substituting the thermometer for the hand in estimating the temperature of the body, demonstrated that even during the rigors which precede the hot skin, there is an appreciable elevation of temperature, sometimes so much as 4° R. It now seems to be placed beyond doubt by the observations of Gierse, Roger, Von Bärensprung, Traube, Zimmermann, and others, that preternatural heat is the symptom, the presence of which is invariable in fever—the symptom which is never absent when fever is present—and that rigor is merely a peripheric phenomenon; that even when the surface feels cold to the bystander, the inner parts are abnormally warm. "While the outer parts freeze," writes Virchow, "the inner burn."

In fever, as in health, the temperature of a part is in proportion to the quantity of blood that flows through its capillaries in a given time; the greater the quantity of the blood, the higher the temperature. In the cold stage of fever, there is some impediment to the flow of blood through the capillaries of the skin: that less blood passes to the skin under these circumstances is proved by the pallor of that structure. The chief part of the nerves by which we appreciate temperature are distributed to the skin; hence, though the temperature of the blood is high, the diminished flow of that fluid to the skin more than counterbalances the elevation in its temperature, and a general sense of chilliness or of absolute cold is the result.

Whatever induces anæmia or venous hyperæmia of a part, must diminish the temperature of that part. But the sensation of cold experienced by the patient will be not merely in proportion to the anæmia or venous hyperæmia, but to these states *plus* the sensibility of the part to changes of temperature.

But yet more, the researches of Zimmermann and Bärensprung have shown that, in some cases at least, an elevation of temperature precedes the cold stage of intermittent fever, and that many fits of ague are to be detected only by an elevation of temperature appreciable alone by the thermometer; and Jochmann even supposes the rapid elevation of the temperature of the blood to be the exciting cause of the rigors. *Calor præter naturam* being, then, the one essential symptom—the pathognomonic symptom—of fever, the question arises, to what abnormal condition of the body is this elevation of temperature due?

"Admitting," says Dr. Parkes, "that normal heat arises from chemical changes controlled by the nervous system, has the excess of heat, which constitutes fever, an analogous origin? Is the chemical action similar to that of health, only in excess, or is it something altogether special? Does it arise from the presence in the body of some peculiar chemical agency, or is there some peculiar bodily condition which leads to the formation of extraordinary compounds out of ordinary tissues? And then what part do the nerves play in these actions, and how far is their affection primary or secondary, essential or subordinate and inconstant?"

The answer to the question is given by Virchow, in his definition of fever:

"FEVER CONSISTS ESSENTIALLY IN ELEVATION OF TEMPERATURE, WHICH MUST ARISE FROM AN INCREASED CONSUMPTION OF TISSUE, AND APPEARS TO HAVE ITS IMMEDIATE CAUSE IN ALTERATIONS OF THE NERVOUS SYSTEM." (p. 37.)

Or at greater length in the following summary of his views, by the same author:

> "Every disease may become febrile, every disturbance may form itself into a fever, if it extend itself to the centres which regulate the waste of the tissues, and the proper moderating power of the tissue-metamorphosis is suspended. If these moderating centres are to be sought for in the nervous system, it will be a question of an abnormal tension-condition (*Sind diese Centren ein Nervensystem zu suchen so wird es sich um ein abnormes Spannungsverhältniss handeln*), which is induced by the exciting cause of the fever, and is not resolved in the natural courses (*Bahnen*). In proportion as the power of the moderating centres is restrained, the waste of the tissues is increased, and so the proper warmth of the body increased; and here the exact starting point of fever is attained. In the precursory stage we see only the weakening of the bodily and mental activity, which follows immediately from the tension-condition (*Spannungsverhältniss*)."

Taking Virchow's definition of fever as a text, Dr. Parkes has brought together a large mass of new and collected facts, for the purpose of proving the truth of each of the three heads into which Virchow's definition naturally falls. We propose to give our readers an abstract of these facts, and of their bearing on the questions at issue.

That *Calor præter naturam* is the essential and pathognomonic symptom of fever, is supported by so large a number of well known, unquestioned, and unquestionable facts, that Dr. Parkes passes it by with a very few words.

The elevation of temperature in fever "*must* arise from increased tissue consumption," says Virchow. What proof is there of this? demands Dr. Parkes, and then proceeds to answer his own question. Now it is manifest that, if we were to collect all the excretions in a given case of fever (the patient taking no more food than in health), and were to find those excretions more abundant than in health, the truth of the proposition would be rendered probable; and further, that if repeated observation showed the same to be true of all other cases of fever, and that the temperature and the amount of the excretions bore some relation to each other, the truth of the proposition would be rendered highly probable; and if yet further the loss of weight of the patient proved to be in all cases in proportion to the amount of excretions and to the temperature, the truth of the proposition would be rendered in the greatest degree probable. But there are practical difficulties in the way of collecting some of the excretions in cases of fever, which have not as yet been efficiently overcome. "Two of the excretions—the cutaneous and pulmonary—cannot be collected and measured with anything like the accuracy necessary in such an inquiry."

Dr. Parkes thinks, however, that it may be assumed that "when the respirations are not quickened, and when the skin is not evidently sweating, the excretions of these two organs are not increased."

The other two excretions can be measured with accuracy; and as the urea of the urine represents "two-thirds of the whole quantity of nitrogen" which passes out of the body, the sulphuric acid "represents almost entirely the oxidation of the sulphur," and the oxidized phosphorus of the body passes away almost together in the same fluid, we may fairly admit Dr. Parkes' conclusion—viz., that

> "A careful examination of the urine and of the intestinal discharges, with an approximative estimate of the pulmonary and cutaneous excretions, give us sufficiently extensive and accurate materials for the question at issue."

The most opposite statements have been made regarding the quantity of the excretions in fever compared with the quantity in health. Dr. Parkes' own researches on this point are most interesting and important; for they have enabled him to show that the excretions are in certain cases increased, while they have enabled him to show also, that in other cases of the *same* febrile diseases they are diminished, the temperature being as high in the one set of cases as in the other. A superficial examination of these facts might lead to the conclusion that tissue-waste and temperature bear no relation to each other; a more profound examination of them leads to the opposite conclusion.

Fever, with Increase in the Amount of Solids excreted.

Six cases observed by himself, and three recorded by Alfred Vogel, are given under this head. Of these nine cases, two were rheumatic fever, one erysipelas of the head and face, four typhoid fever, one febricula, and one pyæmia. In one of Dr. Parkes' cases of rheumatic fever, the temperature being for some days 3° Fahr. over the standard, the urinary solids averaged in each twenty-four hours, 200 grains more than in a state of health; the intestinal excretions were not apparently diminished, and there was profuse sweating. Dr. Parkes' commentary on this case observes:

> "In this instance, if we consider that no food was taken, and yet that the urinary ingredients derived from the daily metamorphosis of tissue was nearly one-fourth more than in the healthy state, when some twenty ounces of food were daily taken, the extraordinary disintegration going on in some tissue or other is evident."

In the other case of the same disease, the temperature was also 3° Fahr. above the standard, the excess of solids in the urine was 100 grains, the intestinal excretions sufficiently abundant, the quantity of food taken was very small, and the man was rapidly losing weight. The facts on this point observed by himself and others, justify Dr. Parkes' conclusion —viz.:

INCREASE OF TEMPERATURE *may* BE ATTENDED WITH INCREASED ELIMINATION; AND THEREFORE PRESUMABLY WITH INCREASED TISSUE CHANGE.

Fever, with a Diminution in the Amount of Solids excreted.

But increased elimination of solids is not a constant concomitant of increased temperature; and Dr. Parkes has given four cases to prove this point—viz., one of bronchitis, one of acute sthenic pneumonia, one of typhoid fever, and one of rheumatic fever. We shall quote the two last:

> "In an unequivocal case of typhoid fever, in a man aged twenty-three, in which there happened to be no diarrhœa, no sweating, and no bronchitis, the urinary ingredients were for many days below the normal amount, though the elevation of temperature was considerable.
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> "A girl, aged nineteen, with acute rheumatism, and with a temperature averaging 102° Fahr. in the mouth, passed for several days an extraordinarily small amount of urinary solids; there was no compensation for this in other ways, for she was breathing tranquilly, the bowels were quiet, and there was only inconsiderable perspiration."

In these cases the urinary constituents which particularly "represent the metamorphosis of albuminous tissues"—viz., the urea and the sulphuric acid, were especially decreased in amount. An objection to the conclusion which seems to follow from these cases might be made—viz., that the diminution in the amount of the solid constituents of the urine was merely the consequence of the withdrawal of food. This objection is met by Dr. Parkes, thus:

> "It is evident, however, that the taking of food has nothing to do with the question at issue, which stands thus: in the healthy body the normal temperature produced by chemical change is represented in the excretions by so much urea, sulphuric acid, carbonic acid, excretine, volatile acids of the skin, &c.; in the febrile body in these cases a *higher temperature* was represented in the excretions by a *smaller quantity* of urea, sulphuric acid, and probably carbonic acid."

The conclusion from his cases is, then, irresistible—viz., that

THE PRODUCTS OF METAMORPHOSIS, AS JUDGED OF BY THE EXCRETA, *may* BE DIMINISHED IN FEBRILE CASES, IN WHICH THE HEAT OF THE BLOOD IS INTENSE.

Consumption of tissue is in health represented by certain excretory substances; the abnormally high temperature of fever is due, say the authors of the works before us, to increased consumption of tissue. But in certain cases of fever, as we have just seen, the excreta representing consumption of tissue are diminished. Does not the hypothesis, then, fall to the ground?

Virchow maintains that the diminished exhalation of carbonic acid from the lungs, which is said to occur in fever, is no proof that there is diminished consumption of oxygen during respiration; and still less, proof that the temperature of the patient suffering from fever is not the effect of chemical changes—of a process of burning. "For," he remarks, "much organic matter can be oxidized without the formation of carbonic acid;" and he leans to the opinion, that under these circumstances water is formed in the body by the direct combination of its elements, and that the oxidation of the hydrogen serves for the production of heat, while the quantity of water formed is so small—even though the heat generated be considerable—as to pass away unnoticed with any of the secretions; and he adds,

> "Nay, it is also conceivable that less oxygen is taken up, and still that a larger quantity of the products of oxidation is formed, although they are less perfect; as for this to occur, it would only be necessary that a greater quantity of readily oxidizable substances should be present in the blood. Such a condition we could very well imagine, for example, in the increase of uric acid compounds, since uric acid, in relation to urea (which latter we know can be formed from it), appears to be a product of a lower degree of oxidation; and its abundant formation leads one to conclude that there is some deficiency of oxidation, which deficiency of oxidation may itself depend either on an incompleteness of the respiratory process, or on the presence of too large a quantity of matter that is capable of decomposition."

With reference to this latter point, Dr. Parkes observes that the examination of the relative amount of uric acid and urea in fever gives no more support than examinations in other disease, to the hypothesis of Liebig, that uric acid is a lower stage of oxidation than urea, because both are often increased together.

Dr. Parkes advocates most ably the hypothesis—by his facts and arguments raised to the rank of a theory—that in the cases of fever in which there is high temperature, with diminution of solid excretions, there is retention of the excretions in the blood, and not diminution in the amount formed. "We can quite readily," he says, "conceive increased metamorphosis with lessened elimination." And it seems to have been in those cases especially in which, during the earlier periods of the disease, there was elevation of temperature with diminished excretions, that at a later period copious discharges occurred from one or other of the eliminating organs.

> "Thus," he remarks, "in the case of pneumonia with lessened excretion, to which I have referred, severe spontaneous diarrhœa came on. In another case of pneumonia with similar diminution in the excretions, violent purging and sweating came on. In other cases, diuresis has occurred, and an increase of urea, of sulphuric acid, and probably of uric, has been poured out."

It is conceivable, however, that even in cases in which there is increased elimination of solids, the quantity eliminated may still be much less than the tissue-waste, and that the surplus may be retained in the blood. So that increased elimination of solid excretions in any given case by no means excludes the possibility, or even the probability, of the retention of excretions in the blood in the same case.

The bearing of these facts and opinions on the explanation of "critical" discharges is evident.

> "It seems," observes Dr. Parkes, "a reasonable explanation of these sudden discharges (usually termed critical, from being coincident with more or less sudden fall of temperature, and improvement in other symptoms), to suppose that a large amount of partially metamorphosed substances have been retained in the body, and at length have been brought to that point of oxidation or change which permits their elimination by one or other organ. Then by their discharge the system is suddenly freed from the noxious compounds which weighed upon it; and the metamorphosis having reached its acme, the temperature immediately falls."

Another fact given by Dr. Parkes in support of the doctrine of retention, is that secondary inflammations have been, in his experience, more common in cases with diminished elimination, than in those with increased elimination of solid excretions. In three cases he observed a sudden diminution of excretion, and simultaneously, or directly after, a local disease developed. These cases are of the highest interest. The following seems to us to tell the most conclusively of the three in favour of Dr. Parkes' position:

> "In a case of protracted typhoid fever, the urine in which was regularly analysed during no less than fifty days, the average excretion of urinary solids from the twenty-ninth to the thirty-seventh days inclusive, was 422·348 grains. Diarrhœa, which had been profuse, had ceased. There had been great sweating, but this had much lessened. In spite of the late period of the disease, the temperature during these eight days averaged 153·4° Fahr. On the three last of these days (the thirty-fifth, thirty-sixth, and thirty-seventh) the urinary solids gradually diminished. On the thirty-eighth day pleurisy came on, and lasted for about five days."

While the pleurisy continued, the urinary solids averaged seventy-eight grains a-day less than during the preceding eight days. The diminution was found to be most marked in the urea, the sulphuric and the phosphoric acids.

The greater part of Dr. Parkes' second lecture is devoted to—

> "A consideration of perhaps the most important general chemical condition of the febrile body; a condition which has long been partially observed, but has never been regarded as being of such interest as it really appears to be. I refer to the remarkable retention of water in the febrile system."

Scanty urine, scantiest when the skin is driest, is an almost constant concomitant of the early stages of pyrexia; and this notwithstanding the large quantity of fluid which is taken to quench the extreme thirst.

In all fevers, however, Dr. Parkes observes, there comes a time when, although the heat of the body is still preternatural, the urine passed equals in quantity, or even exceeds, the standard of health.

> "In rheumatic fever the increase of urinary water is seldom seen before the joint affection has almost disappeared; in typhoid fever it occurs earlier, in mild cases about the fourteenth to the eighteenth or twentieth day, although the febrile heat is still one or two degrees over the standard."

In a case of relapsing fever examined by Dr. Parkes, the urine became abundant when the skin became moist, that is, at the period of remission.

In the early stage of pyrexia it is more common to find the skin moist, than it is to find the urine abundant.

The amount of watery matter given off from the lungs has never been determined; but judging from analogy, Dr. Parkes says, and from the rapidity with which the lips and buccal mucous membranes dry, it cannot be imagined to be so great as is exhaled from the same organs in a state of health.

The quantity of fluid poured into the alimentary canal from the several glands, and by the mucous membrane in a healthy person, is beyond question enormous. The far greater part of this fluid is re-absorbed, to be again probably poured out by the mucous membrane, and again re-absorbed. In fever the amount of this fluid is, Dr. Parkes states, greatly decreased; and this, he thinks, is proved by the dry tongue and buccal mucous membrane, by the observations of Dr. Beaumont on Alexis when in a state of febrile excitement, and by the constipation which is so common a symptom in fever. But Dr. Parkes goes farther, and suggests the probability of the movement of fluid from molecule to molecule, from cell to intercellular fluid, and from fluid to cell, throughout the whole body, being modified or interfered with in the febrile condition.

> "Then at once comes the question, whether this vast circulation, to which the circulation discovered by Harvey is but the servant, is altered in fever? We can scarcely avoid concluding that it is so."

As to the cause of the retention of water in the system in pyrexia, Dr. Parkes suggests that it may possibly be due to the presence in the blood of some substance which has a powerful attraction for water.

> "A fact mentioned to me (Dr. Parkes) by Mr. Graham will put my meaning in a clearer light. Mr. Graham has discovered that gelatine has an extraordinary attraction for water, so that it will even take it from alcohol, and render alcohol almost, if not quite, anhydrous. This property, manifested at all temperatures, is particularly marked at the temperature of 98° to 100° Fahr. Albumen, on the other hand, has little attraction for water, and yields it up at once to alcohol.
>
> "Now supposing that in the rapid metamorphosis of albuminous substances in fever, gelatinous compounds, or something approaching to them, were formed—

and this is by no means unlikely—then, as a consequence of a physical law, the gelatine would at once take water from the albuminous tissues, and would necessarily give rise to intense thirst. Then, unlike sugar, the gelatinous substance would not be discharged, but must be converted into urea and uric acid, as ordinary gelatine is when it is taken as food. I mention this hypothesis merely as an example of how water might be retained in the febrile body. At present, of course, we do not know whether any such compound is or is not formed; for the transition steps, and they may be numerous, between organized albumen and urea are not known."

Another fact of interest in the chemical history of the secretions in fever, is the diminution of chloride of sodium in the urine.

To witness disease pure and uncomplicated, of a single organ, is indeed to witness a rare phenomenon. So intimately are the several parts of the organism related to each other, to the common fluid which supplies all with the elements of growth and repair, and to the agents which preside over those processes, that we can scarcely even imagine disease to be so located for more than the shortest conceivable period. Long before our senses can take cognizance of the primitive affection, other organs, other tissues, and other fluids must deviate more or less from their normal state. But if this be true, it is no less so that almost every disease is, at its outset—theoretically, at least—limited to one fluid, one structure, or one organ. The pathologist has to endeavour, with regard to each so-called distinct disease, to determine what is the part primarily affected, in what manner it is affected, and by what agencies such primary affection influences other parts, so as to give rise to the symptoms proper to or common in such diseases.

Physicians have sought to determine these points in respect of fever, of fever in its abstract and in its specific form. Now inflammation of this organ or tissue, and now of that, was regarded as the starting-point of fever—nay, was supposed to be that which, being removed, the symptoms of fever ceased; and which, so long as it continued, must be accompanied by the fever. Pathological anatomy did medicine the good service of silencing for ever the dogmatists who supported these views with so much arrogance.

In later times, even to the present, the favourite opinion has been that fever is primarily a disease of the blood; and certainly, to use Dr. Parkes' words—

> "In almost all the specific diseases, in small-pox, scarlatina, measles, typhus, typhoid fever, relapsing fever, and yellow fever, a fever-making cause appears to enter the blood—*at least, it can be proved to enter in several cases;* and a strong analogical argument can be proved of its entrance into the rest."

Again: in fever which is consequent on parturition and operations, a material agent can be proved to have found its way into the blood:

> "In fact," again to use Dr. Parkes' words, "this part of the argument scarcely needs discussion, as it is generally admitted that, in almost every case, if not in all, the first action of the febrile cause is on the blood."

But granting this, does it follow that those changes in the chemical constitution and the quantities of the excretions, and those altered relations of the blood and the tissues which eventuate in so great tissue consumption in place of normal nutrition—does it follow, it is asked, that these are directly due to the changes in the blood, induced immediately by the entrance into it of the fever-making cause?

Virchow answers this question in the negative; and assigns the principal part in the production of the symptoms of fever to the nervous system. His opinion is that the exciting cause of fever enters the blood, and exerts its first influence on the moderating nervous centres; and that the perceptible phenomena of fever—the symptoms—are consequent on the withdrawal of the nervous influence.

> "Is it so certain that the very basis of the febrile state is to be located in the nerves, and are *all the phenomena* of fever to be comprehended in the concise though vague phrase of 'perverted elimination?'"

asks Parkes; and then gives his powerful support to the opinion of Virchow. Traube had previously remarked, that observers had directed their attention too exclusively to the origin or source of the temperature (*den Wärmequellen*), neglecting the regulators of the generation of heat (*die Regulatoren der Wärmebildung*).

With reference to the special kind of influence exerted by the nervous system on tissue consumption, *à priori* one of two things may be supposed —viz., either that the nerves are in a state of activity when much heat is being generated, and consequently much material consumption going on, or that the nerves are the moderators of tissue consumption; and that, when much heat is being generated, the nerves are in a state of inactivity, of paralysis (*der Lähmung*). The facts demonstrated by Weber, by Ludwig and Hoffa, and by Volkmann and Fowelin, concerning the functions of the vagus, support the latter view; for Weber found that section of the vagus was followed by increased rapidity in the heart's beats, and the transmission of an electric current through the cut nerve, by diminished rapidity in its action; while Volkmann and Fowelin observed that section of the same nerve was followed by an increased lateral pressure of the blood on the arteries; and Ludwig and Hoffa, that moderate irritation of the vagus was followed by a diminution of the lateral pressure of the blood on the same vessels.

The experiment of Bernard, and its converse by Waller, appear to tell yet more strongly in favour of the same view; for when Bernard divided the sympathetic in the neck, not only were the vessels of the same side of the head dilated and hyperæmic, but the temperature of the blood in the part, was higher than that of the blood in the system at large; while Waller found that contraction of the dilated vessels, diminution in the hyperæmia, and fall in temperature, followed on the transmission of an electric current through the divided sympathetic. There are no experiments or observation in support of the former view at all comparable in force with these.

The arguments advanced by Virchow and Parkes in favour of the opinion, that it is on the nervous system that the primary action of the exciting cause of fever is exerted, may be thus summed up:

Depression, apathy, a sense of exhaustion, and debility—symptoms due directly or indirectly to an affection of the nervous system—are the first evidences of febrile disturbance. Rigors and contraction of the smaller vessels also have their origin in diminished nervous influence. Increased cardiac action, pulmonary congestion, anorexia, and nausea, are most probably the result of the withdrawal of the nervous power of the vagus. The periodicity of certain fevers, the occurrence of death or recovery on so-called critical days; the abnormal state of the secretions; *the fearfully* *rapid death in many cases when no special lesion of any one organ is to be detected;* and the speed with which recovery from periodical fever takes place under the use of quinine, are all due to some perversion of innervation.

Dr. Parkes thus enumerates "the various influences which seem to be active in fever, and by the combined effect of which its complex phenomena may be supposed to be produced."

"First of all, we must place the entrance into the blood of a morbific agent, and the alteration of the blood, to a certain extent, under its influence. Perhaps this occurs under the incubative period, when often there is no rise of temperature, no fever, that is, and when no appreciable alteration of the general health can be discovered. The nature of the change in the blood is unknown.

"Then, secondly, when the change in the blood has reached a certain point, the nervous system, or rather that part especially connected with nutrition and organic contractility, begins to suffer changes in composition, which probably impede or destroy the normal molecular currents. When this occurs, the nervous symptoms of weakness, depression, rigors, and contraction of some parts and vessels, speedily followed by relaxation, mark the stage of invasion.

"Thirdly, and simultaneously, various parts, especially the muscles, and probably some of the organs, deprived in greater or less degree of nervous influence, begin rapidly to disintegrate, and by their disintegration produce supernatural heat.

"Fourthly, this metamorphosis is aided, in most cases, by the condition of the vagus and vasi motor nerves which cause increased action of the heart and dilatation of the vessels.

"Fifthly, the contamination of the blood, already produced by the morbid agent, is increased by the check which the normal extra-vascular currents experience, by the pouring into the blood of the rapidly disintegrating tissues, and by the continued action of the morbid agent, which in almost all cases appears to act more rapidly and more powerfully in blood rendered impure in any way, either, as shown by Dr. Carpenter, by retention of excretions, absorption of septic substances, or, as in fever, by the too rapid metamorphosis of tissue.

"Sixthly, the various organs suffer (apart altogether from specific changes), and must, one would think, produce increased deterioration of the blood. Thus the lungs are congested in so many cases that we can scarcely suppose proper aëration to go on; the liver would seem, from Frerich's observations, to be, in some cases at any rate, in a most abnormal condition, and to produce compounds, such as leucin, unknown in health; and the spleen in many fevers, if not in all, enlarges (in persons of a certain age), and is congested, possibly even to extravasation."

Nor must it be forgotten, Dr. Parkes urges, that neutral salts are passing out of the system, while, in consequence of the withdrawal of food, few inorganic salts enter the blood.

We have now laid before our readers a tolerably full account of the important paper of Virchow, and the even more important lectures of Parkes, and from the abstract we have given it is evident that so consistent a theory of the nature of fever, one so largely supported by facts, has not hitherto been placed before the profession. While, however, we admit this, we feel that all who have to write on the subject have necessarily "to allude," to use the language of Dr. Parkes, "to inexplicable phenomena, to vast spaces still unfilled by solid facts, to spots unknown to observation, and to regions lighted only by the dim and treacherous ray of speculation;" and that much difference of opinion must consequently exist among those who write or think on so obscure a subject.

We must, then, say that, to our mind, it appears possible that too little influence has been attributed by the distinguished German pathologist whose work is before us, to the blood as the effective agent in the production of the primary molecular changes, and of the symptoms in the earlier and the more advanced stages of fever. Dr. Parkes comes much nearer, it seems to us, to the truth in this matter. For whatever evidence, speculation or facts may afford of the participation of the nervous system in the lesions of function which give rise to or constitute the appearances or symptoms of fever, equally strong evidence can be offered of the participation of the blood in the production of the earlier and the later phenomena of fever. The fact is admitted to be indisputable that the fever-making cause enters the blood; so that it is certainly *à priori* more probable that a change should be effected in that fluid before any is effected in the nervous system, than the reverse. And there can be no doubt that the necessity for a healthy condition of the blood is as essential to the formation of normal secretions as a healthy state of the nervous system. "In normal nutrition," says Mr. Paget, "the principal *factors* are the tissues and the blood in their mutual relations." In fevers which end fatally, without organic lesion of any one organ sufficient to account for death, the blood deviates in its most striking physical characters from healthy blood. We remember a case of small-pox which proved fatal on the fourth day of disease, and before the eruption appeared. In this blood, or, more properly speaking, a non-coagulable solution of hæmatosin, escaped into the cutis—giving rise to petechiæ, vibices, and bloody blebs—and into the conjunctivæ. A similar fluid oozed from the eyes, the nose, and the ears, passed from the bowels, and flowed in great abundance from the vagina. Could stronger evidence be offered that the blood was in an highly abnormal state, even from the outset, seeing that some of these symptoms appeared so early as the second day of disease? Was it necessary to go beyond the blood and its direct effects to account for the death of the patient?

Again, in typhus and other specific fevers, the microscopical characters of the blood are often such as to prove a marked deviation from its normal state; amorphous heaps of red discs replace the normal rouleaus, and the adhesion of the red discs to each other in the imperfectly-formed rouleaus is far less complete and long-continued than in healthy blood. The red discs, too, part with their colouring matter more easily, or dissolve more rapidly, than they do in the normal state, as is proved by the red serosity found in every serous cavity, and by the deep dusky red hue of every structure in contact with blood. While blood drawn during life, or found in the body after death, is loosely coagulated or absolutely fluid. And although the acid serum found by Vogel after death from pyæmia and puerperal fever may have been the direct result of changes in that fluid effected after death,[*] still the fact of its being susceptible to such a change shows that it had experienced some considerable deviation from its normal state anterior to the cessation of life.

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[*] The fact of all the solids (except perhaps the adipose tissue) as well as the fluids undergoing a change after death in which an acid is developed, is not sufficiently known. This development of acid occurs in the bodies of those cut off by accident, as certainly as in the bodies of those who die from special diseases. Dr. Budd, in his excellent book On the Liver, refers to a case of his brother's, in which the liver exhibited an acid reaction, and adds, " The

But while we think there is strong evidence in favour of the primary affection of the blood, and of the wide-spread and fearfully severe influence on the system generally of the very deep lesion which in many cases we can demonstrate the blood to have experienced, independently of mere admixture of excess of excrementitious matters, we by no means exclude the nervous or any other part of the body from a share in the production of the symptoms of fever.

We cannot conclude without observing that some of the expressions used by Virchow have little or no definite meaning. For example, we have questioned a very considerable number of the most distinguished physiologists and pathologists in this country as to the exact signification to be attached to the expression *Spannungsverhältniss*, when applied to the nervous system, and one and all have replied, *Vox et præterea nihil.* Professor Virchow is most fortunate in having found so learned and practical a pathologist, and so lucid a writer, as Dr. Parkes, to place his views before the profession in this country, and to support them with such important original facts and such powerful arguments.

W. Jenner.

REVIEW VIII.

1. *Experimental Researches applied to Physiology and Pathology.* By E. BROWN-SÉQUARD, M.D., &c.—*New York*, 1853.

2. *Sur les Résultats de la Section et de la Galvanisation du Nerf Grand Sympathique au Cou.—Paris*, 1854.
On the Results obtained by the Section and Galvanisation of the Sympathetic in the Neck.—Paris, 1854.

3. *Rapports sur quelques Expériences de M. Brown-Séquard.* Par M. PAUL BROCA.—*Paris*, 1855.
Report on some Experiments of M. Brown-Séquard. By M. PAUL BROCA.—*Paris*, 1855.

4. *Deux Mémoires sur la Physiologie de la Moëlle Épinière.* Par M. BROWN-SÉQUARD.—1855.
Two Memoirs on the Physiology of the Spinal Cord. By M. BROWN-SÉQUARD.—1855.

5. *Recherches Expérimentales sur la Transmission Croisée des impression Sensitives dans la Moëlle Épinière.—Paris*, 1855.
Experimental Researches on the Crucial Transmission of Sensory Impressions in the Spinal Cord.—Paris, 1855.

6. *Experimental and Clinical Researches on the Physiology and Pathology of the Spinal Cord.* By E. BROWN-SÉQUARD, M.D.—*Richmond*, 1855.

7. *Notice sur les Travaux de M. Brown-Séquard.—Paris*, 1855.
Notice on the Works of M. Brown-Séquard.—Paris, 1855.

TWELVE years have elapsed since M. E. Brown-Séquard commenced a series of experimental researches on various animals, both warm and

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acid reaction of the liver is a very remarkable fact, and probably will turn out to be very important, when the true explanation of this terrible disease (fatal jaundice) is discovered." But the truth is, that in every case, no matter what the disease of which a patient dies, the liver has a strong acid reaction if decomposition has advanced so far as it had in Dr. Budd's case.

cold-blooded. These investigations were not instituted with the view either to confirm or refute the opinions of other pathologists, nor were they undertaken to support any preconceived theory, but they were planned and executed simply to ascertain the truth on various obscure points of physiology and pathology.

Commenced in the Mauritius, they were subsequently pursued in Paris and the United States, where M. Brown-Séquard for a long time resided. The results were from time to time published in a scattered form, either in communications to the French Academy of Sciences, or in the periodicals of the day. By these works the reputation of M. Brown-Séquard was well established; but his observations had not obtained their full weight until recently, when, having delivered some lectures in Paris detailing the results of his studies, his discoveries excited so much attention, that a commission was appointed by the Société de Biologie to investigate the truth of his statements, and to confirm the results of his experiments. The report of this commission is contained in the paper by M. Paul Broca, published in 1855.

In 1851, and again in the autumn of last year, M. Brown-Séquard visited England, and on both occasions he lectured and performed several experiments at St. Bartholomew's Hospital. All who there saw him, and heard the simplicity of his explanations, could not fail to be convinced of his patience as an investigator, his skill as an operator, and his thorough soundness as a reasoner.

The plan we propose to pursue is identical with that followed in recording M. Bernard's contributions to physiology, contained in 'The British and Foreign Medico-Chirurgical Review' for 1854. It is our object to give a short *résumé* of M. Brown-Séquard's discoveries, and, abstaining from criticism, to present, in as succinct a form as possible, the most important results of his investigations. Our account of the author's discoveries will be necessarily disjointed and fragmentary, collected as it is from his numerous publications. For a general summary of his works we would refer our readers to the work entitled 'Notice sur les Travaux,' published in 1855.

In considering our author's contributions to physiology, the ordinary arrangement of physiological works has been observed; and his discoveries will be noticed in the following order—namely, as they relate (I.) to the Blood; (II.) to Animal Heat; (III.) to the Nervous System; and (IV.) to the Muscles.

I. *The Blood.*—(*a.*) The coagulability of the blood is less influenced by cold than was supposed; if, in frogs exposed to a very low temperature, half the ventricle of the heart be cut off, the blood quickly coagulates, the wound closes, and the animal may live for many months. (*b.*) Of the components of the blood some are necessary for nutrition, and some are excrementitious. The former appear, from experiments, to be the serum, the blood-corpuscles, and the oxygen; the latter are chiefly the carbonic acid, and probably the fibrin. In transfusion, all the vivifying effects of ordinary arterial blood on living animals can be produced when the blood is defibrinated. The presence of serum, blood-corpuscles, and oxygen, is absolutely necessary; no mixture of serum, albumen of egg, or any substance yet discovered, can be used as a substitute for these.

The main facts that have led M. Brown-Séquard to consider fibrin as excrementitious in the blood are the following:—1st. That, as already stated, defibrinated blood apparently possesses all the nutritive properties of natural blood; and 2ndly, that when perfectly defibrinated red blood is injected into the main artery of a limb that has been thoroughly washed out by repeated injections of diluted serum, the blood returns through the veins not only venous in its aspect, but containing small quantities of fibrin, which may be increased by exciting muscular contractions in the limb during the injection.

These results certainly strongly support that *German heresy*, as it has been called, which holds the fibrin to be a product of the retrogressive metamorphosis in the blood.* Still, it is well to be guarded, especially when it is considered how close a relation albumen bears to fibrin. In the experiments referred to, which were performed on man, though both the serum used to remove the fibrin from the vessels of the limb, and the blood previous to injection, were tested, and found to be afibrinous, yet the difficulty and uncertainty of obtaining fibrin from some peculiar states of solution in albuminous fluids is well known. It is especially evidenced in the separation of fibrin from hydrocele fluid; in which, although nothing like spontaneous coagulation will occur, yet under certain apparently mechanical conditions, coagulation ensues, and may be repeatedly produced, by fulfilling the same conditions. In the same way, the recognition of fibrin in the blood after its passage through a limb, may be due to some mechanical or other cause operating during its transit through the capillaries, which has rendered coagulable, fibrin that before escaped detection.

(*c.*) The following is a short summary of the effects that can be produced in the dead bodies of warm-blooded animals by the injection of blood. The blood used has in all cases been defibrinated, and beaten in the open air to oxygenate it; it has been injected through the main artery of the part experimented on; has been collected as it returned through the veins, and again oxygenated and injected. Thus the same blood has been used time after time: its temperature has been that of the surrounding medium.

All muscles, those of animal and those of organic life—including the iris, the muscular fibres of the skin, and bloodvessels—can be recovered from their post-mortem rigidity, and restored to irritability. To this rule the heart is, in a manner, exceptional: it can only be restored to irritability during a short period after death, and before the rigor mortis has affected it.

The muscles of animal life can be restored, by the injection of blood, to a state of irritability four and twenty hours after death, and four or five hours after the supervention of rigor mortis. They may be kept irritable in this manner for forty-one hours in one limb, although at the end of that time the opposite limb may be in a state of putrefaction. Chloroform appears to possess the property of arresting decomposition, and of prolonging post-mortem rigidity. If it be injected through the vessels of a limb, the muscles quickly pass into a condition closely imitating cadaveric rigidity. They are firm, rigid, and apparently contracted; and thus they remain for some days, during which, by the injection of blood, they may at any time be restored to irritability. This state usually continues till the fourth or fifth day after death; but in one case, irritability was restored ten days after the injection of chloroform.

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* The arguments for and against this theory, which in England has found its main supporter in Mr. Simon, are summed up in vol. vii. p. 153, of this Review.—ED.

Many of the results obtained on warm-blooded animals were confirmed by experiments on the bodies of two criminals. The blood used was in the one case human, and in the other taken from a healthy dog; it was subjected to the process before described, and its temperature was that of the surrounding medium. In the first case, half a pound of human blood was used. The man had been dead thirteen hours; rigor mortis had existed two hours. Ten minutes from the commencement of the injection into the radial artery, rigidity disappeared, and quickly gave place to irritability, the existence of which was proved by the contraction of the muscles under galvanism. This condition was sustained by continued injection, in seventeen muscles of the hand and fore-arm, for four hours and a half. In the second experiment, the man had been dead fifteen hours, and rigid for five hours: a pound of dog's blood, subjected to the usual process, was injected into the brachial artery of the left arm:

> "As soon as the blood had been thrown into the artery, some reddish spots appeared in different parts of the skin of the hand, fore-arm, and wrist. These spots became larger and larger, and the skin assumed a rubeoloid appearance. Soon after, the whole surface of the skin was of a violet-red hue. In a few minutes this colour disappeared, and was replaced by the natural colour of the skin during life. The skin became soft and elastic, as in a living man, and the bulbs of the hair became erect, presenting the appearance of *cutis anserina*. By alternately increasing and diminishing the impulse given to the injected blood, the ordinary sensation of pulse in the radial artery was produced. The veins were distinct, and as full as during life. In a short time the fingers, which had hitherto been extremely rigid, relaxed, as also did the muscles of the fore-arm."

Thirty-five minutes from the commencement of the injection, irritability had returned in all the muscles of the limb. The muscles were still contractile twenty hours after the death of the man, when, from extreme fatigue, M. Brown-Séquard desisted from his endeavours to retain irritability in the limb. In this case, one pound of defibrinated dog's blood restored and maintained irritability, in all the muscles of the upper limb below the middle of the humerus, for more than five hours.

(*d.*) M. Brown-Séquard has drawn a most ingenious, and probably true, distinction between arterial and venous blood, in their respective relations to the vital properties of the tissues through which they pass. The former is, or at least contains, the nutritive material for the various tissues and organs of the body; and upon nutrition depend the various vital faculties of these tissues. Thus contractility is dependent upon muscular nutrition; the reflex faculty exists only so long as the due nutrition of the spinal cord is performed. Arterial blood maintains and preserves these faculties, but is unable itself to put them in action. It may give irritability to a muscle, but cannot itself excite a single contraction.

Venous blood, on the other hand, is not a nutrient but an excitant substance. Though unable to support the vital faculties, it can stimulate an irritable muscle or excite a nervous centre, and produce in that a contraction, in this a reflex act.

These properties of arterial blood are generally admitted, and abundantly confirmed by experiments; and its inability to excite muscular contraction has been ascertained by M. Brown-Séquard. The excitant or stimulant effects of venous blood are best seen in death by asphyxia, from whatever cause. The spasmodic contractions of the organic and inorganic muscles occurring in death by strangulation, the various movements of the limbs taking place after death by cholera, and the expulsion of semen in sudden asphyxia,—these are thought by M. Brown-Séquard to be instances of some among the many excitant effects of venous blood. We select the following experiments as bearing on this subject:

In guinea-pigs who had arrived at nearly the full period of gestation, parturition was produced by suddenly applying a ligature to the trachea. The same experiment was performed, with a similar result, when all the lower parts of the spinal cord had been completely crushed.

In an animal just dead, one posterior extremity was injected with venous blood, while into the opposite limb arterial blood was thrown: in the latter case no movements followed, while in the former well-marked spasmodic contraction of the muscles ensued. By suddenly asphyxiating warm-blooded animals, M. Brown-Séquard produced spasmodic contractions in all the muscles of animal life, and in the respiratory muscles, the intestines, the urinary bladder, the ureters, the uterus, the vesiculæ seminales, the dartos, the iris, and the gall bladder.

The rhythmical type of contraction often assumed by muscles when poisoned with venous blood; the effects of artificial asphyxia in producing muscular contractions; and the inability of arterial blood to excite any muscular action;—these, among other considerations, have induced M. Brown-Séquard to attribute to venous blood the cause of the rhythmical movements of the heart.

II. *Animal Heat.*—(*a.*) Hunter placed the standard of the temperature of the human blood at 98·4° Fahr.; more recently, Dr. Davy* has made an extensive series of investigations, and has placed the extremes of animal temperature in man at 97·7° and 99·7° Fahr. respectively. In Dr. Davy's observations, the temperature was taken under the tongue; while Mr. Hunter observed the temperature of the rectum, which he maintained was the same as the left ventricle. M. Brown-Séquard states that in a healthy man the temperature of the rectum is between 100° and 102° Fahr.; he denies that the ventricle is the same, and fixes it from one to three degrees higher. These conclusions have been confirmed by many experiments on human urine, the temperature of which varies from 101° to 103° Fahr. Taking into consideration that the lower part of the abdomen is lower in temperature than the upper, our author fixes the standard of animal temperature for man at 103° Fahr.

(*b.*) This standard is nearly constant under ordinary conditions, and the influence exercised upon it by a change in the temperature of one extremity is very unimportant. But though the temperature of the mass of the blood is unaffected, yet if one hand be plunged into a freezing mixture, the opposite hand rapidly loses its heat. In most cases this loss amounts to 6° or 8°, but in an extreme case the hand was observed to lose 22° Fahr. in seven minutes; meantime, the temperature under the tongue was very little, if at all, altered. We will here quote M. Brown-Séquard's explanation of the foregoing experiment.

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* In the Philosophical Transactions for 1845.

> "It is evident that the chilling of the hand kept in the air is a consequence either of the cooling of the blood, or of a diminution in the quantity supplied to the hand. We know that the former does not take place. The supposition then remains that the quantity of blood arriving in the hand is smaller than usual. It is certain that the heart still continues to send out the same [total] quantity of blood. Therefore, we are induced to attribute the lessening in the supply of blood to a contraction in the capillaries of the hand.
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> "It is well known that under the influence of certain sensations or emotions, the hands or feet become cold. The nervous system, in consequence of that sensation or emotion, acts upon the bloodvessels, and excites them to contract; for the calibre of the cutaneous bloodvessels is sensibly diminished. The same phenomena take place in the two hands when one is dipped into very cold water. An exceedingly violent pain is felt, the nervous centres are strongly excited, and they then act as under the influence of an emotion."

(*c.*) The temperature of the blood exercises an important influence upon asphyxia. The lower the temperature of the animal at the time of experiment, the longer the duration of life under asphyxia.

Omitting the investigations on birds and reptiles, and quoting a few out of many experiments on mammalia, we find that if a ligature be applied simultaneously on the tracheæ of two adult mammals, one being at its normal temperature, and the other nine degrees below it, the former will not live for more than three minutes; the latter will survive for eight minutes. Of four adult rabbits, whose temperatures at the time of the commencement of asphyxia were 103°, 95°, 86°, and 77° Fahr. respectively, the corresponding durations of life were 6, 9½, 10, and 14 minutes. Of three guinea-pigs—one having a temperature of 104° Fahr., another 95° Fahr., the third being at 86° Fahr.—under asphyxia, the first died in two minutes and three-quarters, the second in five minutes and a half, the third in twelve minutes.

The diminution of all the functions of animal and organic life, in consequence of the loss of temperature, and bearing a direct proportion to its degree, is considered by M. Brown-Séquard to explain the diminished necessity for oxygen, and therefore the longer duration of life under asphyxia. On the same hypothesis he explains the persistence of life in cholera, scleroma, and other diseases, where the frequency of respiration is reduced to a minimum.*

(*d.*) Sir B. Brodie, and more recently Chossat, Duméril, and Demarquay, ascertained that certain poisons sensibly diminish the temperature of the blood, thereby causing death before sufficient time has elapsed for their specific action to operate on the system. Among these poisons are opium, cyanide of mercury, diluted acetic and sulphuric acids, and probably many others. Our author has confirmed these conclusions, by discovering that the effects of many of these poisons may be obviated by maintaining the animal temperature. For instance, of two animals with equal doses of the same poison, if one be placed in a temperature of 50° Fahr., and the other be maintained at 88° Fahr., the former will die with great loss of its animal heat, while the latter will completely recover. In this way the usually fatal results of coating an animal's skin with gelatine or varnish, may be avoided.

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* Connected with this subject will be found most useful tables, of the duration of life under asphyxia in different animals (pp. 47–53), in Experimental Researches, by M. Brown-Séquard.

III. *The Nervous System.*—By far the larger number of M. Brown-Séquard's experiments have been made with a view to extend the knowledge of the anatomy and physiology of the nervous system. We will endeavour to give a short summary of his discoveries and his conclusions respecting this extensive subject.

(*a.*) It would appear that the influence of the nervous system upon the functions of organic life has been much over-estimated; and that the influence it exerts is not essential to their performance. Both nutrition and secretion will continue for a limited time when the nervous force has ceased to act in a part. This force takes, indeed, a certain share in nutrition, partly by its influence upon the change of arterial into venous blood—for example, in the respiration of muscles, as it has been aptly called by the younger Liebig—and partly by its regulation of the calibre of the bloodvessels: but the atrophy following loss of nervous power in a limb, which has been regarded as a chief instance of the direct effect of the abstraction of nerve-force, is in all cases simply from disuse, and may be always prevented or cured by keeping the muscles exercised by galvanic stimulus. Animals have been kept alive and in health for some months, without any spinal cord below the fifth cervical vertebra. During this time the secretion of urine, and other organic functions, the growth of hair and nails, and the general growth of the body, have continued; while muscular atrophy has been prevented by galvanism. The change from arterial into venous blood is not so complete in a paralysed limb as in a healthy one; but this also may in a great measure be remedied by exciting muscular contractions, which quickly darkens the colour of the venous blood issuing from the palsied limb.

(*b.*) In many of the vertebrata, the force exercised by the reflex function of the spinal cord far exceeds the most strenuous voluntary efforts: this is especially the case in reptiles and birds, where the development of the reflex function reaches its maximum. The following experiments prove that the reflecting power is a vital attribute of the spinal cord, and dependent upon its nutrition. A pigeon, with its spinal cord separated from the brain, can be made to raise with its feet, by fractions, a weight equal to fifty pounds, to the height of one inch. This power continues so long as the circulation of blood goes on in the spinal cord; but if the carotid artery be divided, and the animal suffered to bleed to death, it is at once lost. It may be restored again by injecting blood through the open carotid. Moreover, animals which manifest the reflex faculty of the spinal cord in the highest degree are, for the most part, though not exclusively, those in whom the supply of blood to the cord flows through lateral or transverse channels, and therefore is less liable to be influenced by transverse section of the cord, than in those where the chief supply of blood is through longitudinal arteries, which are necessarily divided in separating the cord from the brain. Consistently with this, it is found that the reflex faculty exists, in different degrees of intensity, in the several classes of vertebrata, and is least developed in fishes,* more so in mammalia and amphibia, and most of all in reptiles and birds. In man, the uncertainty of the manifestation of the reflex function is probably due to the same cause—viz., to the longitudinal supply of blood to his spinal cord through the anterior and posterior spinal arteries.

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* There are many individual exceptions to this order—for instance, carp, eels, and tench all possess a high degree of reflex power.

To trace the development of the reflex faculty in frogs and birds: We may observe that, immediately after section of the spinal cord, a period of shock supervenes, during which scarce any motor power exists. After the lapse of about five minutes, the reflex function begins to manifest itself; from this time, its power gradually increases for a period of forty-eight hours, when it has generally reached its maximum, and has exceeded the voluntary power; after remaining at this point for a few days, it decreases little by little, until, in the course of a month or six weeks, it has become less than the voluntary power. A frog which, before division of its cord, could lift 60 grs., directly afterwards could lift 20 grs.; fifteen minutes afterwards, 60 grs.; in twenty-five minutes it could raise 80 grs.; in an hour, 130 grs.; after four hours, 140 grs.; and forty-eight hours after the division of its cord, the reflex faculty had reached its maximum, and the frog could raise 150 grs. with its paraplegic limbs.

Magendie long ago discovered that animals poisoned by nux vomica frequently remained without convulsions, as long as they were preserved from the contact of external stimuli; but that convulsions could be immediately produced by irritating or even touching the animal's skin. If in a frog whose brain and medulla oblongata have been removed, strychnine be introduced into the stomach, no convulsions take place so long as the animal is undisturbed; but if the skin be touched, tetanus occurs at once. In birds, cats, or reptiles, if the whole portion of the spinal cord supplying nerves to the posterior limbs be removed, and a solution of strychnine be injected into the rectum, convulsions can only be excited in the anterior parts of the body where the corresponding part of the spinal cord is entire. No direct application of this poison to the surface of the muscles, nor any injection of its solution into a limb separated from the body, will produce convulsions. Again, if a ligature be placed on the abdominal aorta, and strychnine be injected into the general circulation, universal tetanus ensues; in this case no poison has reached the lower limbs. Once more, if all the arteries going to the spinal cord be divided, and strychnine be introduced into the blood, no convulsions can be excited. From these experiments, our author concludes that this poison acts neither on the muscles nor nerves, but produces tetanus by its stimulating effect on the reflex function of the spinal cord.

(*c*.) Sir Charles Bell's discovery of the sensitive and motor roots of the spinal nerves, and his conclusions as to the existence of corresponding columns in the cord, induced many physiologists to repeat his experiments. MM. Magendie, Mayo, Fodera, Schoeps, and others, by a more extended series of investigations, confirmed the former part of his discovery, but disproved the deduction he made from it, on the sensitive and motor functions of the corresponding columns of the spinal cord. Though these were the general results, yet the discrepancies in the effects of particular experiments were remarkable. Longet, by more careful manipulation, and by avoiding many sources of error, discovered, by complete transverse division of the cord, and by employing galvanism to the segments of the different columns, that the posterior columns were sensitive and centripetal, that the anterior were motor and centrifugal, and that stimulus of the grey matter appeared to produce neither sensation nor motion.

This view, with some slight modification, has been the one generally accepted; and the most popular opinion on spinal physiology has held, as regards the columns, the views of M. Longet, and has considered the grey matter as neither sensitive nor motor, but a reflector of impressions from sensitive to motor nerve-roots.

M. Brown-Séquard concludes that the posterior columns are sensitive, that they do not conduct directly to the brain; but convey impressions to the grey matter of the cord, which transmits them onwards: that the conductors of sensitive impressions, whatever they may be, on their entrance into the cord, for the most part pass downwards, and join the grey matter below their point of entrance; that a few ascend and join the grey substance above their point of entrance; and that a still smaller number at once lose themselves in the centre of the cord. All these fibres effect their crossing from side to side in the spinal cord, and no crossing of sensitive fibres takes place in the brain or medulla oblongata.

The anterior and lateral columns are motor and centrifugal, their fibres pass directly onwards from the cerebrum, and effect their crossing in the lower part of the medulla oblongata.

The grey matter of the cord receives sensitive impressions from the posterior roots of the nerves, conducts them onwards to the brain, or reflects them to the motor nerves. It is itself insensible to mechanical or galvanic stimulus.

The experiments and pathological facts that have led our author to his conclusions on the course and functions of the posterior columns, are the following:—A lateral half of the spinal cord being divided at the tenth dorsal vertebra in any mammalian animal, it is found that sensibility is increased in the posterior limb on the side of section; while sensation is lost in the posterior extremity of the opposite side. If, instead of one transverse hemi-section, two or three be made at different points on the same side, the same result will ensue. If two transverse hemi-sections, an inch apart, on the same side of the cord, be joined by a longitudinal median section of the cord, and the piece thus included be removed, the effect will be the same as in the previous experiments.

In making a transverse section of a lateral half of the cord, if the incision deviate so as to divide part of the grey matter of the opposite side, sensation will be impaired on the side of section, and will be lost on the opposite side. If, after a transverse section has been made of one lateral half of the cord at the eleventh dorsal vertebra, the opposite half be divided at the sixth dorsal, sensibility is lost in both posterior limbs, though there may be traces of sensibility on the side corresponding to the highest section of the cord. If in a rabbit, after having divided a lateral half of the cord at the level of the second pair of nerves, the sensitive nerves supplying the ears be examined, it will be found that the ear on the side of section is abnormally sensitive, while the opposite one has lost nearly all sensation. If a complete antero-posterior fission of the cord be made at the cervical enlargement, both anterior extremities lose their sensibility, while the posterior limbs are but slightly affected. If, in the same spinal cord, a transverse division of a lateral half of the cord be made at the centre of the longitudinal section, the opposite posterior limb will become insensible, while the posterior extremity on the side of section will be unaffected.

The difficulty of meeting with diseases in the human subject which affect one-half of the cord, while they leave the opposite half free to perform its functions, has prevented M. Brown-Séquard from collecting a large number of cases in support of these experiments on animals. Nevertheless, he has brought together abundant evidence to prove the truth of his statements, from several cases. We select the following, as well illustrating many of the foregoing conclusions.

1. A patient was admitted into the St. André Hospital, at Bordeaux, his symptoms were the following:—paralysis of voluntary movement on the right side of the body, with unimpaired sensibility. Motion perfect on the left side, with loss of sensation. After death a fungoid growth was found pressing on the right lateral half of his spinal cord.

2. A patient was admitted into the same hospital; he suffered from loss of voluntary power in the arm and leg of the left side, the same parts on the opposite side had nearly lost their sensibility. After death a clot of blood was found in the left lateral half of the spinal cord, in the cervical region.

3. A man, after having felt a sudden and severe pain in his back, lost the power of voluntary movement on the right side; sensation was not affected; on the left side, where motion was not affected, he completely lost sensation. After death a clot was found in the right side of the grey matter of the spinal cord, close to the anterior columns, in the cervical region.

The preceding cases and experiments tend to prove that the sensitive fibres cross in the spinal cord. Many anatomists have held that they cross either in the lower part of the medulla oblongata, or in the pons Varolii; if this were true, diseases of the pons or medulla should produce paraplegia, but from a collection of cases* it appears that disease of one-half of the pons or medulla produces loss of sensation in the opposite side of the body only. The following experiments induced M. Brown-Séquard to form the opinion already stated, on the functions of the posterior columns, and on the direction of their fibres. If the posterior columns of the spinal cord be successfully divided in a sheep or dog, it will be found, after the intense pain of the operation has passed away, that the animal can walk, though it has lost all sensation in the parts below the section. If the cut ends of the posterior columns be now irritated, the lower segments will be found far more sensitive than the upper. If the grey matter be accidentally wounded in making the section of the posterior columns, the lower segment of these columns will lose its sensibility, while the upper remains normally sensitive. When all the spinal cord, except the posterior columns, is divided, there is complete paralysis of both sensation and motion below the injury. In all cases of section of the

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* Experimental and Clinical Researches on the Anatomy and Physiology of the Spinal Cord. By E. Brown-Séquard, M.D.

posterior columns, provided that the grey matter was intact, the lower ends of the columns were invariably hyperæsthetic; and in no case did irritation of the grey substance produce either sensation or motion, though its perfect integrity was essential to the conveyance of impressions to the brain. If the posterior columns be divided, and the cut extremities be separated from their connexion with the cord for a short distance, the lower segment will be found more sensitive than the upper. Consistently with this downward direction of sensitive fibres in the cord, is the result of section of the posterior columns in the medulla oblongata, at the point where they form the restiform bodies; section here deprives the restiform bodies of sensation for some distance above the point of injury.

When a portion of one posterior column is included between two transverse sections, three-quarters of an inch apart, the cephalic segment will be found to be sensitive, the caudal hyperæsthetic, while the intermediate portion will have nearly lost all sensibility.

The functions of the anterior columns, and the course of the voluntary motor fibres in the cord, are more easily demonstrated by experiments. Complete longitudinal fission of the cord in an antero-posterior direction, does not affect the voluntary movements of the parts below. Transverse section of a lateral half of the cord produces paralysis of motion on the side of section, and loss of sensation on the opposite side. The crossing of the voluntary fibres does not take place in the spinal cord, but is probably effected at the decussation of the anterior pyramids, at the lower part of the medulla oblongata. In proof of this, M. Brown-Séquard adduces many experiments, and has collected several cases of disease in the human subject, all of which strongly support his opinion.

To sum up the different paralyses produced by disease in the several parts of the brain and spinal cord in man, we may say, 1st. When the disease is situated in any part of one half of the brain, except the medulla oblongata at the decussation of the pyramids, there will be cross paralysis of both sensation and motion. 2nd. When the disease occupies a lateral half of the inferior part of the medulla oblongata, at the decussation of the pyramids, paralysis of voluntary movement will take place on both sides of the body below the disease, while sensation will be lost only on the opposite side. 3rd. If disease implicate one lateral half of the spinal cord, voluntary movement will be lost on the corresponding side of the body, and sensation will disappear from the opposite side.

M. Brown-Séquard lays great stress upon the consideration that, as in the case of the grey matter of the cord, a part may possess the faculty of transmitting impressions, while itself is wholly insensible to those impressions. This observation loses somewhat of its interest, when we consider that a nerve fibre, or a nervous centre, sensible to its natural and immaterial stimulus, may be entirely insensible to our coarse and mechanical irritations by galvanism or otherwise.

(*d.*) As to the conveyance of impressions by nerve fibres,—M. Brown-Séquard supposes a typical nerve-fibre to consist of an axis-cylinder, the white substance of Schwann, and a membranous neurilemma; and has concluded, as the result of his investigations, that the part of a nerve fibre essential to the conveyance of impressions is probably the membranous neurilemma.

Of the three elements of nerve fibres: the axis is found wanting in many nerve-fibres, and the coagulation of the white substance of Schwann does not affect the transmission of impressions; thus these two elements are excluded, and there remains but the membranous neurilemma, which our author states to be the essential part of a nerve fibre.

(*e*.) The numerous experiments performed by M. Brown-Séquard have given him unequalled opportunities of observing the effects of certain injuries on the nervous system, and of ascertaining the power of repair possessed by nerves and nervous centres.

One of the most strange effects of injury to the spinal cord may be noticed in guinea-pigs and other animals in whom complete or semi-transversal section of the lower dorsal cord has been made. In these animals a convulsive affection comes on within twelve or fourteen days from the time the cord was divided. It consists in occasional attacks of convulsive spasm, affecting first the muscles of the head and face, including those of the eye, the tongue, and the lower jaw, and thence extending to the trunk and lower extremities. If a lateral half only of the cord be divided, the posterior limb of the opposite side is unaffected by the general spasm; if the transverse section of the cord be complete, neither of the posterior limbs is affected. These fits may occur quite independently of any external stimuli, or they may be produced at will, by frightening or pinching the animal. Of all parts of the body, irritation of the skin of the face or neck on the side of section will most surely produce an attack. The liability to this affection continues during many months, and in occasional cases has remained for a year or two. M. Brown-Séquard, during his recent visit to London, exhibited at St. Bartholomew's Hospital a guinea-pig, in which he produced a fit by pinching the skin of the face on the side of section. Very soon after the irritation of the skin, the muscles of the face began to quiver, then became convulsed, and in less than a minute the animal was stretched on its back, with its limbs extended, and with every muscle in its body (excepting those of one posterior limb) in a state of violent convulsive spasm. This continued for a minute or two, when an apparently involuntary discharge of fæces took place, and the animal began to regain its consciousness, and soon assumed its natural position, although for some minutes afterwards it had a stupid semi-conscious look. The whole fit lasted about five minutes, and to an ordinary observer was undistinguishable from epilepsy, though it is said to differ from the latter in that consciousness is not completely lost, the animal remaining sensible to a strong and painful stimulus. For some time after the recovery from a fit it is impossible to excite a fresh attack. All animals experimented on are not found liable to this affection in the same degree; guinea-pigs confined in a small space and largely fed were seized with violent and frequent convulsions; while those that were sparely fed and allowed to run about were either completely cured or suffered from the attacks much less frequently. The results of treatment on more than a hundred epileptic guinea-pigs are thus summed up by our author:—1. For each epileptic animal the number of fits in a given time is generally in a direct proportion to the quantity of food eaten. 2. There is an inverse proportion between the amount of exercise and the number of fits. 3.

Cauterization of the mucous membrane of the larynx is the most efficacious plan of treatment.

Among the effects of injuries to the nervous system, we have here placed a list of the various injuries that produce turning and rolling movements. These may be classified as—1st. Those that produce rotatory movements towards the injured side; and 2nd. Those that cause rolling on the opposite side to the injury. Under the first head are injuries or punctures of the crura cerebri; the corpora quadrigemina; the pons Varolii; the posterior part of the processus cerebelli ad pontem; the auditory nerve; the medulla oblongata at the origin of the facial nerve; the medulla oblongata outside the anterior pyramids; and the posterior surface of the medulla oblongata. Under the second head are injuries to the posterior extremity of the thalami optici; the crura cerebri; the anterior part of the processus cerebelli ad pontem; a small part of the medulla oblongata at the nib of the calamus, and injury to the facial nerve near its origin. M. Brown-Séquard has discovered a new movement performed by animals in whom the corpora quadrigemina and pons Varolii on one side have been punctured. After this injury the body of the animal remains straight, but when it walks it moves sideways, instead of going forward. In turning, it describes a circle, of which the longitudinal axis of its body is a radius; while its head is at the circumference, and its tail directed towards the centre.

(*f.*) The power of repair possessed by nerves and nervous centres is considerable. Wounds of the spinal cord may heal; and even after complete transverse section, the cord will unite and eventually regain its function. Divided nerves it is well known will re-unite and regain their power of transmitting impressions; in a guinea-pig, whose sciatic nerve had been divided and had re-united, no trace of union was visible, either to the naked eye or by microscopic examination, eleven months after its division.

In reviewing the effects of injuries to the spinal cord, M. Brown-Séquard comments strongly on the fallacious idea of danger attached to the exposure of the cord; and in the case of man, contrasts the comparative safety of its exposure with the almost certainly fatal results of its compression, and concludes by advocating the operation for raising the neural arches when depressed by violence and when interfering with the function of the spinal cord.

(*g.*) *The Sympathetic System.*—Budge, Waller, and Bernard agree with M. Brown-Séquard in considering the cervical sympathetic to have its origin in the spinal cord, and to be thence continued upwards to the brain. The general effects of section of the cervical sympathetic are well known and recognised as congestion and hyperæsthesia in the part to which the nerve is supplied. If galvanism be applied to the divided nerve, and it be stimulated to action, the bloodvessels begin to contract, and all the effects of congestion, the increased heat, vascularity, hyperæsthesia, and the contraction of the pupil, disappear. The effect of paralysis of the cervical sympathetic may be closely imitated by hanging an animal with its head downwards for ten minutes; this will quickly produce contraction of the pupil, suffusion of the eyes, increase in the vascularity, heat, and sensibility of the head and face. This similarity in effect indicates, what is very probable, that in both cases the same cause is in operation, namely, congestion; in the one case the effect of paralysis of the bloodvessels, in the other the result of the dependent position of the head.

These facts help to elucidate the hitherto anomalous results of galvanization of the vagus, as performed by Professors E. H. and E. Weber, who announced that if the pneumogastric nerve or medulla oblongata were excited by a powerful electro-magnetic stimulus, the movements of the heart were suddenly stopped. This is true if the extremity of the nerve alone be galvanized, but if the galvanic current be suffered to pass through the nerve and the muscular substance of the heart, the heart continues its pulsations until the galvanic current is interrupted, when it ceases to beat. From the effects of stimuli on other muscles, this is what should happen if the cessation were the result of permanent contraction; but after death the muscular substance is found flaccid and the bloodvessels empty. M. Brown-Séquard explains this in the following way. Considering that the vagus is chiefly a sympathetic nerve, and that its ultimate distribution is principally to the bloodvessels of the heart, and holding that these bloodvessels contain the stimulus to the rhythmical movements of the heart, it follows that the cessation of pulsation is the ordinary effect of stimulus on a vaso-motor nerve,—namely, contraction of the bloodvessels to which it is supplied, which, by emptying the capillaries, deprives the heart of stimulus to action. But when the galvanic current passes through the muscular substance, it substitutes a direct stimulus to its muscular irritability in place of the blood in its capillaries.

IV. *The Muscles.*—Our author's investigations on the muscular system bear chiefly on the condition of the muscles after death, and on the effect of the injection of blood in restoring and sustaining muscular irritability. These latter we have noticed elsewhere, as well as the contractions observed in the various contractile tissues after death by asphyxia, which will be found in a former part of this paper under the subject of the Blood.

(*a.*) *Rigor Mortis.*—In all cases in which, immediately before death, muscular irritability has been exhausted by prolonged or energetic muscular contraction, there is a proportionately quick approach and short duration of the rigor mortis, with a speedy supervention of putrefaction. In over-driven oxen, in animals hunted to death, in cocks killed in fighting, these effects are commonly observed. After death by strychnine and the various poisons producing convulsions, they may be seen in a higher degree, and in death by lightning they reach their maximum.

If to one dog strychnine be administered in a sufficient dose to cause death in a single convulsion, while to another it is given in such a dose as will cause death after about twenty or thirty convulsions, eight days will ensue before putrefaction is established in the former; but in the latter it will be found to exist eight hours after death. These experiments were performed at a temperature of 46° Fahr., and various poisons were used which it was found would produce the same effects; among these were morphine, nicotine, and hydrocyanic acid.

John Hunter stated, that in men and animals who were killed by lightning no rigor mortis followed. This assertion has recently been denied by Mr. Gulliver; it seems probable, from M. Brown-Séquard's investigations, that, if it does ensue at all, it only lasts for a few seconds, and does not remain sufficiently long to be recognised as post-mortem rigidity. As far as possible, the effects of lightning have been imitated on several mammalian animals; and we proceed to give the results of galvanic shocks of different intensity on five rabbits. When the hearts had been removed from all, one was suffered to die without galvanism; the others were subjected to galvanic currents, increasing in intensity from the first to the last. In the first animal who was suffered to die, rigor mortis lasted for 192 hours; in the second, for 144 hours; in the third, for 72 hours; in the fourth, for a day; and in the last, in whom the galvanic current had been strongest, rigor mortis only continued for fifteen minutes. Our author contends that if he can, by galvanization, reduce the duration of post-mortem rigidity to so short a period, the action of lightning, which far surpasses galvanism in the intensity of its action, may reduce the duration of rigor mortis to a minimum.

We here close our account of M. Brown-Séquard's discoveries, and would refer the reader to the work entitled 'Notices sur les Travaux,' for a more full account of many of his investigations. Our summary of the results of the inquiries of M. Brown-Séquard, can give but a faint impression of the amount of labour and the acuteness of mind required to arrive at the conclusions we have enumerated. Let the reader imagine how much ingenuity would be required to plan such experiments as we have referred to; how much dexterity to execute them; what patience to watch them—often through many weeks of daily or even hourly observation; what clearness of apprehension to interpret them aright; and what fulness of knowledge to compare such interpretations with the admitted truths of physiology, or to weigh them against its previously admitted errors,—let the reader, we say, imagine all this, and he may then form some fair estimate of the scientific merit of M. Brown-Séquard.

Thomas Smith.

REVIEW IX.

1. *Descriptive and Illustrated Catalogue of the Histological Series contained in the Museum of the Royal College of Surgeons of England prepared for the Microscope.* Vol. II. *Structure of the Skeleton of Vertebrate Animals.—London,* 1855. pp. 248.
2. *Manual of Human Histology.* By A. KÖLLIKER. Translated and edited by GEORGE BUSK, F.R.S., and THOMAS HUXLEY, F.R.S. (Sydenham Society.)—*London,* 1853. Vol. I. pp. 498.
3. *Human Osteology.* Comprising a Description of the Bones, with Delineations of the Attachments of the Muscles; the General and Microscopic Structure of Bone and its Development. To which is added, a Brief Notice of the Unity of Type in the Construction of the Vertebrate Skeleton. By LUTHER HOLDEN, F.R.C.S., Demonstrator of Anatomy at St. Bartholomew's Hospital.—*London,* 1855. pp. 204.
4. *Osteological Memoirs.* No. I. *The Clavicle.* By JOHN STRUTHERS, M.D., Fellow of the Royal College of Surgeons of Edinburgh, Lecturer on Anatomy.—*Edinburgh,* 1855. pp. 90.

So universally is it recognised that a correct knowledge of the bone forms the groundwork of practical anatomy, that we need not be surprised at the appearance of two new works on Osteology. The one is intended as a text-book for students, but is equally serviceable as a book of reference to the surgeon. The other, of which the first number—which treats on the Clavicle—is alone before us, goes into its subject far too elaborately to have a chance of being a student's book, but records observations on the bone with an accuracy that will make it a valuable aid to the surgeon or lecturer. We remember when the so-called French anatomy was first introduced into our schools, the sort of contempt which was expressed at what was considered its unnecessary minuteness. How astonished would people have been at seeing a work of ninety octavo pages devoted to one small bone alone! Why, Cruveilhier, who was thought to have exhausted the subject of descriptive anatomy, dismisses it in six pages; yet Dr. Struthers has shown that a great number of very useful and practical observations were still to be made, and we hope that he will continue his memoirs on other bones. Nothing in nature is too minute or trivial to be unworthy of patient investigation, and we are quite of opinion with the author, that

> "Each new discovery, or method, or addition, opens the way to further research and thought, and each new and greater application brings out new facts and principles, which gradually unfold themselves under the patient exercise of observasion and thought, the combined use of the bodily and mental eye."*

The study of the minute anatomy and development of bone is to the histologist what that of the special anatomy of the skeleton is to the surgeon or comparative anatomist. The properties of permanence and resistance, which bone must necessarily possess to fit it for its office, render it especially available for microscopical investigation. The changes which take place in its development from cartilage—the arrangement of its tissue—the nature of the processes by which it grows or is renewed—can all be observed under fewer interfering circumstances than perhaps any other tissue in the body. Hence cartilage and bone afford the readiest means for investigating structure and development; and although the subject be special, yet whatever can be accurately determined with regard to it, will throw additional light on what is known of other tissues. The Palæontologist also finds his advantage in studying the minute structure of bone. There are certain general differences of character in the bone tissue of animals, the discrimination of which has led to the determination of the position of extinct animals, where portions only of their bones have been found.

Towards the attainment of this latter object, the work which stands first on our list is by far the most valuable aid which has appeared. The histological department of the museum of the Royal College of Surgeons bids fair, under the able direction of Professor Quekett—to whom it owes, we might almost say its existence—to rival in completeness its more imposing neighbour. We believe that when Mr. Quekett was first connected as a student with the Hunterian Museum, it possessed only about 150 microscopical specimens, which, according to Mr. Clift's report, were made by Hewson. In 1841, the Council purchased the collection of the late Dr. Todd, of Brighton, containing 1558 specimens. Since Mr.

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* Osteological Memoirs, preface, p. iv.

Quekett became connected with the museum, we understand that up to the time of the publication of the Catalogue, between 8000 and 9000 specimens have been added by him, or under his direction; and every day, new and valuable additions are being made by him. In the year 1848, he began the formation of the 'Histological Catalogue,' the second part of which is now before us. None but those who have attempted the task can appreciate the vast amount of labour, and of very uninteresting labour too, to those who have made the preparations, which has been here accomplished. In the present part, accurate descriptions of no less than 945 specimens of the minute structure of the bones are recorded, illustrated by 440 drawings of the four classes of vertebrata, recent and fossil. The subject, for all practical purposes, may be looked on as exhausted; for though, of course, the structure of the bones of a large number of genera is not here described, yet any more detailed account would add little or nothing of real value, as it would be but a repetition of what is amply illustrated in the work.

Prefixed to the descriptive catalogue is a short account of the structure and development of bone, a subject on which we purpose presently to make a few observations. Professor Quekett was the first to draw attention to the fact that there seems to be a relation between the size of blood-corpuscles and of lacunæ. He here repeats the observation:

> "A remarkable relation in point of size has been found to exist between the blood-corpuscles and the lacunæ of the same animal: thus, for instance, in the siren and lepidosiren, animals having the largest blood-corpuscles, the lacunæ are also the largest; in the sloths, the blood-corpuscles are larger than in any mammal, except the elephant; in one of these—the three-toed species—the lacunæ are the largest. There are, however, some few instances in which this rule has been departed from, and these are alluded to in different parts of the present volume."*

In looking through the measurements of the lacunæ given in the descriptions, and comparing them with the ascertained measurements of the blood-corpuscles of the same animals, this observation appears to be, to a great extent, correct. Still there are many exceptions—some so striking as to raise a doubt whether there can be any essential connexion between the dimensions of these two structures. Thus, birds with far larger blood-corpuscles have generally smaller lacunæ than mammals; and even in comparing animals of the same class, the absence of this connexion is very marked. The musk-deer, for example, with the smallest blood-corpuscles, has lacunæ not much smaller than the mouse, in which the blood-corpuscles are more than three times as large, and indeed, nearly as large as in man. The question of greatest interest, however, to which the work will materially help to furnish an answer, is, whether the bone-structure presents any special differences in the different classes or orders of animals, such as are furnished by the dentine and enamel of the teeth, by which the positions of portions of bone of extinct or unknown animals might be determined? Mr. Quekett has given particular attention to this subject, and we believe that constant observation has trained his eye to a power of ready and accurate discrimination possessed by few. To those who have not an extensive collection of preparations, the plates will serve as safe and good substitutes from which to draw conclusions, or with which to compare unknown specimens. It seems clear that there are *general* characters by which the bone tissue in the four great classes of vertebrata may be distinguished, although at the confines of each class there is a tendency to gradual transition. Thus the bone-structure of the lower mammalia resembles that of birds; while in the bird there is some resemblance to the higher reptile. As might be expected, the different orders cannot be so easily recognised by the characters of the bone-tissue—especially in the higher classes. Even in the mammalia, however, there are many animals which may be thus recognised—the whalebone-whale and cachalot, for example—the monotremata, some of the marsupials, and the sloths. Some of the monkeys, even, as the spider monkey and short-tailed monkey, have peculiar characters of bone-structure.

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* Histological Catalogue, Introduction, p. xxiii.

The descriptive catalogues of the College museum are undoubtedly the most valuable records of the kind extant; and the present series reflects additional credit on the Council, under whose auspices it has been undertaken, and will remain a lasting memorial of the energy and of the extensive and accurate observation of the histological professor by whom it is compiled.

In the account of the structure and development of bone, which forms an introduction to his work, Mr. Holden adopts in general the views of the authors of this article, contained in a paper in the 'Philosophical Transactions' for 1853. The editors of the Sydenham Society's edition of Kölliker's 'Manual,' also, in the very valuable notes with which they have enriched it, while they admit their general correctness, demur to the interpretations of some of the observations recorded in that paper. More recently, Mr. Huxley, in an article of great power and ability, published in the twelfth volume of this Review, endeavours to show that the cell theory of Schwann is inadmissible in the case of many tissues, which are generally believed to be cell products; and gives an explanation of some of the appearances found in bone, based on his ideas of the laws of development.

The general views of which Mr. Huxley is the most able exponent in this country, cannot be now discussed. The article referred to demands a careful study; and it certainly indicates that the whole subject of development requires reconsideration. The negative character of the nucleated cell as an instrument of development, is maintained with great acuteness and ingenuity; and cogent reasons are undoubtedly adduced for at least a modification of the prevailing doctrines.

We may be allowed perhaps to re-state the general conclusions at which Mr. Huxley has arrived, but must refer to the article itself for the arguments in support of them.

> "We have brought forward evidence to the effect that this primary differentiation is not a preliminary to further organization—that the cells are not machines by which alone further development can take place, nor, even with Dr. Carpenter's restriction, are to be considered as 'instrumental' to that development. We have tried to show that they are not instruments, but indications—that they are no more the producers of vital phenomena, than the shells scattered in orderly lines along the sea beach are the instruments by which the gravitative force of the moon acts upon the ocean. Like these, the cells mark only where the vital tides have been, and how they have acted.

"Again, we have failed to discover any satisfactory evidence that the endoplast, once formed, exercises any attractive, metamorphic, or metabolic force upon the periplast; and we have therefore maintained the broad doctrine established by Wolff, that the vital phenomena are not necessarily preceded by organization, nor are in any way the result or effect of formed parts, but that the faculty of manifesting them resides in the matter of which living bodies are composed, as such; or, to use the language of the day, that the 'vital forces' are molecular forces."*

Before entering into the question, whether in the case of bone formation, the cell does not play a more important part than is here assigned to it, it may be as well to state very briefly what we believe to be the structure of bone and the mode of its development in the higher animals.

Bone, then, may be formed in cartilage—primary bone; or in a layer or mass of cells closely aggregated, and without or with very little apparent intercellular substance—secondary bone. It is unnecessary here to re-open the discussion whether, morphologically, this distinction is or is not correct. Of one thing there is no doubt, viz., that so-called primary and secondary bone are so different in general characters, that the smallest part of the former may be distinguished from the latter when lying embedded in it, as in the processes of growth is by no means uncommon.

When bone is about to develope in cartilage, the cells of the cartilage multiply by repeated bipartition, and increase in size to so great an extent as to encroach on and diminish the intercellular substance. Ossific granules are deposited in the intercellular substance, and at the same time the cells undergo change, partially ossify, and are found imbedded in the crypts formed by the ossified intercellular substance, their interior representing stellated lacunæ. This primary bone has usually but a short existence.

At a very little distance below the point where ossification is advancing, a process of absorption has already begun in the bone; irregularly margined spaces, of various sizes, are found, eroded, as it were, in it. These are entirely filled by masses of soft cells, so thickly clustered as to leave but little room for blastema. In these spaces the secondary bone is formed, apparently by the ossification of the hitherto soft cells and of the intercellular matrix or blastema. Again and again is this process repeated. In fact, the absorption in mass of the older bone, and the deposition of new in the spaces thus produced by absorption, are found to occur throughout life. The process is of course most frequently repeated in young growing bone, especially in the shaft of the long bones, and at the same time additions of new bone are being made to the surface by a process of ossification in all respects similar. In young bone which is thus constantly undergoing change, the tissue does not present that definite arrangement or lamination which is seen in older and more permanent tissue, and which is the result of the arrangement of the cells and of their ossification in successive layers. The lacunæ in secondary bone are not simply spaces left in the process of development, but, like those in primary bone, are cells with ossified walls imbedded in the tissue.

Now if this be a correct view of the history of bone development—and it is supported by the authority of Professor Quekett in the work before us—we think that it goes some way to indicate that cell agency is at work; or, at least, that cells are made subservient to formation and growth. The adoption of such a conclusion will of course depend on the opinion of the reader as to whether the cell is ever an instrument, and is not always a result merely, of vital action.

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* British and Foreign Medico-Chirurgical Review, p. 314. Oct. 1853.

In the whole course of the changes which occur in cartilage, from its first development to its ossification, those which are taking place in the cells are the most remarkable. At first cartilage consists of cells only, which become separated by the addition, perhaps to their walls, perhaps external to them, of interstitial substance. So long as the cartilage is simply growing, no further change takes place in them than their multiplication in an apparently irregular manner; and unless a further development into bone is intended, the cells do not alter their appearance. But the first step towards ossification seems to be not merely a multiplication, but a great increase in the size of the cells, with change of form of their contents, and the previously abundant intercellular substance is proportionately diminished. Then, and not till then, the calcareous salts, which previously existed only in very small quantity in the tissue, are eliminated from the blood, and deposited in the cells and intercellular substance. But the soft contents of the cell still remain embedded in the ossified cell wall, which now forms a lacuna. This looks as though there were a necessary connexion between cell growth and bone deposit—whether of cause and effect cannot of course be determined by the simple observation of the fact.

The connexion, however, between cell and bone formation is more striking in the instance of development of secondary bone. But we may first allude to those *spaces*, to which we have given the name of "Haversian spaces," produced by the removal of bone already formed, and which become filled up again with bone arranged in the form of Haversian systems. These spaces Mr. Huxley considers to be the effect of a process of vacuolation "strictly comparable to that described as giving rise to the areolated connective tissue." Now the description of the process of vacuolation giving rise to areolated connective tissue, certainly does not agree with anything which we have seen in the formation of Haversian spaces.

> "If the portion of young gelatinous tissue," Mr. Huxley observes, "which lies immediately adjacent to the epidermis or epithelium, be examined, it will be found to present a structure in all respects similar to fœtal cartilage; that is, there is a homogeneous matrix in which the endoplasts are dispersed. If this be traced inwards, it will be found that the endoplasts become more widely separated from one another, and that the matrix in places between them is softened and altered; while in their immediate neighbourhood, and in the direction of irregular lines stretching from them, it is unaltered. This is, in fact, the first stage of that process which we have called vacuolation. In this condition the intermediate softened spots still retain sufficient consistence not to flow out of a section; but yielding, as it does, in these localities much more readily than in others, it is easy enough to tear out the firmer portion in the shape of 'cells,' which are fusiform, irregular, or stellate; and the whole tissue has therefore been described (Reichart, Virchow, Schwann) as consisting of cells, connected by an 'intercellular substance.' Both 'cell walls' and 'intercellular substance,' however, are portions of the same periplast, and together correspond with the matrix of the cartilage. When, therefore, in the course of further development, the 'intercellular substance' becomes quite fluid and so disappears, the outer portion of these cells being converted

into fibrillated collagenous tissue, and the inner into elastic substance, we have, notwithstanding the apparently great difference, in reality exactly the same mode of metamorphosis of the same elements as in the preceding instance."*

Here, then, is a clear account of a gradual and progressive change from a firm matrix down to complete fluidity. But the examination of a Haversian space shows us a cavity uniformly filled with cells, which require no tearing to separate them, but float off, free, and of uniform size, when simply placed in fluid. The boundary of the space is firm bone, quite unchanged in character, but the cells which lie in immediate contact with it in no way differ from those which lie in the more central parts, and which have been previously formed. We discover here, then, no intermediate progressive stage. The only impression which we receive is, that from some central point—probably the neighbourhood of a vessel—the bone is attacked, and removed atom by atom, its place being occupied by small granular-looking cells, "the cellular mass presenting a perfect cast of the surface of the bone, suggesting to the mind that the soft was growing at the expense of the hard tissue, or at all events, that the former was instrumental in the removal of the latter." There is, in short, no intermediate condition discoverable between firm natural bone and granular cell. Now, if the term vacuolation may be applied alike to this process and to that by which it is assumed that areolated connective tissue is formed, we do not see what advantage is gained by its introduction, as it only expresses a condition already familiarly known under the name of absorption.

This, then, is the appearance presented by a Haversian space in recent bone—a space with irregular sharply emarginated borders, and filled with small granular nucleated cells. The process by which the absorption is effected has its limits, and when these are attained a new series of changes begins. Ossification takes place in the spaces: and we believe that the mode in which it occurs is capable of easy demonstration. Whether there be any alteration in the cells in the Haversian spaces previous to ossification, cannot perhaps be determined; none, at any rate, is perceptible; but it is seen that a layer of cells lying next in contact with bone has become darker, and adheres to the bone. The cells are found, on examining the bone in a dry state, to be converted, together with some intermediate tissue, into a bony structure. Then the layer next to them undergoes a similar process, and so on, until the ossification of the part is completed. Where a Haversian system with definite lamination is to be formed, the cells are arranged row by row, each row with perhaps some intermediate tissue representing a lamina; where, however, lamination does not occur, the ossification takes place without such arrangement of the cells, the conversion of which into bone is nevertheless equally obvious. Immediately around the Haversian canal, which is left as a central space in the midst of the ossified cells, a greater or less quantity of clear homogeneous calcified matrix is found, which in the first instance is perhaps a simple transudation from the vessel.

The editors of Kölliker's 'Manual' do not admit the correctness of this view.

"We have never been able," they say, "to find evidence of any of the corpuscles becoming converted into 'osteal cells,' and we believe, for the following reasons, that this does not take place. In examining the growing Haversian canals in man, and particularly in the calf, we have frequently found the innermost layer transparent, glassy, and structureless, exhibiting nothing but the corpuscles lying in lacunæ without canaliculi. This layer would be as much as the one-two thousandth of an inch thick; in the layer immediately external to it, however, the 'osteal cells' were exceedingly well marked. The inner layer looked like smooth ice, and the outer like ice which had cracked into innumerable tolerably even portions; but these cracks were by no means produced by the canaliculi, which, as yet, were hardly at all developed. Now, it seems clear, that if the 'osteal cells' were produced by the calcification of certain of the corpuscles, they ought to be more obvious in the young inner layer than in the outer; whereas, just the reverse occurs. The fact stated by Messrs. Tomes and De Morgan, that lamination is less obvious in young than in old bone, tends to exactly the same conclusion. Again, if the granular substance between the lacunæ were composed of calcified corpuscles—'osteal cells'—the action of acids ought to bring them out as strongly as it does those of the lacunæ; whereas, neither in young bone nor in old can anything of the kind be seen."*

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* Loc. cit., p. 310.

The description of the inner transparent lamina of recently formed Haversian systems does not materially differ from that which we have given in our paper:

"The inner lamina forms, when the development is completed, a perfect ring, and not unfrequently presents a second peculiarity. The tissue of which it is composed is transparent, and affords, with our present means of investigation, little evidence of structure."†

According to our observations, however, this transparent lamina is much more frequently seen in old than in young growing bone. The objection that lamination is less obvious in young than in old bone, requires a clear understanding as to the application of the terms young and old. By young bone may be meant, either the bone of a young animal, or the new tissue deposited in a Haversian space or elsewhere in the bone of an adult. In the long bone of a young rapidly growing animal it is true that lamination is often absent—that is, in a bone undergoing constant change there is not the same tendency to regular arrangement as in bone intended to be more permanent. But in new bone deposited in the midst of old, lamination is distinct; though sometimes, from the greater abundance of the canaliculi, it is less pronounced at first view; for the clearness of lamination is always in the inverse ratio to the abundance of the canaliculi. In either case, however, the appearance of ossified cells is clear and defined. And to the statement, that neither in young nor in old bone can any appearance of cells be found when decalcified by acids, we must express our entire dissent, as we find no difficulty in demonstrating their presence more clearly than in the unaltered bone. One thing must be insisted on—viz., the necessity for using the most perfect means of definition. What with an ordinary reflector or condenser will appear a confused mass, will often, under a Gillett's condenser, come out as a regular well-defined tissue.

Another, and perhaps a stronger, argument in favour of the view that cells minister to the development of bone tissue, may be drawn from the fact, that when ossification is taking place in connexion with fibrous tissue—as periosteum, for example—the fibrous elements are, in whole or part, replaced by cells. This fact is the more striking if the fibrous be, as the editors of Kölliker maintain with Reichart, a homogeneous tissue with a plaited or fibrous arrangement. If the development could take place in a tissue homologous, as it is believed, with the matrix of cartilage, without the intervention of cells, why should the cell formation precede the ossification, and take the place of the previous tissue?

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* Kölliker's Manual, vol. i., p. 378, Syd. Soc. Edition.

† Phil. Trans., 1853, p. 113.

What, then, is the signification of these cells which are thus found to be so universally present wherever bone is forming? This is perhaps matter for conjecture only. We are inclined to suggest the following view:—The process of calcification is as much one of excretion of salts from the blood, as the separation of bile is an act of excretion of its elements or its organic compounds by the liver; or as the separation of urea is an act of excretion by the kidney. But in all glandular excretions—wherever, that is to say, any removal of special materials from the blood takes place—*there* is found the nucleated cell or nucleus; and these cells, as in the liver, or kidney, or testis, are, we believe, universally admitted to be the agents of separation. If the cells degenerate, the secretion is stopped. Indeed, wherever active processes are going on—in the higher animals, at least—there, and in a direct ratio to that activity, cells exist. Is it not probable that, in the separation of calcareous salts from the blood, the cells, instead of being merely incidental structures, in reality determine and become the seats of that process; and that, in fact, so far as the arrangement of its tissue is concerned, there is no essential difference between the formation of bone and that of horn or any other epidermic structure? It is a begging of the question to deny this on the ground of the two tissues being homologically different. Is our present knowledge of development sufficiently advanced to enable us to determine what parts are, so to speak, histologically homologous? We think not. Nor do we think that any satisfactory argument in opposition to such a hypothesis can be derived from the fact of certain lower organisms, the so-called unicellular animals and plants, possessing no structure. Mr. Huxley says in reference to them:

> "If it be once admitted that a particle of vitalizable matter may assume a definite and complex form, may take on different functions in its different parts, and may exhibit all the phenomena of life without assuming the cellular structure, we think it necessarily follows, that the cells are not the centres of the manifestation of the vital forces; or that, if they be so, the nature of these forces is different in the lower organisms from what it is in the higher; a proposition which, probably, few would feel disposed to maintain."

Now, we confess that, either we do not clearly understand this argument, or it is one which, if pressed to its legitimate conclusions, would require us equally to admit that a nervous system, where it exists, is not the centre of the manifestation of nerve force, or muscle of contractile force, because both volition and contraction are manifested in animals possessing neither of these systems. If the special manifestations of vital force are made dependent in the higher animals on special organization, surely that manifestation of it which results in the formation of definite structure may be made equally dependent on organization. But, it may be argued, there are many of the lower animals possessed of complex internal structures, yet in whom no cells are found taking part in, or co-existing with, their development—in whom, in short, the nucleated cell does not make its appearance at all in connexion with these structures. Be it so. If, however, this be proved, we think it is a fact which cuts both ways—or rather, that it is one which entirely bears out the cell theory. Before, however, entering on this question, we may refer to a paragraph in Mr. Huxley's paper which seems to embody his views on the meaning of the presence of cells in the animal body:

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* British and Foreign Med.-Chir. Review, vol. xii., p. 304, Oct. 1853.

> "As the whole animal is the result of a differentiation of a structureless yelk, so is every tissue the result of a differentiation of a structureless blastema; the first step in that differentiation being the separation of the blastema into endoplast and periplast, or the formation of what is called a 'nucleated cell.' There, just as in the development of the embryo, when the blastodermic membrane is once formed, new organs are not developed in other parts of the yelk, but proceed wholly from the differentiation of the blastoderm; so, histologically, the 'nucleated cell,' the periplast with its endoplast, once formed, further development takes place by their growth and differentiation, into new endoplasts and periplasts. The further change into a special tissue, of course, succeeds and results from this primary differentiation, as we have seen the bodies of the vertebræ succeed the chorda dorsalis. But is there any more reason for supposing a causal connexion between the one pair of phenomena than between the other? The cellular structure precedes the special structure; but is the latter therefore the result of a 'cell force' of whose existence there is, on other grounds, no evidence whatever? We must answer in the negative. For us, the primarily cellular structure of plants and animals is simply a fact in the history of their histological development—a histologically necessary stage, if one may so call it, which has no more causal connexion with that which follows it than the equally puzzling morphological necessity for the existence of a chorda dorsalis or of Wolffian bodies has with the development of the true vertebræ or of the true kidneys."*

Without stopping to inquire what proof there may be that there is no causal connexion between the existence of the chorda dorsalis and the development of the true vertebræ, we may return to the question, whether the fact, if it be one, that in some of the lower animals the development of complex organs occurs without the intervention of cells, is not, on the whole, favourable to the cell theory as applied to cases in which cells do exist, and to the same extent opposed to the views of Mr. Huxley. We would wish it, however, to be expressly understood, that we are not in a position to support or deny the *universal* application of Schwann's theory to animal development, but are arguing only in favour of its probability in relation with certain tissues of vertebrated animals.

If it were found that the first step from a structureless blastema towards the formation of a perfect tissue was invariably the formation of a cell, it might be plausibly assumed, unless direct evidence to the contrary could be shown, that the cell was a mere intermediate stage—a necessary result of a differentiation into endoplast and periplast, not itself an instrument, but only an indication. But if, as is believed, development can and does take place in animals possessed of complex structure, without any such intermediation, it would be only fair to conclude that the presence of cells in connexion with development is something more than an indication, and is really an evidence of some modification of the mode in which it takes place, and to which they are instrumental. Could it be maintained that the absorbent gland, for example, which is nothing more than a compact plexus of absorbents, containing an abundance of nucleated corpuscles, is not an active agent, because in the lower animals there are no absorbents at all? or that the red-blood corpuscle, which is either a cell or a nucleus, is not an active agent because development goes on, and all kinds of animal and organic functions are performed in the lower animals without its presence? No one would deny, we imagine, that in these and many other instances, the presence of the cell elements is a sufficient proof of some higher, or at least some modified, manifestation of vital force of which they are not the indication, but the instruments. And we see no reason whatever why the same argument should not apply to the cell, wherever found, inasmuch as its presence is not a necessary condition of development.

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* Loc. cit., p. 303.

What share may be taken by the nucleus, or what by the cell wall, in any change which goes on in or around them, is a point perhaps impossible to determine. Looking to the simple fact, that when a tissue is growing or active, the nuclei are usually abundant; but that in general, when it has arrived at maturity, or degenerates, they disappear, the impression is naturally given, that these organs may be of more importance than many are disposed to admit. In our minds, this impression is strengthened by observing the nature of the lacunæ. They appear as small cavities, and, as is generally allowed, they contain nuclei. The importance of this depends very much on the real mode of their development. Are they merely spaces left during the calcification of the surrounding tissue? or are they cavities contained in walls formed by the ossification of a separate element? If the former, it might be argued that they are the necessary consequences of ossification occurring round a structure not prone to ossification; and that hence, whatever might be the use of the lacunæ themselves, the corpuscles or nuclei within them merely determined their position. But if the latter view be correct, it is not so easy to explain their formation on the supposition of their contents being accidents and not essentials. This view, however, we believe to be the right one, and not difficult of demonstration. At present we would only draw attention to the fact, that in certain situations, as in the cancelli of the flat bones of very old persons, ossified lacunal cells are found, some free, some attached to or imbedded in bone tissue. They consist of a granular cell, containing a nucleus, and surrounded by a thick transparent wall. That they are lacunal is shown by the fact that they are found at times imbedded in bone, and, like the cartilage cells, display every gradation, from the granular cell to the finished lacuna. The editors of Kölliker's 'Manual' suggest that they may be compared rather to the globules of dentine than to cells. But is it proved that the dentine globules are not ossified cells? If these bodies be really detached lacunal cells, does it not indicate that the lacuna, with its contents, is a distinct organ? and can it be that such an organ should be simply an indication, not an instrument? In the perfect lacuna, the only parts recognisable are the osseous wall and the nucleus; and this latter is the only part capable of undergoing or determining change, supposing such to be the office of the lacuna. But if the lacuna is not destined to some special purpose, why is it present at all? So far as the mere conveyance of nutritive fluid is concerned, the canaliculi would answer all the purpose. The fact, however, of the lacunæ in new bone being large, and the canaliculi proceeding from them numerous (the canaliculi being formed by a secondary process), and of the lacunæ in old bone being small, and the canaliculi few, with occasional obliteration of some of them, indicates a more than accidental connexion between the presence of these organs and the process of development and nutrition.

What may be the actual agency of these parts, it is of course impossible, in the present state of our knowledge of vital processes, even to surmise. All we would wish to be understood as maintaining is, that while, perhaps, good grounds have been adduced for assuming that, in many cases, phenomena which have been attributed to cell agency, are really due to molecular forces residing in all living matter however organized (and no one, we believe, can study the observations of the editors of Kölliker, and the admirable papers by Mr. Huxley on the subject, without coming to that conclusion), yet that there are facts connected with the structure and growth of bone (amongst other tissues) which will not permit us altogether to discard the hypothesis of cell agency as a means of development.

We have dwelt so long on the subject of bone development, that we have little space to devote to the remainder of Mr. Holden's work. Its object is, as the author states, "to teach the student the bones and the accurate attachment of the muscles." This object the work is well calculated to fulfil. The descriptions of the bones are full and minute, couched in plain intelligible language, and interspersed with many valuable practical observations. The drawings of the bones are made more available for reference by having the names placed opposite to the various points of demonstration, instead of figures referring to the letterpress. The plan of the plates is in one respect novel. The attachments of the muscles are indicated in coloured outline—the origin in red, the insertions in blue ink—and hence, as the names of the muscles are in each case placed within or near the outline, an inspection of the plate would afford to the student a good notion of the most important points in the anatomy of the bone. Altogether, we know of no work on osteology better suited to aid the student in acquiring a knowledge of this most essential branch of anatomy.

The latter part of the book is devoted to a consideration of the homologies of the vertebrate skeleton. The author tells us that this portion of the work has been supervised by Professor Owen, and, as we need scarcely mention, he adopts entirely the views of that distinguished anatomist. So far as is possible, he has divested the subject of technicalities, which is perhaps doing as much for it as could at present be expected.

C. De Morgan.
T. Tomes.

Review X.

General Board of Health. Instructional Minute relative to the Duties and Qualifications of Officers of Health in Districts under the Public Health Act, 1848. (Issued Dec. 20, 1855.) pp. 6.

The hundred and thirty-second clause of the hundred and twentieth chapter of the eighteenth and nineteenth of Victoria has caused more commotion in the medical coteries of the metropolis, since the passing of Sir Benjamin Hall's bill, which is typified by the formula "18 and 19 Victoriæ, cap. 120," than has probably ever been excited in the profession by so brief a passage. Though only a fractional portion of the bill, we cannot, as sanitary reformers, regard the clause otherwise than as one of its most important features. As but few of our readers may have met with the act, we give the clause entire:

> "Every vestry and district board shall, from time to time, appoint one or more legally-qualified medical practitioner or practitioners, of skill and experience, to inspect and report periodically upon the sanitary condition of their parish or district, to ascertain the existence of diseases, more especially epidemics, increasing the rate of mortality; and to point out the existence of any nuisance, or other local causes, which are likely to originate and maintain such diseases, and injuriously affect the health of the inhabitants; and to take cognisance of the fact of the existence of any contagious or epidemic diseases, and to point out the most efficacious mode of checking or preventing the spread of such diseases; and also to point out the most efficient modes for the ventilation of churches, chapels, schools, lodging-houses, and other public edifices within the parish or district; and to perform any other duties of a like nature which may be required of him or them. And such persons shall be called medical officers of health; and it shall be lawful for the vestry or board to pay to every such officer such salary as they think fit, and also to remove any such officer at the pleasure of such vestry or board."

The act acknowledges the duty of society to seek for the best information which science is able to afford in relation to the causation and prevention of disease; and it inculcates the necessity of applying to the medical profession as the only true authority on such matters. The parochial officers have had no such functions to perform; they were appointed for the purpose of treating disease after its appearance in the individual, though under a properly organized system of medical police throughout the country they might be rendered most valuable *aides-de-camp* of the officers of health.

The new medical officers of health receive their commission to act on the defensive, and to prevent and anticipate the inroads of the enemy, by means that shall at the same time enhance the value and enjoyment of life. The general outline of their duties, which is given in clause one hundred and thirty-two of Sir Benjamin Hall's act, embraces subjects to which medical men have turned their attention more than members of other professions; but it is very certain that, although the necessity for medical officers of health has long been felt, the new act will create a new order of medical men, whose special studies will be devoted to the matters adverted to. At present, we are acquainted with but few whose acquirements and practical knowledge would enable them, without further preparation, duly to respond to the call. This difficulty becomes yet more apparent when we examine the 'Instructional Minute' issued under the sanction and with the signature of the present President of the Board of Health.

Of this 'Minute' we cannot speak too highly. It throws a great responsibility upon the medical profession. It forms an exalted estimate of the duties to be performed, while it demands qualifications such as are to be found only among the best of our profession. In addition to a thorough general knowledge of medicine, the 'Minute' justly requires special qualifications in pathology, vital statistics, in chemistry, in microscopy, and in natural philosophy; we may add, in geology. We cannot sum up the reasons for such qualifications better than they are to be found in the official document. The officer of health must be well grounded

> "In *pathology*, because this science implies an exact study of the causes of disease in their relation to the living body,—a study of what they are, and how they act, and why they seem to vary in operation:
>
> "In *vital statistics* (properly a section of pathology), because, by analysing the composition of various death rates, and by learning how the pressure of particular diseases differs under different circumstances of climate, season, dwelling, age, sex, and occupation, definite standards of comparison are gained, without which the officer of health could not estimate the healthiness or unhealthiness of the population under his charge:
>
> "In *chemistry* (including the power of microscopical observation), because without such aid there can be no accurate judgment as to impurities of air and water, dangerous impregnations of soil, or poisonous admixtures in food; and because the same science also guides the application of deodorizing and disinfectant agents:
>
> "In *natural philosophy*, because many nuisances are traced, and many questions as to ventilation and over-crowding are answered by its laws; further, because by its aid the officer of health studies the atmospheric changes, and learns the climate of his district—important steps in proceeding to speak of its diseases; and finally, because natural philosophy in conjunction with chemistry renders him competent to report on many manufacturing processes alleged to be hurtful to health, and on the sufficiency of such means as are employed to reduce the evils ascribed to them." (pp. 3, 4.)

Further arguments why the medical officer of health should be possessed of these qualifications will become apparent by a perusal of the enumeration of the duties required of him. Thus he is to make himself acquainted—

> "With the levels, inclinations, soil, wells, and water-springs of the district; with its meteorological peculiarities; with the distribution of its buildings and open spaces, paved or unpaved; of its burial grounds and laystalls; with the plan of its drains, sewers, and water-supply; with the nature of its manufacturing and other industrial establishments; with the house accommodation of the poorer classes, and the facilities afforded them for bathing and washing; with the arrangements for burial of the dead, and with the regulations in force for lodging-houses and slaughtering-places; for the cleansing of public ways and markets; and for the removal of domestic refuse."

Of course it is necessary that the medical officer should be familiar with all these matters, in order that he may understand the various influences that operate in the production of epidemic or endemic disease, and have it in his power to suggest the remedy, where hygienic measures are imperfectly carried out. He will necessarily be much dependent upon the assistance rendered by the residents of his district, hence one of his functions will be to invite communications relating to the "sanitary wants of the district from the resident clergy, medical practitioners, registrars, relieving officers, and other persons or societies engaged in the visitation of the poor." The less, however, he depends upon extraneous aid, the more efficiently will he carry out his duties, the more valuable will the reports be, the more certainly will he command that attention and respect which we desire that he should command. All the matters adverted to, and many others connected with the preservation of health and warding off of disease, will form the subjects of the reports the medical officer of health is required to make to the local board. These reports will necessarily involve interests of a very varied and important character: they will render circumspection and tact quite as imperative as high scientific attainments; for, as we have seen it stated somewhere, "property may be libelled as well as character."

The results that have been hitherto achieved in sanitary science have been mainly due to the self-sacrificing labours of individual medical men. But the proverbial apathy of ruling powers to all matters not immediately affecting place and emolument has received no apter illustration than is afforded by the history of disease, and the tardy acknowledgment of the relation of state medicine and the welfare of nations.

If the 'Minute' of the General Board of Health be acted upon, if the medical officers of health of the different districts in the metropolis fulfil the expectations that have been raised, we may date from the passing of Sir. Benjamin Hall's act a new era for the medical profession; but that such a consummation may result, it is necessary that the members of the profession should be true to themselves and to one another. It is not for us to determine in how far the proceedings of the last few months may be conducive to their dignity or interests; but of one thing we are certain—that the due performance of the duties required of the medical officer of health in a large metropolitan district, is incompatible with the demands of private practice. This is well put in the 'Minute':

> "Where possible, it will be well to debar him" (the medical officer of health) "from the private practice of his profession: first, because the claims of such practice would be constantly adverse to those of his public appointment, the duties of which (especially at times of epidemic disease, when his official capacity would be most needed) private practice could scarcely fail to interrupt and embarrass; secondly, because the personal relations of private practice might render it difficult for him to fulfil with impartiality his frequent functions of complainant; and thirdly, because, with a view to the cordial good-will and co-operation of his medical brethren, it is of paramount importance that the officer of health should not be their rival in practice, and that his opportunities of admonitory intercourse with sick families should not even be liable to abuse for the purposes of professional competition."

The inherent difficulties attending the fulfilment of the duties of the medical officer of health will be sufficient to render any complication with professional jealousies not only undesirable, but fatal to the office.

The 'Minute' very properly hints, that if such and such requirements are necessary; if we are to see men distinguished from their fellows by talents and acquirements, and, withal, possessed of the tact and moral independence that characterize a high-toned gentleman, the salary ought to be sufficient to enable the holder of the office to dispense with the income derivable from private practice. None of the salaries hitherto given or promised show that the vestries have formed a proper estimate of the importance of the office; and we see much reason to regret that the apparent willingness of medical men of every denomination to take the appointment before the duties were defined or the salaries were fixed, should have tended to confirm the vestries in their opinion, that the market value of medical science must be low indeed when, under such circumstances, the competition is unlimited.

The individual member of a profession should never forget that he has duties to perform to his fraternity, almost as imperative as those to himself and his family; he should never dissever their interests in his mind, as they are indeed intimately interdependent. Other professions may possibly at times have carried this feeling too far—we have yet to arrive at that point of political development at which we regard our individual interests as bound up in the welfare of our common profession.

Such a view would find no advocacy with us did we not at the same time feel that we were advocating the real benefit of the community at large. But to obtain such men as the 'Minute' requires, or rather to retain them—for it is evident that some, if not all, districts will, in the first instance, have it in their power to appoint first-class men,—to retain such men, it will be necessary that the salary should be commensurate with the demands and usages of modern society.

The appointments will lead those who take them with the idea of prospective advantages, to sad disappointments. The office ought not to be an introduction to private practice; it cannot, with but solitary exceptions, lead to higher appointments; and of retiring pensions, even upon the paltry pittances now decreed, we hear nothing. Nay, the medical officer of health is entirely dependent for his tenure of office upon the caprice of the local vestries, a relation which is not likely to conduce to his freedom of action.

Still, we regard these appointments as a great step in modern civilization; they establish a most important principle—but that that principle be duly and permanently realized, it is right and proper that the men by whom this is to be done should be as unfettered as possible, and that they should receive that public recognition of their value which a vestry can express in no other form than by a large quarterly cheque. May we hope that an ample discussion of the question at issue may be ultimately as conducive to the interests of the profession as we believe that the hundred and thirty-second clause of Sir Benjamin Hall's act will, if effectively administered, prove to the metropolis, and, eventually, by example, to the entire kingdom.

REVIEW XI.

Medico-Chirurgical Transactions. Vol. XXXVIII. *London,* 1855. 8vo. pp. 352.

ALTHOUGH not so bulky as some of its predecessors, the present volume is second to none in point of intrinsic value. The following analysis will bear us out in the opinion that the Society which can contribute annually to the science of medicine and surgery such a body of pathological and therapeutic information as is herein presented, must ever maintain a most exalted position in the scientific world.

I. *The Fifth Series of Pathological Researches into the Diseases of the Ear.* By Joseph Toynbee, Esq., F.R.S.

The researches here recorded have been prosecuted by Mr. Toynbee during the last five years. Compared with the results of 915 dissections published in the thirty-second volume of the 'Transactions,' the author considers the later to have a higher value than the preceding, inasmuch as these made known only the morbid anatomy of the ear, while those open the field of pathology. This will be seen in the tabular statement of the morbid appearances found in 134 cases of diseased ears. Two or three passages must be quoted from this paper.

> "In a paper laid before the Royal Society, I have endeavoured to demonstrate that the faucial orifice of the tube is always shut, except during the momentary act of swallowing, when the tensor and levator palati muscles open the tube, and allow mucus to escape from the tympanic cavity, or air to pass in or out. It must be apparent that a thickened condition of the mucous membrane covering the guttural orifice of the tube is liable to cause obstruction, by preventing the muscles from separating the margins of the orifice; and obstruction from this cause does, I believe, not unfrequently take place; at least I am induced to form this opinion from the careful examination of cases, and from the post-mortem inspection of the guttural portion of the tube in many dissections conducted subsequent to my former paper."

Mr. Toynbee had previously expressed a conviction that deafness seldom depends upon obstruction of the Eustachian tube. This candid avowal of a modification of his opinions is deserving of attention. Mr. Toynbee's enlarged knowledge, derived from so accurate an observation of the post-mortem appearances in diseases of the ear, has induced him to think that

> "Some of the most common, and yet most important morbid conditions of the ear, had not previously been even suspected. Of these I will merely mention the presence of osseous and molluscous tumours in the external meatus, the various diseases of the dermoid layer of the membrana tympani, the ulceration of its fibrous laminæ, having among its singular results obliteration of the tympanic cavity; the existence of membranous bands connecting the ossicles to each other, to the membrana tympani, and to the walls of the tympanum; the various morbid states of the mucous membrane of the tympanum; and lastly, ankylosis of the stapes to the fenestra ovalis."

Mr. Toynbee further points out the confusion of cases that has prevailed under the names of otitis, otorrhœa, and caries of the petrous portion of the temporal bones. Tables are given of the results of 1523 dissections.

II. *Operation for Congenital Cataract on an Adult, followed by Division of the Recti Muscles, for the purpose of Controlling the Oscillation of the Globes.* By G. Critchett, Surgeon to the Royal London Ophthalmic Hospital.

The following is a short abstract of a history fully given by Mr. Critchett:—

Jane Smith, aged twenty-two, a tall well-formed young woman, with a pleasing, intelligent, and cheerful appearance and manner, was admitted into the Ophthalmic Hospital in the spring of 1849. There was slight internal strabismus of both eyes; considerable oscillation and involuntary rolling of the globes from side to side; the head was depressed, and the lids were usually kept nearly closed. The corneæ were bright and normal; the anterior chambers were rather large; the irides were bright, and of a deep brown colour, rather thin, and stretched towards the pupil, which was very small, rather irregular, and filled with a flat white substance, which had the appearance of being a dense piece of the capsule of the lens; the pupillary margin of both irides were firmly adherent to these capsules, and were, in consequence, quite unacted upon by the stimulus of light, or by belladonna. There was good perception of light, and under certain circumstances, when the light was very strong and thrown in a favourable way, a very bright colour could be distinguished from a dark one. At the ages of three, nine, and eleven years, she had been operated upon without benefit.

Having by the performance of several operations overcome the extreme firmness and toughness of the capsule, the strength of the adhesions between the capsule and iris, and the constant, irregular, oscillatory movements of the globes, Mr. Critchett considered that he had accomplished all that operative measures could secure. The poor girl had two fine dark hazel eyes, with small, clear, central pupils; and the globes were quiet, central, worked together, and under fair control. Still, vision was imperfect, owing probably to the absence of lenses, the partial development of the retina, and the entire want of education. With the aid of glasses she obtained a clear outline of objects, which appeared to her larger than her previous experience, derived through the sense of touch, had taught her. Her description of objects, their shape, colour, &c., proved that she saw them in their erect and natural position, and not inverted, as they appear upon the retina. She was very near-sighted, and everything seemed flat to her. Her greatest difficulty was with the human face. Within the last two years she has made great progress in the rapidity and certainty with which she can recognise objects. She can distinguish small objects, and find her way about; she has learnt her letters, but has not the opportunity of further education.

To this history the author appends a few practical remarks upon the condition of the eye itself, and the operations that were had recourse to in this case.

III. *Inguinal Tumour associated with Symptoms of Strangulated Hernia, and Absence of the Testicle on the affected Side.* By Henry Thompson, F.R.C.S., M.B.

The case here related forms, with its attendant commentary, a valuable contribution to the surgery of hernia.

IV. *On the Juvantia and Lædentia in Diabetes.* By John M. Camplin, F.L.S., &c.

Over and above its autobiographic interest, the chief practical point of this contribution is to be found in its commendation of bran-bread as an article of diet in cases of diabetes.

V. *On a Plastic Operation for the Restoration of the Lower Lip.* By Thomas T. Teale, Surgeon to the Leeds General Infirmary.

"Of the evils resulting from the contraction of scars, few," observes Mr. Teale, "are more serious, either in the distress or the disfigurement they occasion, than the eversion and dragging downwards of the lower lip following burns of the neck." For the relief of this deformity, Mr. Teale has adopted a mode of operating, which he describes in this paper. This operation consists in the formation of two lateral flaps from the everted lip and neighbouring portions of the cheeks, and in uniting them in the median line above the central portion of the base of the everted lip; or, in other words, in building up a new lip upon the base of the old one. The operation is rendered more intelligible by woodcuts. Three cases are related by Mr. Teale.

VI. *On Dislocation of the Femur upwards and forwards beneath the Crural Arch.* By W. Cadge, F.R.C.S.

Mr. Cadge has enjoyed the opportunity of a post-mortem examination of a case of this injury, in a case which was described by Mr. Travers, Jun., in the twentieth volume of the Society's 'Transactions.' The bones have been removed and preserved by Mr. Cadge. A full description of the changes found on dissection is given, together with a drawing of the new acetabulum, and its relation to the old acetabulum, the spinous processes, &c.

Among the practical remarks which accompany this pathological contribution, is the question of the period beyond which reduction in such a dislocation ought not to be attempted. This cannot be solved, observes the author, by the application of any rule based on the lapse of time only. The age and condition of the patient must in each case guide the decision. Sir Astley Cooper's limit of eight weeks, Mr. Cadge thinks too restricted.

VII. *On Bony Ankylosis between the Humerus and Scapula, after Disease.* By Holmes Coote, F.R.C.S.

Mr. Coote states that he has searched the Museum of the Royal College of Surgeons of England, and the collections attached to most of the metropolitan schools of medicine, in vain; and has been equally unsuccessful in his inquiries among his personal friends; not having been able to meet with another example of this termination of inflammatory morbid changes, besides the specimen he exhibited to the Society. The specimen in question came from the dissecting-room of St. Bartholomew's Hospital, having been taken from the body of a man aged thirty, who used to obtain his living by performing as a juggler and tumbler in the public streets. Mr. Coote thus describes this specimen:

"The humerus and scapula, both somewhat smaller than natural, are completely fused by ankylosis. The sawn surface shows that, but for a line indicating the junction of the epiphysis with the shaft, the general cancellous texture of the former would be continuous with that of the latter. As it is, no line of demarcation exists between the glenoid cavity and the altered head of the bone. A small cavity, capable of holding a pea, seen in the detached fragment of the specimen, was filled by a substance, thicker, more yellow and opaque, than the oily fluid permeating the rest of the bone; I believe this to have been the seat of a small abscess, which, having formed, ceased to extend, and the contents gradually acquired a consistence which has not uncommonly led to the mistake of their being considered tuberculous. It appears to me that, first, the head of the bone has been changed in form by ulceration; and that, secondly, the disease having come to an end, osseous union has taken place by fusion of the opposed granulating surfaces of humerus and scapula, and by a limited deposit of new bony matter."

The humerus was in this case dislocated upwards; the whole results, Mr. Coote supposes, were produced by rheumatic inflammation.

Mr. Coote refers to a nearly similar specimen in the Museum of St. Bartholomew's Hospital, and to two apparently parallel preparations in the catalogues of the Musée Dupuytren, and of the Richmond Hospital Museum, Dublin.

Mr. Coote draws the practical inference, that the possibility of bony ankylosis in cases of diseased shoulder-joint, is not sufficiently considered when the question of resection of the head of the humerus is raised.

VIII. *On the Relations of the Mortality from Cholera in London to the General Mortality and Temperature.* By Edward Smith, M.D., LL.B. Lond.

"This communication has been prepared with a view to prove that there are certain meteorological conditions related to cholera which have not hitherto been noticed; and which, although they do not suffice to account for the outbreak of that scourge, give information which may assist us in our investigation as to its nature and origin. It is limited to a consideration of the two epidemics in London of 1849 and 1854; and since the former has already received much attention a the hands of the most competent authorities, it will be alluded to here only as offering points of comparison and contrast with that of 1854."

Two charts or diagrams are given to show the above-mentioned relations of cholera. The accompanying observations by Dr. Smith are too condensed to admit of their being transferred to our pages. We may, however, give the following general conclusions which are found on Dr. Smith's chart, No. 1.

"At the *period of the year* when *cholera prevails:*

"1. *The temperature* has progressively increased for months, and has attained its acme, so that the ground, water, and air are hotter than at any other period of the year.

"As it declines, so does cholera.

"2. The *daily variation* is very changeable, but is the highest in the week when the cholera was the highest, both in 1849 and 1854.

"3. The *dryness of the air* has long been very considerable; and then, although great, is beginning to decline.

"4. *The rains* do not prevail, so that the ground is dry, the rivers low, the beds of stagnant waters exposed, the drains unflushed, and emitting foul air.

"5. *The winds* are light and south-westerly, so that the temperature is maintained, and exhalations not removed.

"6. *The electricity* is collected in the air.

"All these circumstances have a tendency to exhaust or to oppress animal and vegetable vitality. All have *greater influence in hot climates, and in low and enclosed situations*, where exhalations abound, and cannot be removed."

IX. *On a Successful Method of Treating Acute Rheumatism by large and frequent Doses of Bicarbonate of Potash.* By Alfred Baring Garrod, M.D.

Considering that Dr. Garrod is the author of a work on Materia Medica, we are not a little surprised at his announcement of a *novel* treatment of acute rheumatism by bicarbonate of potash. Dr. Wright, of Birmingham, in the year 1849 or 1850, published the result of his observation of the benefits derived from alkaline baths. Dr. Fuller, in 1852, published a treatise upon rheumatism, in which he still further enforced the efficacy of alkaline medicines, and showed the chemico-pathological principles upon which the treatment is based, as well as their influence in diminishing the tendency to fibrinous deposits from the blood. Dr. Garrod's paper does not contain any additional information beyond what the profession was already in possession of from the above-mentioned sources. Dr. Garrod states that his cases date from May, 1852; Dr. Fuller's first case is dated August, 1845. Dr. Garrod's conclusions do no more than confirm the statements made by Dr. Fuller, as to the *modus operandi* of alkalies, and of their effects upon the heart, and upon the various excretions.*

X. *Cases of Phlebitis, with Pneumonia and Pleurisy, from Chronic Disease of the Ear.* By W. W. Gull, M.D.

Three cases are here related, in neither of which—

"Had the cerebrum or cerebellum become the seat of disease; but the caries of the mastoid cells had set up local phlebitis, extending down the jugular vein, and producing an affection of the lung, which was in all the immediate cause of the fatal termination."

XI. *Notes on Lithotrity.* By Sir Benj. C. Brodie, Bart.

This paper has been noticed in this journal, January, 1856.

XII. *Observations respecting Degeneration of the Pancreas.*
By C. Handfield Jones, M.B. Cantab., F.R.S.

Thirty cases are here recorded, in which Dr. Handfield Jones carefully examined the pancreas; the results he has obtained constitute a pathological contribution of great value, the diseases of the pancreas being but obscurely known. In a tabular form, the author has given briefly the history of the illness in each case, and the post-mortem examination, including the state of the stomach, duodenum, and pancreas.

The changes observed in the pancreas have been microscopically examined.

Degeneration of such a kind as might be termed fatty, is the most striking morbid change met with by Dr. Jones:

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* It is right to observe that while Dr. Garrod especially recommends the employment of bicarbonate of potash in two-scruple doses, Dr. Fuller appears to give the preference to the neutral salts of soda.—Ed.

"The following details of the examination of case 17 in the table will convey a good idea of the condition of the gland in an advanced stage of this form of wasting (which is further illustrated by a lithograph). The glandular vesicles or ultimate cavities are entirely destroyed—no trace of limitary membrane is to be seen; the whole tissue is reduced to a coarsely-lobulated mass, which contains a large quantity of oily matter. The epithelium consists of mere shadowy traces of nuclei, with the smallest amount of faint, dim, amorphous matter, containing much oil in a finely divided state. The nuclei do not show a well-marked contour, like normal ones; nor have they the refracting contents, nor nucleoli. Sometimes, as stated by Dr. Salter, there are absolutely no nuclei or cells—nothing but amorphous and oily matter. A gland thus degenerated is of a dirty-yellowish colour, soft, lax, and flabby; and often exhibits some white spots upon its surface, which consist of groups of fat-cells. Similar groups are also present in the interior of the gland, but it is very clear that the wasting process is quite independent of their formation."

Dr. Jones describes earlier stages of this change, as well as another morbid condition, consisting in an excessive accumulation of the epithelial contents of the ultimate cavities.

Pancreatic degeneration, so far as the statistics adduced by Dr. Jones can supply data, appears rather to affect middle life. Sex does not seem to have much influence in its causation; neither is it shown that any special morbid condition is associated therewith beyond indications of impaired vital power. No correlation could be discovered between degeneration of the pancreas and disease of the stomach or duodenum. No symptoms, Dr. Jones states, give any intimation of the existence of even the most advanced pancreatic degeneration.

XIII. *Sequel of a Case of Extirpation of the Uterus.*
By John Windsor, Esq.

On the 22nd of June, 1819, a paper by Mr. Windsor was communicated to the Society by Sir A. Cooper, on the subject of inversion of the uterus, with a case of successful extirpation of that organ. It was published in the tenth volume of the 'Transactions.' Mr. Windsor now gives the sequel up to the period of the patient's death, on October 27th, 1854, she having survived the operation thirty-six years. Her death was occasioned by severe injury to the brain from an accident in travelling between Leeds and Bradford.

Mr. Windsor has no note of this case between November, 1819, when she was "in an excellent state of health," and September, 1840, when she was fifty-three years of age, and had married again. Having left Manchester, she subsequently fell under the care of Mr. Teale, of Leeds, who reports that he is informed that, in 1840, at Bradford, she had strangulated femoral hernia, which was allowed to pursue its course without any surgical treatment. In eight days, the hernia and its coverings sloughed; fæces were discharged from the wound; and in six weeks the abnormal anus spontaneously closed.

In December, 1850, January, 1851, and in 1853, she was operated upon for strangulated right femoral hernia.

The pelvic organs were removed by Mr. Teale, and transmitted to Mr. Windsor. The os uteri is apparently in its normal state: it is about half an inch in width; a probe passed through it into the blind or closed cavity beyond, does not penetrate more than three-eighths of an inch. This is all that remains of the truncated cervix. The communication with the abdomen seems to have closed well "by a membranous or fleshy surface, on which a portion apparently of one Fallopian tube with its fimbriated extremity can be traced, and near it is an appearance of an atrophied ovarium." The ovary and Fallopian tube of the opposite side terminated in a similar manner in connexion with the cervix uteri.

XIV. *Cases of Disease of the Larynx, and some Observations on the Operation of Tracheotomy.* By T. A. Barker, M.D.

These cases are related with a view to enforcing the practice of operating much earlier than is usual—that is, before the accession of those urgent symptoms which leave no alternative except the immediate performance of the operation and speedy death. They certainly bear out the conclusion, and therefore, if for this reason only, should be carefully perused. They do not admit of abridgment.

XV. *Researches on Gout.* By William Budd, M.D.

Two principal points are noticeable in this paper by Dr. Budd: one, the detection of urate of soda as an interstitial deposit in the cartilages of gouty patients. The form, size, and general appearance of these dots of deposit bear a certain resemblance to the minute nodules of oolite. The cartilage cell is the focus of each individual deposit—the original centre within and around which the crystallization occurs. The evidence of this is gathered from the form, size, and grouping of the deposits; from the effects of re-agents; and from the occurrence of groups of crystals in the very early stage in which the relation between the cell and the crystalline matter is plain to the eye. This relation is soon superseded by common physical influences, the crystals radiating from the cell to the whole central mass.

The second series of researches traces those changes which the morbid matter undergoes, and the detection of their products in the blood. In 11 cases, the analysis is stated to have been performed with the result of the detection of urea, and other crystallizable matters resembling oxalate of lime, benzoic acid, &c., and probably hippuric acid, in the blood. Our analysis of this interesting paper is necessarily brief—the original, in fact, scarcely admitting of condensation. We should add, that the appearances described are illustrated by lithographs.

XVI. *Account of a Growth of Cartilage in a Testicle and its Lymphatics, and in other Parts.* By James Paget, F.R.S.

This is a very extraordinary instance of the formation of cartilage in the parts above mentioned, as well as in the lungs, pulmonary artery, and vena cava inferior. The preparation of the testicle and other parts are to be seen in the museum of St. Bartholomew's Hospital. Mr. Paget has here given a minute description of these, accompanied with five lithographs, one of which is coloured. We should only mislead our readers were we to attempt an abridgment of this unique history; at the same time that we should do injustice to Mr. Paget's pathological comments, which deserve undivided attention.

XVII. *Cases illustrating the Pathology of Mania and Dementia.*
By Alex. John Sutherland, M.D.

The object of this paper is to show what light may be thrown upon the pathology of insanity by analysis of the mixed phosphates in the urine. The chemical analyses were made by Dr. Beale, of King's College. The following are the chief points in Dr. Sutherland's "conclusions." Phosphates exist in excess in the urine, in paroxysms of acute mania. They form a minus quantity in the stage of exhaustion and in the third stage of general paralysis. The quantities of phosphates in the urine correspond with the proportion of phosphorus in the brain. The plus quantity of phosphates in the urine denote the expenditure of nervous force, and is not a proof of the existence of acute inflammation.

XVIII. *Supplement to a Paper on the Use of the Speculum in the Diagnosis and Treatment of Uterine Diseases.* By Robert Lee, M.D., F.R.S.

The paper to which the present is referred as a supplement was published in the thirty-third volume of the Society's 'Transactions.' Two hundred and seventeen additional cases have been brought under Dr. Lee's notice during the short period of five years that have since elapsed!

A more painful and humiliating picture of uterine pathology could not have been drawn than is exhibited in this table of instances of erroneous diagnosis and practical maltreatment. The paper should have been entitled, 'On the *Mis*use of the Speculum;' a misuse, moreover, not ending simply in difference of opinion, or of treatment, but leading to demoralization, increase of physical suffering, and too often to a fatal termination. Surely the publication of such facts should cause practitioners to hesitate in the adoption of measures which result in such lamentable consequences.

PART SECOND.

Bibliographical Record.

ART. I.—*Cretins and Cretinism.* A Prize Thesis of the University of Edinburgh. By GEORGE S. BLACKIE, M.D., Curator of the Botanical Society of Edinburgh.—*Edinburgh,* 1855. pp. 70.

MANY of our readers are doubtless familiar with the noble efforts made by Dr. Guggenbühl to restore to the ranks of humanity the Cretin who, until recently, was considered a *lusus naturæ,* and an irreclaimable outcast from human society. In a letter to Lord Ashley, which Dr. Guggenbühl published several years ago, he showed that it was erroneous to suppose the Cretin to exist solely in the valleys of the Alps; he brought the subject home to us by demonstrating the existence of this fearful condition among ourselves, in localities where causes were found to operate similar to those prevailing in the Swiss valleys. Thus, Dr. Guggenbühl met with numerous cases of well-marked cretinism in Settle and Silserdale, in Lancashire. He found them even more numerous in the West of England,

> "Where similar predisposing causes exist as on the Continent. The village of Chiselborough, in Somersetshire," we are quoting from Dr. Guggenbühl, "lies in a narrow valley enclosed on three sides by high hills, and is very badly ventilated in consequence. The majority of the 350 inhabitants are affected with goître, hard hearing, indistinct articulation, and hebetude; in twenty-four of the inhabitants these characters are so developed as to constitute confirmed cretinism."

In the well-written monograph of which Dr. Blackie is the author, we find corroborative proof of Dr. Guggenbühl's observations in England, for he shows that—

> "In the highlands of Scotland and Wales, and in the mountainous parts of the south of Scotland, in such enclosed valleys, we almost invariably find idiots; in most of our valleys the other conditions mentioned as causing cretinism do not exist, such as lime formations, snow water, indolence, and excessive heat; but we have the shut-up valley, the drunkenness, the bad nourishment, the intermarriage of relations, just as in the Swiss and German mountains. And what are these deformed idiots? To all intents and purposes they are cretins, which, according to the definition given above, are beings possessed of deformed bodies and fatuous minds, incapable of performing any mental, and more than a very limited proportion of physical offices, if any."

While it is melancholy to find that extended investigation lays bare greater social and physical vices, it is satisfactory to know that increased exertions are made in London, in Edinburgh, and elsewhere, to carry out those curative indications which Dr. Guggenbühl has been the first to point out.

Dr. Blackie's essay deserves the special attention of all interested in the study of the human mind, and its relation to physical training and development. It forms an excellent summary of what is known on the subject of cretinism, and is replete with wise and suggestive remarks.

ART. II.—*Surgical and Pathological Observations.* By EDWIN CANTON, F.R.C.S., Assistant-Surgeon to Charing Cross Hospital; Lecturer on Surgical Anatomy, &c.—*London*, 1855. 8vo, pp. 106.

THE materials of which this volume is composed have already appeared, at various periods, as contributions to two of the weekly medical journals; but the author states that he has endeavoured to amplify, amend, and illustrate his observations, in thus submitting them to the profession in a collective form. The subjects treated of are—chronic rheumatic arthritis; shortening of the leg from bruise of the hip; the employment of purgative medicine after the operation for hernia; the occurrence of cysticercus cellulosæ in the sub-conjunctival areolar tissue, and within the human eyeball; congenital deficiency of the gall bladder; dislocations of the astragalus; a rare form of aneurism.

The subject of chronic rheumatic arthritis is considered especially in relation to its morbid anatomy. This singular and intractable malady has for some years past engaged the attention of medical inquirers.

> "It invades the small and large articulations: equally may it affect those of the fingers and toes, or of the knee and hip, inducing in them the most unsightly deformity, and permanently impairing their functions, so that the power of prehension is lessened or lost, and locomotion is perverted or prevented. The joints of the lower jaw may experience an attack, when discordance of speech ensues and mastication of food becomes difficult. The spinal column may suffer, and the body be irremediably contorted, whilst internal organs are thereby secondarily, and often seriously, affected. Exostoses (osteophytes) very characteristically spring forth from near the joint-surfaces of the bones; cartilaginous productions may lie, in large numbers, freely in the articulation, or project pedunculated within it, associated with groups of vascular, synovial fimbriæ; the encrusting cartilages become absorbed, and the exposed bone is hardened and eburnated; the fibrous structures around are in part removed, and in part encroached upon by ossific deposits, which must in their progress of formation press injuriously upon the nervous filaments in their vicinity, and thus superadd continued irritation as they are producing insuperable disfigurement and impeding freedom of motion. The muscles in the neighbourhood become powerless, as atrophy and degeneration implicate them." (p. 2.)

The symptoms are fully described. Under the head of Morbid Changes the author observes:

> "Shortening of the cervix femoris is very characteristic of chronic rheumatic arthritis, and especially so is the horizontal position it assumes, or the still greater depression it suffers, whereby a direction shall be given exactly the reverse of its natural one. The lower limb is shortened in proportion as the one or other of these conditions obtains and the foot becomes more or less everted." (p. 7.)

This memoir is illustrated by several well-executed wood engravings. In the following paper the author points attention to the fact, that interstitial absorption may supervene upon bruise of the hip. He is opposed to the employment of purgative medicines after the operation for hernia; maintaining, that if adequate time be allowed, such medicines withheld, and a soothing plan adopted, nature will perfect the cure. On this vexed point of surgical practice we conceive the judicious practitioner will always be guided by circumstances, and act accordingly. In reference to the occurrence of cysticercus within the eye, of which six cases are on record, the author concurs with Dr. Mackenzie as to the propriety of early removal of the hydatid, in order that no permanent mischief may be produced by its presence.

In treating of dislocations of the astragalus, the author gives minute details of the dissection of an unusual form of luxation of that bone—namely, that of the body partially inwards with the tibia, the astragalo-scaphoid articulation remaining perfect. The rare form of aneurism which he describes was situate in the substance of the sartorius muscle, and occasioned considerable difficulty with respect to diagnosis. He is of opinion that this peculiar form of aneurismal disease is most likely to be produced, as in the instance detailed, in situations where a muscle, in overhanging a large artery, receives a branch directly from it.

In concluding this brief notice, we would simply remark, that the observations contained in this volume are on the whole creditable to its author.

ART. III.—*Om Dodeligheden i Norge, Bidrag til Kundskab om Folkets Kaar.* Af EILERT SUNDT, Cand. Theol.—*Christiana,* 1855.
On the Mortality in Norway; a Contribution to the Knowledge of the Condition of its People. By EILERT SUNDT, Candidate in Theology. With numerous Tables.—8vo, pp. 206.

ISSUING from the pen of a clergyman, the volume before us naturally has a clerical character. The introductory and closing portions of Herr Sundt's volume are written in a tone of serious piety well suited to the gravity of the important subject of which it treats, embracing the consideration of the uncertainty of human life in individual cases, and of the great change which, sooner or later, awaits us all. The bulk of the work is necessarily statistical, and as the field of observation, the population of Norway, is not very extensive, we shall quote but briefly from the author's pages, and shall chiefly point out the portions in which the results obtained are compared with those derived from similar investigations in other countries, as being the parts of the book most likely to interest the English reader.

From a Danish table published in 1845, to which the author appends the numbers for Norway, the value of life would appear to be high in the latter country. Thus the mortality is quoted as being, in certain years, for Iceland, 1 in 30; Saxony, 1 in 32; Európean Russia, 1 in 32; the Austrian Empire, 1 in 33; Sardinia and Prussia, each, 1 in 35; Bavaria, 1 in 36; the Netherlands, 1 in 39; Belgium, 1 in 40; France, 1 in 42; Hanover, 1 in 43; Sweden, 1 in 44; Great Britain and Ireland (1838—42), 1 in 45; in Denmark, 1 in 47 (including stillborn children);

in the Danish Duchies, 1 in 49 (exclusive of the stillborn); in Norway (1826—45), 1 in 51·9 (exclusive of the stillborn), and including the stillborn, for the same period, 1 in 49·05. The author obtains the mortality by dividing the mean of the number of the population, at the commencement and close of each decennial period, by the annual average of the number of deaths which have occurred during the ten years.

In a subsequent chapter the author compares the values of the mean term of life at various ages for each of the only four countries in reference to which, he states, the materials for such a comparison exist—namely, England and Wales, Denmark, Sweden (with Finland), and Norway; four countries of Northern Europe which, for the most part, are inhabited by two great divisions of the Germanic race—the Anglo-Saxons and the Scandinavians. Here, again, Norway has the advantage at all ages; Denmark has the advantage over England up to thirty years of age for the male sex, and up to fifty for the female: after these ages the value of life would appear to be greater in England than in Denmark. The numbers for Sweden are not introduced into the comparative table, as they are derived from a more distant period (1801—5). A comparison of the results obtained for the whole population of Norway with those collected in the country districts of Denmark and in the Surrey district in England, proves that the advantage possessed by Norway is not solely owing to the fact that its rural population bears a larger proportion to that of the towns than is the case in Denmark, and still more in England. From a comparison of three decennial periods, into which the author has divided the years 1821 to 1850, it appears, contrary to what might have been expected, that the mortality in the second of these periods was considerably greater than that in the first, and that the result in the third was not quite so favourable as in the first. The author discusses at considerable length the probable causes of this circumstance, but does not seem to have arrived at any very satisfactory solution of the fact.

The next four sections are devoted to a review of the comparative mortality in the several dioceses and deaneries of Norway; but as this subject can possess little more than a local interest, we shall not enter upon it.

The number of violent and accidental deaths—as by lightning, snow slips, the capsizing of boats, exposure to the fumes of charcoal, &c.—is very large, amounting, on an average of ten years, to 945·8 out of 24,065·4, the average annual mortality, and is in fact much higher than in other countries. Thus, the author states that the yearly number of accidental deaths is, for each million of the population, in France (1846), 212; in Denmark (1845—49), 393; in Sweden (1841—50), 625; in England (1840), 627; and in Norway (1841—50), 712. It would hence appear, that while the total mortality is less in Norway than in the other countries here mentioned, violent or accidental deaths are much more frequent. That this is owing to the natural circumstances of the country is shown by the fact that, during the ten years 1836—1845, of 901, the yearly average of violent or accidental deaths, 682 were caused by drowning; and that in the diocese of Christiana, which is mostly inland, the average annual number of accidental deaths was only 45 for 100,000 of the population; while in Tromso, which is almost exclusively a mountainous sea coast, the proportion was 224 in 100,000. The author makes this fearful proportion of violent deaths still more apparent by some striking illustrations.

The author gives the following as the yearly number of suicides for 100,000 of the population in each of the countries mentioned:—Ireland (1831—41), 1; Lombardy, 1·7; England (1840), 5·7; Sweden, 6·7; France (1835—43), 7·9; Prussia, 10·2; Norway, 10·8; Denmark, 23·1.

Before concluding this brief notice, it may be well to mention the sources of information open, in Norway, to the author in his investigations: these were the lists of mortality which the clergymen have been, since the year 1735, obliged to send in yearly to the dean or bishop, and which the latter in their turn must collate, each for his own deanery or bishopric, according to a scheme which, especially for the last thirty years or more, has been very complete; and the equally perfect decennial censuses which have been taken during the same thirty years. Of these materials Herr Sundt has well availed himself; his book is elaborately and carefully compiled, and will be found by those who are acquainted with the Norwegian language, and may be interested in the investigation of the condition of the people, to contain a large amount of accurate information, conveyed in a style which is both appropriate to the subject, and is suggestive of serious reflection.

ART. IV.—*Diseases of the Rectum.* By RICHARD QUAIN, F.R.S., Professor of Clinical Surgery in University College Hospital. Second Edition.—*London,* 1855. pp. 332.

THIS treatise is founded upon a series of clinical lectures upon the diseases of the rectum. It will not be saying too much of it, that it presents, what we should regard as, a model of excellence in clinical lectures. A few cases are concisely and graphically related, illustrating the several forms of disease, and presenting distinct pictures thereof. These are followed by clear, apt, and condensed comments, setting forth the structure of the diseased parts, and the best practice in each instance. The practical directions are not given merely at second-hand, as expressing routine practice; they lay down several very important deviations from, and improvements upon, the ordinary modes of proceeding. More particularly are these features observable with regard to the treatment of painful ulcers and spasm of the rectum. The volume constitutes a highly valuable guide either for student or practitioner.

ART. V.—*Headaches; their Causes and their Cure.* By HENRY G. WRIGHT, M.D., M.R.C.S.L., Fellow of the Royal Medico-Chirurgical Society, Physician to the St. Pancras Royal Dispensary.—*London,* 1856. pp. 152.

THE careful examination of an important and prevailing affection, even though in itself necessarily symptomatic of deeper-seated derangements, cannot fail to prove serviceable to science, and thus beneficial to the sufferer. It is very proper that we should investigate and try to ascertain the exact meaning of pain, whether affecting one organ or another, just as we seek to read in the abnormal conditions of a secretion, the indications of the derangements affecting the viscus from which it is derived. But in a monograph supposed to be written for the professional reader, it is surely right to expect that the author should seek either to establish new points, or to place well-known facts in a new light, and that regard should be had for what others have previously done in the same field. Throughout Dr. Wright's book, we find scarcely a reference to writers who have turned their special attention to the subject, or to such more comprehensive works, as those of Abercrombie, Graves, Romberg, or Todd, in which the student may learn the variety of bearings under which this symptom manifests itself. We have looked in vain for any new illustration in the domain of pathology or therapeutics; and regret to observe, that the book bears all the characters of being addressed rather to the patient than to the professional reader.

We should not be induced to pass our strictures upon the book in question, were it not that, from indications scattered here and there, we incline to think that Dr. Wright is capable of better things; and that if he will avoid the *ad captandum* style, he may produce a contribution to medical literature, to which we may be enabled to point in terms more agreeable to ourselves and more flattering to the author.

ART. VI.—*Statistics of Grave-Yards in Scotland.* By JOHN WEBSTER, M.D., F.R.S., F.R.C.P., Physician to the Scottish Hospital.

WE read of Old Mortality clearing the moss from the monuments of the Covenanters with his chisel, "trimming, as it were, the beacon-light which was to warn future generations to defend their religion even unto blood." Such are not the objects which now lead the philosopher into the grave-yards; he seeks life in death, and desires to pick out of the tombstones a lesson of humanity and physiology, which may teach present and future generations how to prolong their sojourn in this world. In this sense, Dr. Webster has been gleaning some curious and interesting information from the grave-yards of Edinburgh, Greenock, Stirling, Perth, St. Andrew's, Dundee, Arbroath, Forfar, Brechin, Montrose, Aberdeen, Tealing, Inverarity, Aberlemno, St. Vigians, Lochlee, and Edzell. Among the curiosities adverted to by Dr. Webster, are the instances of longevity which he has found recorded. Edinburgh is not remarkable for the high age attained by its inhabitants; yet the Canongate bellman, William Edie, who died in 1731, attained his 120th year. No such instance is to be found in Glasgow, where 44·50 per cent. of the population die before they are five years old, and 84 in every 100 persons born never pass their sixtieth year. St. Andrew's, again, is distinguished by its salubrity, and boasts also of having had an inhabitant who attained the age of 185.

With regard to the occurrence of longevity in towns and country districts, Dr. Webster makes the following practical remarks:

"According to the statements previously detailed, it appears that old people were more numerous in town districts than most persons would have supposed.

> Unquestionably, many of the oldest persons recorded on gravestones died in populous towns; but a large proportion of these individuals had migrated from the country in early or in middle life, some towards the evening of their days. Glasgow is an instructive example of this; since, in this city, as I was informed on very good authority, several patriarchal individuals, buried in its cemeteries, were strangers. That towns are inimical to infantile life, there cannot exist any question; seeing that the records of all densely populated localities prove the correctness of such an opinion. Nevertheless, many towns possess advantages as residences for old people. There is more warmth in houses so situated. The congregation of so much animal life exerts a beneficial influence upon weakened and decaying constitutions. Association with younger fellow-creatures proves often advantageous to older physical frames. Besides this, aged persons, placed under the above circumstances, are more likely to receive kindness and attention from friends, and will be less likely to experience neglect, than in out-of-the-way rural places; while the benevolent hand of charity is more freely extended. But, whatever may be the influences, very old people generally live and die in towns." (pp. 18, 19.)

The bearing of hygienic conditions upon the duration of life, receives some apt illustrations in this visit to the Scotch graveyards, as in the case of Glasgow and Aberdeen.

> "At Glasgow, old people are comparatively rare: in Aberdeen the contrary obtains; yet both are large manufacturing towns, situated on navigable rivers, and having each much shipping. Both are near the sea; and in many particulars they bear a marked resemblance. Aberdeen, however, lies on the east coast of Scotland; Glasgow near the western. Aberdeen possesses a dry granitic foundation, sloping towards the adjacent river; is abundantly supplied with excellent water; has a hardy, not mongrel, race of inhabitants; streets wide, well ventilated, and straight; an atmosphere little deteriorated by thick smoke or chemical impurities; weather which, though cold and often stormy in winter, is often good at particular seasons; and lastly, but not leastly, a degree of longevity which even appears to be hereditary. Here, therefore, varied circumstances exert considerable influence on public health and human existence, tending to support the physical powers, the *vis vitæ*, and to ensure longevity. In Glasgow, on the other hand, we have exemplified the simple reverse of the above description, thus affording, *pro tanto*, an explanation of the contrast I have noticed." (p. 19.)

We have arrived at a great epoch for London graveyards; they will soon belong to the things that have passed away. They have been the text of many a sermon, and of not a few lectures and addresses, and yet we conceive that there is still much unread on their stone tables; much that would repay the student, with reference to the habits and customs of past generations; much that, if examined with the spirit that has prompted Dr. Webster, might become profitable to present and future denizens of the metropolis.

Art. VII.—*Medical Notes and Reflections.* By Sir Henry Holland, Bart., M.D., F.R.S., &c., &c., Fellow of the Royal College of Physicians, Physician in Ordinary to the Queen, and Physician in Ordinary to His Royal Highness Prince Albert. Third Edition.—*London.* pp. 638.

We have a particular pleasure in again recommending to our professional brethren the work of which a third edition, more strongly than we are able to urge, proves the hold which Sir Henry Holland has already acquired among his contemporaries. The constant references to be met with to his views in the works of esteemed authors, is the best testimony to their value. The 'Medical Notes and Reflections' have already taken their place among the standard works on medical subjects. We venture to prophesy that, as a comprehensive survey of the present and most enlightened views on a large range of medical topics, and as a model of elegant and convincing medical writing, the work will enjoy a reputation which will survive more than the generation to which it was first addressed.

ART. VIII.—*Hooper's Physician's Vade-Mecum; or, a Manual of the Principles and Practice of Physic.* Fifth Edition, considerably enlarged and improved; with an Outline of General Pathology and Therapeutics. By WILLIAM AUGUSTUS GUY, M.B. Cantab., Fellow of the College of Physicians; Professor of Forensic Medicine, King's College, London; Physician to King's College Hospital, &c., &c.—*London*, 1856. pp. 676.

HOOPER'S 'Physician's Vade-Mecum' is gradually undergoing so complete "a metamorphosis of tissue," that one or two more editions will leave scarce a vestige of the original. The editor thus undoubtedly acts up to the spirit of the author, who, in leaving his legacy, would certainly have desired no better immortality than that which appears secured to him by the constant alterations and additions rendered imperative by the demands of our advances in science.

It is our pleasant duty to call the attention especially of the student and the junior practitioner to this valuable manual in its present enlarged form, and to wish it that continued success which the labour bestowed upon its revision fully merits.

ART. IX.—*A Dictionary of Practical Medicine.* Part XVII.; being Part VIII. of Vol. III. By JAMES COPLAND, M.D., F.R.S., Fellow of the Royal College of Physicians; Honorary Member of the Royal Academy of Sciences of Sweden, of the American Philosophical Society, and of the Royal Academy of Medicine of Belgium; lately President of the Medical and Chirurgical Society of London; formerly Consulting Physician to Queen Charlotte's Lying-in Hospital; and Senior Physician to the South London Dispensary; Consulting, and lately Senior, Physician to the Royal Infirmary for Diseases of Children, &c.

IT is now exactly twelve years since one of our predecessors* gave that award of praise which was most justly due to Dr. Copland, on the completion of the first two volumes of his Dictionary. We received the seventeenth Part at the close of the past year; and as it concludes with Tubercular Consumption, we may now look forward to the possibility of the work being entirely achieved.

While we would wish to congratulate Dr. Copland on the approach of the termination of a task which has associated his name with the most eminent cultivators of science of the present age, we cannot but give expression to a feeling of regret, that the termination should be so long delayed. All serial publications labour under the disadvantage of occasional unavoidable delays; but in most instances the material is somewhat prepared before the publication is commenced. We cannot doubt that this is the case with the 'Dictionary of Practical Medicine,' since we are informed by Dr. Copland himself, that he presents the results of labours accumulated since 1814. In justice to the present generation of students, and to early subscribers, of whom some are as fortunate as we ourselves, yet to survive; in justice to Dr. Copland himself, we trust that the accomplishment of the entire design may soon stand upon record.

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* The British and Foreign Medical Review, April, 1844, p. 503.

ART. X.—1. *On Personal and Domestic Hygiène; showing the Value of Sanitary Laws: addressed especially to the Working Classes.* By LIONEL J. BEALE, Surgeon, &c.—*London*, 1855. pp. 23.

2. *Common Sense versus Homœopathy.* By SAMUEL KNAGGS, Member of the Royal College of Surgeons, London, &c.—*London*, 1855. pp. 48.

IF there be a means of circulating such pamphlets as the two of which the titles stand at the head of this notice, and the general public to whom they are addressed can be induced to read them, we most willingly lend our aid by expressing a hearty approval of their contents and style. Mr. Knaggs has put the questions at issue regarding homœopathy in a proper light, and handles his subject in a manner which we think a non-medical person, of ordinary education, must be able to follow and understand. Still, so long as the public, and more particularly the educated classes, are blindly ignorant of natural laws, they will regard all arguments and facts employed by the regular medical practitioner as *ex parte* statements. The only way by which their blind faith can be shaken is by familiarizing them with the laws of physiology. It is

> "More light and fuller that they want."

In this sense, we are glad to promote the diffusion of books and pamphlets written like the one of which Mr. Beale is the author.

ART. XI.—*The Journal of Psychological Medicine and Mental Pathology.* Edited by FORBES WINSLOW, M.D., D.C.L. New Series. No. 1.—January, 1856.

AMONG the various departments of medical science, there is scarcely one of greater importance and more general interest, whether we regard it in a purely scientific point of view or in its social relations—domestic, political, or religious,—than that bearing upon the healthy and morbid state of the mind. Though the practice in the domain of mental pathology is necessarily a specialty, no enlightened physician can fail daily to be arrested by the observation of the intimate reciprocal influences of body and mind. To all medical men who take an enlarged view of their calling, the study of mental derangement must be one of surpassing interest; and to those whose duties do not permit extensive reading on the subject, we can very cordially recommend Dr. Winslow's journal. The first number of the new series contains articles of great value, to which we only regret not to be able to devote more than this cursory notice.

Art. XII.—1. *A Catechism of Chemical Philosophy; being a familiar Exposition of the Principles of Chemistry and Physics, in their application to the Arts and Comforts of Life.* Illustrated by One Hundred and Fifty Woodcuts. By John Horsley.—*London*, 1856. pp. 247.

2. *The First Step in Chemistry.* A new method for teaching the Elements of the Science. By Robert Galloway, F.C.S. Second Edition, with Illustrations on Wood.—*London*, 1856.

Both these elementary treatises may be recommended to the teacher and learner of chemistry. The first is arranged in the form of questions and answers, and very copiously illustrated; on which account, as well as on account of the simple explanations and systematic arrangement, it is likely to be a favourite. One illustration is particularly useful—viz., a coloured plan of the reactions produced by the ordinary tests of metals.

Mr. Galloway's volume, though less extensively illustrated, aims at a more scientific character, but offers a feature which must be of great use to the pupil or the tutor, in the shape of series of exercises, which are to be found at the end of each chapter.

Art. XIII.—*The Micrographic Dictionary; being a Guide to the Examination of Microscopic Objects.* Parts XVI. and XVII. By J. W. Griffith, M.D., F.L.S., and Arthur Henfrey, F.R.S., F.L.S.—*London*, 1856.

We have only to announce the completion of this work, on the excellence of the preceding numbers of which we have already expressed ourselves in terms of unqualified praise. It is in all respects such as to serve the purpose for which it is intended by its distinguished authors.

Art. XIV.—*Trees and their Nature; or, the Bud and its Attributes.* By Alexander Harvey, M.D., M.A., &c., &c.—*London*, 1856. pp. 236.

The purpose of this agreeable little book is the discussion of a favourite theory. The author disclaims all pretensions to originality as regards the theory itself; there is, however, much that is new in the illustration. The indefinite longevity of many forest trees had seemed to him at a former period an exception to the general law of the determinate duration of all living organisms. In the endeavour to reconcile this apparent incongruity, the author had arrived at the conclusion which is in accordance with the teaching of every physiologist of the present day, that the tree is not to be considered as a "single or individual plant," but rather as "a congregation of individual plants of the same species."

The letters, of which the book consists, are addressed to his children, to whom it seems rather his object to communicate his own love of nature, than to give direct instruction. As a book for the young we cordially recommend it. It is likely to foster the habit of deriving pleasure from the observation of natural objects, and is moreover pervaded by a wholesome spirit of piety.

PART THIRD.

Original Communications.

ART. I.

On the Curative Processes in Chronic Pulmonary Tubercle, and the Local Conditions which Promote or Oppose them. By C. RADCLYFFE HALL, M.D., F.R.C.P.E., Physician to the Hospital for Consumption, and to the Institution for Ladies with Diseases of the Chest, Torquay.

(Concluded from No. 32, p. 485.)

PUTTING aside for the moment the constitutional part of the disease, there are several modes by which the local deposits in chronic phthisis may pursue a course not inconsistent with the life of the patient. The local cure of a pulmonary tubercle takes place—1, when the tubercle, being latent, permanently continues so, and does not progress; 2, when the tubercle undergoes without ulceration such transformation as renders it no longer a source of irritation; 3, when the tubercle, having led to ulceration, is ejected, and the ulcer afterwards heals in some way.

The cure without ulceration constitutes arrest: which thus includes both the case in which latent tubercle remains stationary—simple arrest; and that in which, after undergoing progressive changes, the tubercle attains a safe termination—arrest by transformation of the tubercles.

SIMPLE ARREST.

Latent Tubercles.—The mere presence of abnormal deposits in the lungs, even to a very large extent, provided irritative inflammation be not set up, interferes surprisingly little with the general signs of health. It is quite compatible with ease and comfort in breathing, unless perhaps under extraordinary exertion, with a good amount of muscular vigour, and with a fair performance of general nutrition.

A mason, aged twenty-seven, was killed by the falling of a wall. On examination, the apices of both lungs were found stuffed with grey miliary tubercles. There was no inflammation around them. The man had been considered in good health, and free from cough or complaint.

It is, however, easier to prove the point by comparative pathology. For fifteen years past I have been in the habit of noticing the lungs in butchers' shops and at slaughter-houses. I have never seen a single specimen of the lung of a full grown sheep that was entirely free from entozoic disease. The disease is not hereditary, since the lungs in young lambs are healthy. Nor, I conclude, is it restricted to any specific locality, since I have found it at every place in Great Britain, France, Germany, and Switzerland that I have happened to visit.

The lungs, then, of any full-grown sheep taken indiscriminately will be found to contain, and often to be thickly studded with, small nodules varying in size from a pin-head to a barleycorn, or larger. The nodules are either small cysts, or firm soft deposits, or grit-like bodies. The cysts are filled with clear fluid, and contain cysticerci hanging upon an epithelial lining-membrane. The firm soft deposits consist of granule-cells and molecular matter, in which minute ascaris-like worms are found.[*] The gritty nodule is one or other of these which has undergone calcareous transformation. The particular point bearing upon my subject is, that this pulmonic affection does not prevent the sheep from furnishing excellent mutton. The condition of the animal for the market is often the finest possible. Not only has general nutrition been unimpaired, but there is reason for thinking that the animal may have been at first more disposed to fatten after the invasion of the entozoa than before. As sheep are rarely free from flukes in the biliary passages, it is difficult to decide how much influence is due to the presence of the hepatic, and how much to that of the pulmonary, entozoa. But we shall not greatly err in concluding that when both liver and lungs have their functions partially obstructed, so long as irritative disease is not set up, nor anæmia induced, the surplus hydro-carbon is excreted vicariously into the reservoir-cells of the adipose tissue, causing the animal, in the absence of brisk exercise, rapidly to grow fat. As soon, however, as the blood has become too much weakened by loss of albumen, emaciation and dropsy take the place of fatness.[†]

We do not of course infer that the presence of tubercles in the lungs would interfere with health as little as that of these entozoa ordinarily does. Tubercles imply constitutional disease. The pulmonary entozoa do not. Still, the analogy may be used to support the conclusion derived from other considerations, that the mere presence of tubercles in the lungs, so long as they are unattended by inflammation, is compatible with the appearance of health, and with a fair share of the reality also.

Could we manage to keep existing tubercules quiescent, we should fulfil the first condition of safety.

But there is probably only one form of tubercle that can remain permanently unaltered and quiescent, viz., Bayle's granulations, or the small semi-transparent tubercles. When none other than these exist, it is impossible to decide with certainty, so long as they remain unchanged, that the patient is tuberculous at all; and equally impossible, on discovering such tubercles after death, to say for how long a period they may have existed in this latent form. Such unmixed cases are rare, but from their occasional occurrence we infer that some of the small firm semi-transparent tubercles commonly found in an ordinary case of phthisis, *may be* of considerable age. It is indeed entirely unknown for how long a time before the outbreak of developed consumption, the lungs may have been the seat of latent tubercles.

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[*] Is there any relationship between these minute worms and the larval condition of the flukes? The ova of flukes might readily find their way into the lungs.

[†] A celebrated agriculturalist, Mr. Bakewell, when he desired to fatten his sheep, quickly, was in the habit of sending them to pasture in the neighbourhood of a certain pool which was infested with the ova of flukes. The sheep fattened greatly for a time, but eventually, if not sold, were as certain to fall off in condition.

Primary yellow tubercle in all probability is never latent, being never free from attendant inflammation. Bayle's granulations are probably latent, as the rule. Semi-transparent grey miliary tubercles are probably latent at first, manifesting their presence gradually as they progress.

We may assume that the elements of a slowly progressing tubercle have a certain term of existence, at the end of which they infallibly undergo degeneration. The change may take place so slowly and quietly, and may occasion so small an amount of irritation in the surrounding pulmonary texture, as not to prove destructive in its issue. The patient's death in phthisis is not caused by the degeneration of the tubercle simply, but by the destructive influence of the degenerating tubercles upon the adjoining texture. The very same mode of degenerating, therefore, which in the more common case leads to death, may in another case, wherein it does not induce much local irritation, pursue its course without any considerable damage to the economy.

That tubercle shall degenerate slowly and gently when its time comes, is another main condition of safety.

ARREST BY TRANSFORMATION.

When a tubercle has undergone a curative transformation, some one or several of the following changes are found to have taken place in the tubercle and lung immediately around it:—Liquefaction; absorption; fatty degeneration; granular degeneration; calcareous degeneration; pigmental degeneration; shrivelling; sequestration.

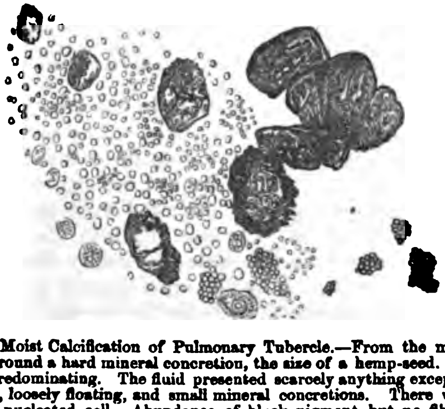

Fig. 23. Moist Calcification of Pulmonary Tubercle.—From the mortar-like fluid around a hard mineral concretion, the size of a hemp-seed. Grey tubercle predominating. The fluid presented scarcely anything except free oil-vesicles, loosely floating, and small mineral concretions. There was an occasional nucleated cell. Abundance of black pigment, but no fibrinous capsule, existed around the site of the concretion. All the calcareous matter was amorphous: no crystals, either of lime or of cholesterine.

The most common instance of safe transformation includes several of these alterations. We have the disintegration of the original corpuscular elements of the tubercle; the partial removal of them by absorption; the substitution in their stead of oil, cholesterine, and calcareous matter; and, around the whole a quantity of black matter in condensed lung-tissue, or, a distinct fibrinous capsule. Reduced to such a *caput mortuum*, a tubercle is passive, no more offensive than an encysted pellet, and may remain without further change indefinitely.

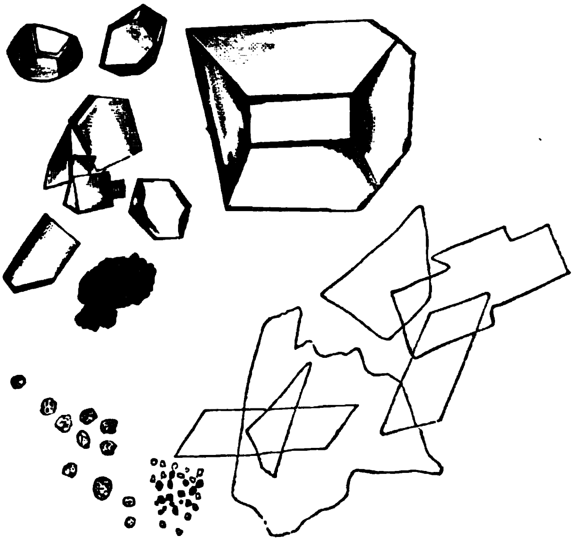

Fig. 24. Moist Calcification of Pulmonary Tubercle. — From concrete mortar-like dots upon the lining membrane of a large closed cavity. In each dot, when crushed, and floated under the microscope, were presented—shrivelled pus-cells, free fat in abundance, no tubercle-cells, plates of cholesterine, calcareous matter in amorphous heaps and in crystals.

For tubercle, the natural cure consists in its mineralization. And in this there is nothing peculiar to tubercle. Any normal texture which becomes too feeble in active power to keep itself fitted for the discharge of its function, provided its course be not interfered with by inflammation, tends to become less and less endowed with animal life, and sinks gradually through a stage of vegetable life into the mineral condition. And to this slow descent in the scale of vitality the adjoining texture accommodates itself, becoming by the same degrees as fitted for harmless contact with the mineral matter as is the living texture of the bones or teeth. The whole indeed is typified in the conversion of bone-cartilage into healthy bone. We see other examples in the general hardening of the soft tissues in old age; in the senile ossification of the costal and laryngeal cartilages; in calcification of the valves of the heart and coats of the arteries. So far, such changes are only the normal consequences of age; the slow instalments by which Death asserts his right. But it is by changes precisely similar that any morbid product, if not interfered with by inflammation, will slowly and safely die. Any considerable exudation of common lymph is usually the result of inflammation, and is too much accompanied by it to furnish an illustration in point. It liquefies and is absorbed, or suppurates and is discharged, or dies rapidly; processes all too quick for safe degeneration. But in the oldest coatings of an old aneurism we may find oil-molecules and cretaceous particles. And the same in old enlarged partially calcified lymphatic glands in non-tuberculous subjects; and in some chronic non-malignant tumours of the uterus. We may therefore conclude that simple lymph, if left alone, provided it could neither become properly organized nor altogether absorbed, would ultimately pass on towards the mineral state. Nor is there any reason to doubt that any variety of malignant formation (cancer), in which the fast life and rapid death which confer upon it its dangerous character, could be exchanged for a very slow mode of dying, would likewise pass through a fatty into an earthy condition. Portions of slow cancer are sometimes met with in which such alterations can be demonstrated.

The natural cure of any disease, consisting in the restoration of disordered actions to their normal course, must ever follow some plan of normal working. The physiological course of dying pursued by any portion of the body which dies without compromising the rest, is the same as the pathological course by which alone a morbid formation can die with safety to the economy. Cure of tubercle is death of tubercle so gradually and quietly brought about as not to entail death of the entire body. A cretified tubercle is a dead tubercle. A softened tubercle leading to ulceration is also a dead tubercle; but the one has died safely, the other not.

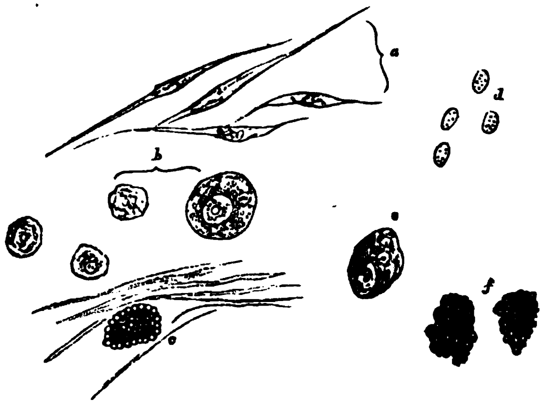

Fig. 25. Dry Calcification of Pleural Tubercle.—From a dark-grey tubercle, hard, yellow, and calcified in the centre; situated in a pleural adhesion between the first and second lobes of the right lung.

From uncalcified part—
a. Fibres developing.
b. Nucleated cells.
c. Glomerulus.
d. Tubercle-corpuscles.
No many-nucleated cells.

From calcified portion—
e. Large nucleated cell, apparently calcifying.
f. Amorphous calcareous masses.

The phenomena of the natural course of chronic tubercle are best explained by assuming that the tubercle at the first is endowed with a certain very low amount of vitality, as evidenced by its ability to form cells (however imperfect or lowly), and by its power to maintain itself *statu quo* for a given length of time,—indefinite and probably most variable as this may be. A tubercle cannot of course undergo that molecular removal and deposition, retaining its form the while, which constitute the nutrition and maintain the life of healthy structures of the higher class. But this is unnecessary to our assumption. Having no function to discharge, a tubercle needs not an active form of life. Having only to keep itself as it is, like an unhatched egg, it needs only a passive vitality. It is a reasonable conjecture, that whilst in constant contact with vitalized fluid and living structure, a tubercle may possess this low degree of life. Some healthy tissues, whose office is merely a mechanical one, have scarcely more.

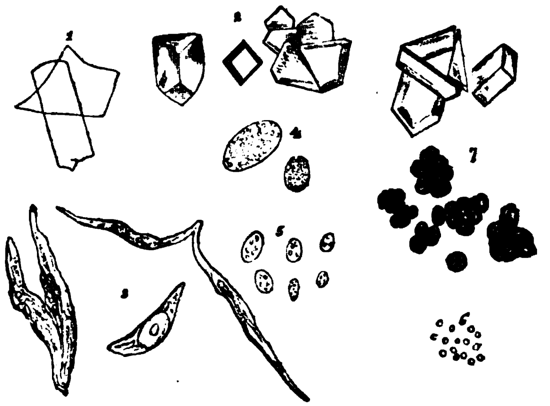

Fig. 26. Dry Calcification of Sub-peritoneal Tubercle.—No inflammation around it. Many other grey tubercles which were not calcified, dotted the peritoneum. This one was the size of a large pin-head; whitish, semi-opaque, not cheesy. It was situated in the areolar texture beneath the peritoneum, which was lifted off over the tubercle, so as to form its roof. In the fibrillar matrix were numerous granule-cells, and single tubercle-cells, and free fat-vesicles, all matted together. No many-nucleated cells.

1. Cholesterine.
2. Mineral matter, in crystals.
3. Fibres.
4. Glomeruli.
5. Single tubercle-cells.
6. Free fat.
7. Calcareous matter, in amorphous nodules.

If this be so, the period during which a tubercle is latent is the term of its life. No sooner does it cease to nourish itself than its degeneration commences, and more or less irritation ensues. The measure of the life of a tubercle is its duration before softening. In proportion therefore as the tubercle is small, transparent, and unsurrounded by inflammation, may it be considered as living. Possessing none of these characteristics, a yellow tubercle must be deemed not more alive than so much pus.

To keep a tubercle alive as long as possible, is tantamount to keeping a tubercle latent as long as possible; and the possibility, if it be a fact, concerns our reasonings rather than our practice; excepting in so far that the method best adapted for keeping progressing tubercles as quiet as possible, is doubtless the method most suited for preserving latent tubercles unchanged for the longest period.

Liquefaction.—Between the softening which is innocent in its results, and that which is destructive, what is the difference? The only difference we can discover is that of the time occupied in the process. Rapid softening of tubercle is always destructive. And in proportion as the disintegration is slow and gradual, the more is it accompanied by the formation of innocent compounds, such as oil and lime. So slowly is the change sometimes effected, that there does not occur a general melting down of the whole tubercle at once, but such a bit-by-bit softening, that one portion of a tubercle is found to have been changed into cholesterine and calcareous matter, whilst the rest has not lost its crude condition. Whether by such very partial, or by a more general change, the melting of crude tubercle into a fluid is probably an essential preliminary to ulterior changes, whether these are to be for good or evil. It will, however, be shown presently that the amount of liquefaction varies extremely.

Absorption.—The occurrence of a single specimen of obsolete tubercle, wherein no trace of the corpuscular elements which once existed is to be found, suffices to prove that the internal removal of tubercular matter is possible. Liquefaction and absorption, either atom by atom, after the fashion of normal nutrition; or by solution in the gross, and subsequent absorption of the liquid, offer the only conceivable channel. Hence, tubercle can be absorbed. But whether it can ever be so absorbed as to leave not a trace behind, is another matter. Most probably the absorption

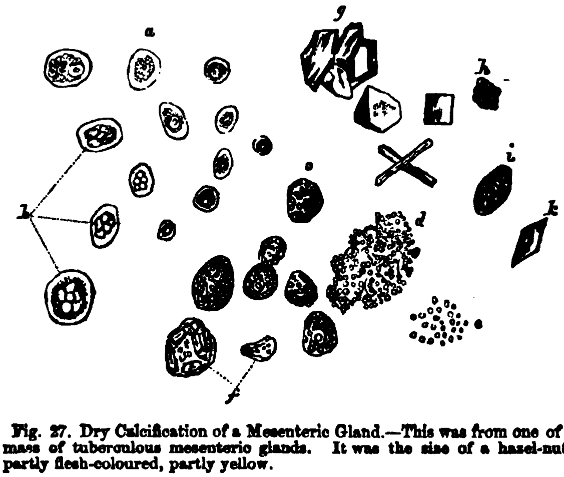

Fig. 27. Dry Calcification of a Mesenteric Gland.—This was from one of a mass of tuberculous mesenteric glands. It was the size of a hazel-nut; partly flesh-coloured, partly yellow.

a. Granulous nucleated gland-cells.
b. Gland-cells, possessing several nuclei and clear space near the margin; these were not very abundant.
c. Gland-cells, fattily degenerating; very abundant.
d. Patch of fatty débris; plentiful.
e. Free fat; abundant.
f. Cells apparently degenerating by simple, and not by fatty, atrophy.
g. Mineral matter in crystals. *h*. Amorphous mineral matter.
i, Black pigment-cell. *k*. Orange (i. e., hæmatin) crystal.

is never complete, but occasions merely the removal of the more soluble portions of the tubercular material, leaving behind the rest, which, with or without subsequent addition, forms eventually a permanent relic quite different in chemical composition to the original tubercle.

Of the perfect absorption of nascent tubercles, it is not easy to imagine what proof would be possible. Certainly, such proof is not to be found in the disappearance of dulness, and the restoration of presumedly healthy respiration in the pulmonary apices in certain cases which have been published as examples of arrested phthisis. Granting the existence of miliary tubercles in these instances, the physical signs are due to the accompanying inflammatory changes, and will disappear if the whole of these can be made to disappear, notwithstanding that the tubercles, either stationary or transformed, still remain. These cases, satisfactory as they are when observed by such observers as some who have narrated them, as evidences of arrested tubercle, are no evidence whatever that the arrest has taken place by means of absorption. At present we are without any grounds for hoping that tubercle can ever undergo entire absorption. The conditions of the deposit are against it; for even if the tubercle itself could so liquefy into a simple fluid as to be fitted for complete resorption, the state of the parts in which it lies is unfavourable to it. Vascular by nature, as it is, immediately around the tubercle, the lung no longer possesses its natural condition. If not condensed by exudation-matter, still it can no longer have its natural freedom of capillary circulation close to such a firm morbid deposit as even the smallest and most innocent tubercle constitutes. There must always be a tendency to more or less of stagnation of blood around even a Bayle's granulation; and in most instances of arrest, the metamorphosed tubercle is narrowly enclosed by a bed of black matter, which in itself indicates that there has been such a stagnation of blood; or, by firmly contracted pulmonary texture, in which but little circulation of any kind can have been carried on; or lastly, by a dense fibrinous capsule. In every case the conditions are such as do not favour absorption.

Absorption, nevertheless, must take place under the following circumstances:—The thinner part of the fluid plasma, when tuberculous blastema is exuded, is probably resorbed. For the thickening of liquefied tubercle on its way to cretifaction, absorption is necessary. And in certain closed tuberculous abscesses, which are only partially filled, the same probably occurs. Such an abscess may occasionally be found, varying in size from a pea to a walnut, forming a perfectly closed cavity, which is not more than half-filled by its pus-like contents, and is lined throughout by a soft velvety coat. In the surrounding lung, which is for a certain distance consolidated, the bronchial tubes end in blind conical terminations, which are sealed up by fibrinous adhesion, next to which lies a little concrete pus. This is the state of things after death. During life, of course, there was no empty space in the vomica. It was filled according to its then capacity; but it had contained more fluid formerly than recently, and had latterly been accommodated to the reduced quantity of its contents by the pneumatic pressure of the lung. This being granted, there are two modes only by which the reduction of the contents of the vomica could have been effected; by the now closed vomica having once communicated with a bronchial tube in the ordinary way, and, after having partially emptied itself, having had its communication closed up and healed—which is so opposed to the ordinary course of events, that we can hardly venture to accept it; or by absorption from the vomica, closed in from the first, of the thinner portion of its contents.

We must conclude, that tubercle may be sufficiently absorbed under favourable circumstances to be rendered innocuous; but that its capability to undergo perfect and entire removal by absorption in any case, is unproven and improbable.

Fatty Degeneration of Tubercle.—All normal tissues, and all morbid formations, and perhaps we may add all animal fluids which can become inspissated whilst within the body, are susceptible of transformation into oily matter. This assumption of the form of a product which is not peculiar to the animal kingdom, is in every instance a sign of declension in the scale of vitality. When it affects what is useful to the economy, it is of course an evil; but when it affects what is itself injurious to the system, it becomes a benefit. It is the stepping-stone towards the gradual removal, or the safe housing, of that which cannot directly melt into simple liquid fitted for absorption. It is the half-way stage between the animal and mineral conditions. It affects tubercle in various degrees. When there is but little of fatty degeneration, with much of granular detritus and of liquid, the softening is of a bad character and ulcerative tendency; but in proportion as there is more of fatty metamorphosis, with plates of cholesterine and particles of lime-salts, is the softening slower, less locally irritating, and therefore safer.

To a greater or less extent, fatty degeneration plays an essential part in all slow softening of tubercle. Whether in a given instance it will prove curative or destructive, depends entirely upon the attendant conditions, particularly in respect to the time permitted for the performance of the change.

Granular Degeneration of tubercle is simply its disintegration into such molecules as are not oily. The resultant molecular matter is ordinarily considered to be albuminous. More or less of it attends every stage of the transformation of tubercle into cretaceous matter.

Calcareous Degeneration.—Mineral matter is met with in tubercle in the amorphous and in the crystalline form. The amorphous is most common and abundant; to the naked eye it resembles bits of grey hard mortar, either in loose particles or in masses; under the microscope it is black and opaque, and when crushed as fine as possible, the small particles being highly refractive, have a resemblance to oil-dots. In chemical composition, Boudet stated (1844) that they consist principally (70 per cent.) of the soluble salts—chloride of sodium, phosphate of soda, and sulphate of soda. Before that, Thenard had found them to be composed almost entirely of phosphate of lime, with a little carbonate of lime. More recently, Scherer, and several other authorities referred to by Simon, have arrived at the same conclusion. It may, therefore, be decided that the old opinion is the correct one; and that a calcified tubercle consists mainly of the insoluble phosphate and carbonate of lime, held together by a little animal matter.

The crystals resemble those of triple phosphate, which they probably are.

Before any calcareous matter in a distinct form can appear, it is requisite that the crude tubercle shall have liquefied, either generally or partially. Crystallization requires such a physical condition as will allow the saline atoms to move freely into the sphere of their respective affinities, and can therefore only take place under the fluid condition, either gaseous or liquid. The presence of crystals in a tubercle, therefore, demonstrates that there has been fluidity. But as we sometimes notice crystals of hæmatine under circumstances where the fluidity must have been limited to a very small point, the crystals in a metamorphosed tubercle do not necessarily prove that there has been any universal liquefaction of the tubercle at once. That may have been so; or, on the other hand, a minute point of tubercle only may have been liquid at the moment of crystallization. And if general liquefaction of the tubercle be not an essential preliminary to calcification when crystals are present, still less can it be considered as a *sine quâ non* when merely the amorphous variety of calcareous matter exists.

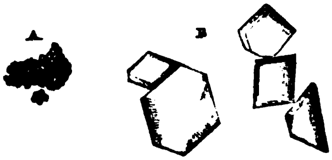

Fig. 28. Mineral Concretions from Tuberculous Bronchial Gland.
A. Amorphous; most abundant. B. Crystals.

We will assume that a tubercle may attain the state of calcification, either by way of general liquefaction (moist calcification), or by way of comparatively dry fatty metamorphosis (dry calcification).

When we find a mineral concretion closely invested by condensed lung, nested-in, as it were, without any fluid about it, it is impossible to draw any conclusion as to the degree of liquefaction which may previously have taken place during the calcifying process. But when we see a tubercle dry and cheesy, which contains granules, abundant fatty molecules, plates of cholesterine, and calcareous particles, there is no evidence that fluid *in notable quantity* has any share in effecting the transmutation. We have examples of this when we find a nucleus of calcareous matter in the centre of a crude pulmonary tubercle; when a grey opaque tubercle on the peritoneum is only partially calcified; when a tuberculous bronchial or mesenteric gland is solid and gritty, from the presence in combination of unsoftened tubercle and calcareous matter. In the large entozoic abscesses so common in the lungs of the ox and the pig, no stage of general fluidity seems to intervene between that of thick sticky pus which follows the death of the cysticerci, and that of calcareous formation.

The practical interest of the question resides in this:—If tubercle can reach its ultimate safe condition of calcification without having general softening as a necessary antecedent stage, this must be the most favourable course; and if so, *the cure of tubercle by cretifaction will proceed the best when there is least of local or constitutional manifestation of its performance.*

The origin of the lime-salts is probably twofold, original and superadded. Primarily, they are merely the insoluble elements of the original tubercle, left behind when the remainder is dissolved and removed by absorption. During the early stage of fluid blastema, these salts are held in solution, or mechanically suspended, just as they constantly are in the blood-serum, and just as lime is in hard spring water. But after coagulation, and the resorption of the thinner part of the exudation, the mineral matter being left behind, bears a larger relative proportion—too large now to admit of being re-dissolved when the solid tubercle liquefies. Moreover, just as the hard water on losing some of its carbonic acid by boiling, becomes unable any longer to retain all its lime in solution, so may the tubercle-salts undergo some chemical change which deprives them of the solubility which they formerly possessed. In whatever way it arises, the insolubility of these salts renders them as fixed, as the insolubility of its carbon renders the pigment in black pulmonary matter.

The mineral matter of a calcified tubercle, however, is ordinarily more than could originally have entered into the composition of the tubercle. According to Thenard, whilst crude tubercle contains only three per cent. of mineral matter, a calcified tubercle contains ninety-six per cent. This does not prove much, unless it can be shown that the tubercle has not dwindled down from its original size in a corresponding degree. There is always some reduction in size, although in the lungs this is difficult to estimate, owing to the extensive shrinking and puckering of the lung around the cretified tubercle. In some instances, however, its occurrence to a large extent is undoubted. But amongst tubercles on serous membranes we frequently find a calcareous nodule as large as any of the neighbouring little tubercles, in which it is plain that there is more of earthy matter than could have belonged to the original tubercle.

There are certain appearances in an incipient stage of calcification which seem to indicate that the mineralization sometimes commences in the cell-formations of the deposit. This is less obvious in tubercle than in other deposits in which bolder cells are present. In one instance (see fig. 29) the nucleus of a cell of bronchial epithelium found in a partially calcified entozoic granule-mass in the lung of a sheep, appeared to be the seat of calcareous deposit before the rest. And the general resemblance in outline of clusters of cells to masses of calcareous matter in many instances suggests the idea of a sort of petrifaction having affected these. If this be so, it is only similar to what happens when dissolved hæmatine is attracted towards the nuclei, or into the interior of cells (see fig. 30). For any such petrifactive process, lime in solution, over and above that originally present in the tubercle, must be supplied. Considering the long duration occupied by the process, there is no difficulty in admitting that the fluid of the softened tubercle when in contact with lung-tissue may, by intermixture, obtain from the serum of the blood the required addition of lime-salts for the augmentation of the calcareous deposit already there.

The state of the pulmonary texture which immediately surrounds a tubercle cured by calcification, is obviously a subject of great interest. We might expect that we could learn at once what condition was most favourable to a termination so desirable: but the information is not easily gained. In one instance, we find a calcified tubercle embedded in lung which is merely condensed and blackened with pigment; in another, the lung is apparently healthy, but separated from the calcified tubercle by a fibrinous capsule; in a third, the surrounding lung may present any degree of inflammatory consolidation. Are we, then, to conclude, that the condition of the surrounding lung is a matter of indifference as regards the calcareous degeneration of the tubercle? By no means. When inflammation exists, there is always reason to infer from the general appearances presented, either that it arose subsequently to the mineralization of the tubercle, or else, if it preceded this, that it was of that mild plastic kind which, in surgery, is styled adhesive inflammation.

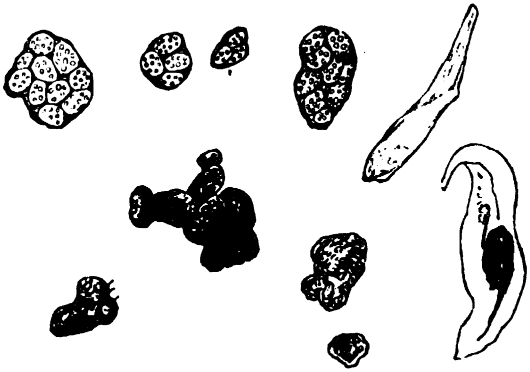

Fig. 29. From Sheep.—From a cretifying granule-mass, which had formerly been the nidus of worms in the lungs of a sheep. Many-nucleated cells and bronchial epithelial cells apparently withering and becoming the seat of calcareous deposit.

Calcification is the great arrestive change of developed tubercle; but it is often abortive. A young person may expectorate cretaceous particles, and never have phthisis; but another person may do the same towards the close of mortal consumption. In a large cavity we may find such numerous cretaceous particles, that the lining membrane is gritty to the touch, and yet the course of that tubercle-mass had proved destructive, notwithstanding this tendency to cretifaction. Or again, many small tubercles may be found perfectly calcified, and thereby cured so far as they individually were concerned; but inasmuch as many other tubercles had run the destructive course, this partial cure was futile. It is indeed very rare to examine a case of chronic phthisis after death without observing some evidence of tubercles having undergone calcification.

Pigmental Degeneration.—After adult age, as is well known, the lungs are never free from dark streaking. This darkness increases in extent as age advances, so that the black mottled lung of the old always forms a marked contrast to the pinkish lung of the infant. In every case of chronic phthisis, we find more or less of this black matter. In primary acute phthisis, it is not necessarily present. It always exists in abundance around the firm walls of old cavities; around cicatrices; around calcified tubercles; and in the immediate neighbourhood of a cluster of shrunken miliary grey tubercles. It is therefore an habitual concomitant of every form of arrested tubercle.

On the other hand, in certain cases the blackened lung is so hard and evidently disabled in function, that the presence of black matter has been accounted a specific disease. The diseased lung "as hard as granite, crackling under the scalpel, breaking with a metallic lustre, presenting small cavities filled with pus-like fluid, and accompanied by suppuration in the adjoining bronchial tubes, occasioning during life the ordinary symptoms of phthisis,"—in such terms Bayle described the disease as phthisis from melanosis. Haller had previously referred to it as constituting "a horrible species of phthisis, in which the lung is filled with a substance as black as ink." Besides the diffused black induration, there are other cases in which the lung is filled with distinct hard black knobs and nodules, the intervening tissue appearing to be natural. Some of these black knobs are softened in the centre, containing, however, not black, but the ordinary yellow fluid of softened indurations. Others sometimes enclose the cretified remains of a tubercle; and there is usually the common kind of tubercle present in some other part. In another case, the lung may be full of black softening nodules, whilst soft cancer exists, either with or without blackness, elsewhere. In every instance of common chronic phthisis which has proved fatal, we find abundance of blackened lung. The change is therefore an habitual concomitant of chronic tubercle when it pursues a destructive course.

Now, are these several varieties of black lung the same thing? As regards the blackness, yes; as regards the disease of which the blackness is merely the accompaniment, no. Just as hæmorrhage may attend repeated congestions, or pneumonia, or tubercle, or cancer of the lung, so may the pigmental change. When, therefore, Bayle described a melanotic phthisis; Laennec considered melanosis of the lung as a species of cancer; Andral viewed it as a peculiar form of chronic pneumonia; and Hasse, under the term pseudo-melanosis, distinguished two forms of black pulmonary disease, an innocent and a malignant; each of these eminent authorities was quite accurate as far as his observation extended.

As to the consequence of this black change, those who, like Laennec, draw a generic distinction between the common black matter and the black matter of melanosis, of course consider all blackening of the lung which exceeds the physiological amount as unequivocally bad. Dr. Paxton, without drawing any such distinction, remarks, that "We ought to be no longer under the erroneous impression that the retention of uncombined carbon has any other than ill consequences."* Others consider, with Dr. Hope, "the black pulmonary matter to be altogether compatible with perfect health." M. Nathalis Guillot was the first to affirm (in 1845) that the formation of black pulmonary matter around tubercles exercised a direct curative influence over them. He considered it to be a deposition of carbon, which occluded the vessels around the tubercle by choking-up, as it were, the tissue of the lung, and so cut off the tubercle from the influence of the circulation. He stated (as formerly remarked) that every tubercle is separated from permeable lung by a zone, into which the pulmonary capillaries do not enter, but in which twigs from the bronchial arteries anastomose with twigs from the arteries of the thoracic walls through the intervention of pleural adhesions. This zone becoming filled with carbon, the tubercle, thus walled in by a charcoal barrier, ceases to receive any nutritious supply, and therefore ceases to grow. This conclusion he founded chiefly on two circumstances. First, that all tubercles which are found in a cretified state, all cicatrices presumed to mark the site of former tuberculous cavities, or puckerings around shrunken tubercles, have, as a rule, a quantity of black pigment close around them. Second, that when tubercles in the aged are so close together as not to admit of any black deposit between them, they pursue the destructive course. It is obvious that neither of these reasons is conclusive. The former may be a mere coincidence; and the latter admits of a different explanation. However, there is still some value in the supposition that the black matter tends to promote arrest.

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* Transactions of the Provincial Medical and Surgical Association, vol. xvi. p. 51.

What is the nature of the pigmental degeneration? The lung might be rendered black by the infiltration of a black secretion; by the introduction of insoluble black matter from without; by a defective excretion of carbonic acid, causing (if it can do so) a precipitation of carbon; by a metamorphosis of extravasated red blood-globules; or by a solution of the colouring matter of the blood first infiltrating the tissue, and then precipitating its altered colour-element.

Seeing that it is presumable that all colouring, both of tissues and secretions, arises ultimately from changed hæmatine, there is little real difference between the first and the last of these hypotheses. Laying aside, therefore, the first, the second expresses a demonstrated fact. Carbon is really introduced from without, and works its way into the lungs from the bronchial tubes and air-cells, in coal miners and others who constantly inhale the thick smoke of the candles by which they see to work. After a time, this smoke-carbon induces local irritation, and, under the name of miner's asthma, is analogous to the pulmonary disease of masons and knife-grinders occasioned by the habitual inhalation of stone dust or iron particles. Designated anthracosis, this, which is the correct explanation of certain cases, was incorrectly applied by its discoverers to all instances of black pulmonary change. We may at least deduce from it (taking into account the co-presence of other irritating elements of smoke), that a large quantity of carbon will in the end act as an irritant when introduced within the texture of the lung.

Heusinger's idea (with which Dr. Paxton coincides), that the black matter is due to defective elimination of carbonic acid, is inadequate to explain the presence of black pigment in the centre of a tubercle, or cancer, or upon the pleura or peritoneum; whilst all the arguments in its favour are equally well explained by ascribing them to obstructed circulation in the part, or in its immediate vicinity.

The only view which will at once meet all the facts, is that foreshadowed by Breschet, Andral, and Dr. C. B. Williams, cleared up by the German pathologists, and now generally received—that, namely, which refers all black colouration of tissues to a direct transformation of the colouring matter of the blood. The most clear and masterly account of what is known on the subject is given by Rokitansky.*

We occasionally find extravasated red globules in the substance of tubercle; we may always find them in the lung-tissue close to tubercles, at every stage of transformation into black pigment. They may be seen in the interior of filmy cells, within which they disintegrate and change into orange, brown, and finally, black molecules, thus eventually forming cells filled with black granules; or, remaining unenclosed, they pursue a similar course as free molecules. The changes of colour of a common bruise doubtless depend on similar metamorphoses of the red globules extravasated into the skin.

But there are other appearances, only to be explained by assuming that the hæmatine has first been dissolved and then the coloured solution imbibed by the cells of the morbid formation, or tissue, and afterwards precipitated within these in the shape of black molecules.

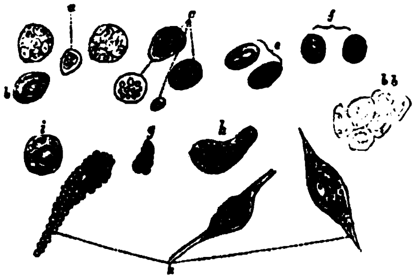

Fig. 30. Pigmental Change of Lung.

a. Compound cells, not blackened; few. *b*. Single nucleated cell.
b. *b*. Small flake of epithelial cells. *c*. Pigment-cells, not nucleated.
e. *f*. Pigment-cells, nucleated. Those bracketed together are the same cells seen at different foci.
g. Black pigment, concreted, apparently not in a cell.
h. An orange-coloured cell (? a bronchial cell, degenerating, and stained with hæmatine).
i. Round nucleated cell, in which black pigment seems to have concreted in contact with the nucleus.
k. Bronchial epithelial cells, blackened with pigment.

In this manner only can we account for the partial or complete blackening of bronchial epithelium, or nucleated round cells, or fibre cells, or cancer cells.

Pigmental change, then, whether in the form of free black molecules, or of the same in cells, is essentially a degeneration of red blood-globules, a safe local necræmia, and must always imply a previous obstructed state of the capillary circulation at the part. In chemical composition, it is carbonaceous—indeed, as carbon is the chief fixed element of organized bodies, all dead organic matter, decayed wood and dry gangrene alike, is chiefly carbon, and is more or less black.

According to Rokitansky, black pigment in the lungs causes thickening of the areolar tissue, which impedes the development of the air cells, gradually obliterates their vessels, and so occasions their atrophy. Senile atrophy of the lungs, he states, is undoubtedly often induced by an excessive accumulation of pigment in the interstitial tissue. "It is the result of slight irritative processes and of transient stases; the pigment is conveyed by absorption to the bronchial glands, and is thus deposited in them,"—probably it is absorbed in the form of a solution of hæmatine.

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* Sydenham Society's edition, vol. i. p. 204; vol. iv. p. 101.

It thus becomes intelligible how a small amount of black degeneration of lung is unimportant, whilst an excessive amount may induce of itself serious disease; how, when the blackness is merely a dress worn for the occasion by cancer, blackness of lung will be malignant; or, when assumed by tubercle, the term employed by Dr. C. B. Williams, of "black tubercle," becomes strictly applicable. Not so, perhaps, the remark of Haller, that this form of consumption is unusually "horrible;" on the contrary, it is habitually more chronic than usual.

And although pigmental deposition often fails to check the destructiveness of tubercle, yet, as Guillot contends, it is, in a tuberculous lung, of good rather than of evil import. It is so both for what it implies and for what it does. It implies chronicity and the absence of acute inflammation. It leads to a diminished supply of blood-plasma to the periphery of the tubercle, and incapacitates the portion of lung immediately around the tubercle from mechanical movement and functional activity. It surrounds the tubercle with a barrier both physical and vital. By lessening the supply of blood it starves the tubercle, and so prevents its increase in size—in itself a great gain. As vascularity is lessened the chance of acute inflammation lessens, and the tubercle is left to itself, to undergo its natural degeneration slowly and quietly; and, next to having a tubercle quite stationary, to have it left to itself without adjoining irritation, affords the most advantageous terms. Even if, when in excess, black pigment is itself somewhat irritating, provided it exclude the presence of more tubercle, by occupying the place of it on the one hand, and by occluding the capillaries on the other, it is so much of a gain, as black pigment *per se* is a more innocent formation than tubercle.

This question is of little importance during life, since we cannot, from greyness or black streaking of the sputa, augur anything. But it seems very interesting in pathology from its accordance with the anti-tuberculous tendency of compression and diminished vascularity evidenced under other circumstances.

Shrivelling; Cornification.—On the authority of Rokitansky, grey semi-transparent miliary tubercles may change into horn-like nodules. In his opinion, this is the *only* kind of metamorphosis to which this particular tubercle is liable. Reasons have already been assigned for preferring the view more generally entertained. Bearing in mind the cartilage-like resistance and condensed appearance of the tubercle called Bayle's granulation, and of the smaller grey distinct tubercles in general, it seems difficult to discriminate between these and a tubercle said to be cornified. No doubt, *all* firm semi-transparent tubercles are dried, and more resistant than they were at the earliest moment of their formation; but this is for them only the mature or crude condition, and not a state of decadence. Having no personal knowledge on this point, I will merely quote Rokitansky's brief and clear description:

"After abiding in the primitive, *crude* condition, the simple fibrinous tubercle becomes transformed with the loss of its moisture—with condensation—to a hard nodule, and shrivels into a tough, amorphous, or indistinctly fibrous horn-like mass—in a word, *cornifies*. This determines a complete wasting or death of the tubercle, subversive of all further change." *

Primary yellow tubercle is never changed into a hard resistant nodule by any other method than calcification. When completely calcified, the hardened mass occupies less space than did the original tubercle, the lung is condensed and puckered around it, and hence both tubercle and pulmonary texture may be said to have undergone shrinking—but this is not of the same character as the shrivelling of grey tubercle described above. A slight degree of shrivelling, however, without previous calcification, may be assumed for yellow tubercle under the following circumstances.

Sequestration.—We sometimes find a distinct yellow tubercle, firm, smooth, and leathery, seldom exceeding the size of a bean or hazel-nut, which is surrounded by a distinct fibrinous capsule. It may be situated in apparently healthy lung, or in inflamed lung; but in the latter case the inflammation has obviously been a subsequent occurrence. Such a capsulated tubercle may be found by itself in a lung which is elsewhere riddled with cavities. It is quite evident that the capsulated tubercle is an old one. It has, therefore, apparently been saved from pursuing the destructive course of the other tubercles by the circumstance of its isolation by means of its fibrinous capsule. We may conjecture that such a tubercle was laid down early in the disease, that by some temporary improvement in the cachexy, healthy induration-lymph was thrown out around it: that this, becoming as much organized as such low fibrinous tissue requires, contracted around the tubercle, which thus became subjected to the twofold influence of compression and diminished supply of blood. Under such circumstances, a yellow tubercle may be considered to have undergone a certain degree of shrivelling.

We have never met with a grey semi-transparent tubercle capsulated; and yet, as this is the more sthenic variety, we might anticipate that it would be more likely to be surrounded by a sthenic kind of lymph than the yellow tubercle. And as the freer access of blood, in consequence of the small size of a grey semi-transparent tubercle, has been assigned as a means of keeping such a tubercle stationary, is there not a seeming contradiction in now ascribing to a cutting-off of the supply of blood a beneficial tendency?

A seeming contradiction only. So long as a miliary tubercle is minute and grey and transparent, and free from surrounding inflammation, neither the tubercle nor the lung-tissue included in it can be looked upon as quite dead. To keep it from dying, a supply of liquor sanguinis is an essential. A second essential is, that no inflammation shall interfere. So soon as inflammation does arise, the transparency vanishes. The tubercle becomes opaque, and thenceforward pursues the usual course. Now, as a fibrinous capsule can only originate from a certain (adhesive) amount of inflammation, and as this would occasion opacity, we see why the enclosed tubercle is never found to be of the semi-transparent kind. Yellow tubercle involves speedy death of the included lung-tissue, and is itself as devoid of vitality as pus. Like pus, it is susceptible of a very slow metamorphosis of a fatty and calcareous nature, during which it may produce no persistent local irritation, and which eventually converts it into an inert concrete. But, in order to do this, pus must be almost isolated from the circulation by its abscess-walls; the inflammation which produced it and its walls must cease; a lowly organized texture must in this way be interposed between the pus and the living tissues; the pus must be left to itself. In like manner, in the rare instances in which the inflammatory reaction of the lung around a yellow tubercle leads to the effusion of good adhesive lymph, unmixed with tubercle-plasma, the tubercle becomes isolated and compressed; it is not kept alive—for it is not alive to begin with—but, as inanimate matter, its changes are allowed to take place slowly, and therefore they do not irritate.

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* Sydenham Society's edition, vol. i. p. 295.

The fact is, that the two varieties of tubercle are virtually two things in their deportment towards the texture affected and the system at large. It is, therefore, no real contradiction to infer that a supply of blood-plasma is good for the one, bad for the other. Or, in other words, that it is desirable to feed that which can live, however humbly, and to cut off supplies from that which is incapable of living at all.

On microscopically examining a buff capsulated tubercle, we find abundance of oil-molecules, plates of cholesterine, some remains of tubercle-corpuscles, granular matter, and occasionally calcareous particles or masses.

CURE BY ELIMINATION.

After tubercle has softened and opened into a bronchial tube, the following modes of cure are still possible:

Hæmorrhage may take place and fill the cavity. The blood-clot, by sealing up and compressing the opened bloodvessel, checks the hæmorrhage. It afterwards gradually undergoes fatty and calcareous degeneration; the walls of the cavity contract around it, and ultimately form a cicatrix containing within it calcareous matter. Rokitansky is the sole authority for this; and the unusual concatenation of favourable circumstances under which alone it could happen, must render its occurrence so exceedingly rare that it is here mentioned first because mere allusion to its possibility will suffice.

After the softened tubercle has been ejected, the cavity may heal by union of its walls together so as to form a cicatrix;* or, it may remain open but contracted in size, ceasing to secrete pus, and forming an innocent blind fistula in communication with a bronchial tube; or, remaining open but not extending itself, it may continue to secrete pus in small quantity for an indefinite period.

When a cavity has contracted into a blind sinus, like a large offshoot from a bronchial tube, either it is lined by a soft velvety coat which will strip off the wall, or the wall itself presents a smooth free surface, without any distinct removable layer. The microscopic appearance of the surface is very similar in either case. It presents a network of delicate fibres, thickly dotted with granule cells entangled in its meshes, having none of the regularity of a pavement epithelium, but reminding one of the inner surface of an old abscess. When a cavity has ended in cicatrization, there may be simply a dense linear fibrous scar, without anything in it; or, what is more common, some calcareous matter is found imbedded in the scar. Under all circumstances, whether of partial or complete obliteration of the cavity, the adjoining lung is condensed, puckered, black, and usually emphysematous. The condensation and puckering are, of course, directly due to the contraction of the induration-lymph which is the primary step in the healing process. The pigmental change depends upon the obstructed circulation. The emphysema is caused by the compensation-expansion of some air vesicles in lieu of those which are atrophied in the condensation of the lung, on the pneumatic-pressure theory so clearly stated by Hasse, and fully argued by Dr. Gairdner. By lessening the vascularity and mobility of the part, both these changes promote the further progress of the favourable condition which caused them. And thus, once in a good line, all Nature's interpendent processes here become curative; as, in a bad line, they all aid and abet each other in proving destructive.

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* See the beautiful illustrations given by Dr. Hughes Bennett, in his work on Pulmonary Tuberculosis. 1853.

In order to admit of any of these favourable conditions, the lung-tissue immediately around a cavity must have been infiltrated with tolerably good lymph, or common induration-matter. This must be permitted to pursue its natural course of firmly contracting, and so drawing together the cavity it encloses; it must not, therefore, be interfered with by the continuance of inflammation. Inflammation at first is necessary for producing the requisite exudation, but after this the lymph is only injured by the persistence of inflammatory action. In short, in order to heal, either partially or completely, a tuberculous abscess must follow the course of a simple abscess. It must, therefore, as far as possible, have been reduced to the condition of a simple abscess beforehand. Now the lungs furnish an unfavourable site for the healing of even a common abscess, owing to their incessant movement. To obviate this, any lesion of continuity in the lungs requires to be first rendered, as much as may be, a fixed point. This is accomplished by surrounding it with induration matter. If, after this, *all inflammation subside*, and *the constitution be healthy*, the wound or the abscess in the lung will heal, always leaving behind it a certain amount of cicatrix. Under all circumstances a crude tubercle involves a solution of continuity, even before it begins to soften, because it has necessarily destroyed some of the texture of the lung. For a tubercle, therefore, entirely to disappear, and leave not a trace behind, is an impossibility. When it pursues its most favourable course, and undergoes arrest without ever forming an abscess, there is still a destruction of so much of the lung as the tubercle occupied space. Hence, a lung once the seat of tubercle can never be restored to perfect health, in the sense of restoration to its pristine anatomical state. Cure of tubercle can be nothing better than the safe destruction of the tuberculised portions, and a healthy condition of the remainder.

To the cure of a tuberculous abscess the previous opening into a bronchial tube, and emptying of the contents, are neither essential, nor do they constitute the most favourable course. In the lungs of the ox, we may frequently see examples of entozoic abscesses in every stage towards obliteration, without ulcerating into a bronchial tube. First, there is a silvery fibrous cyst, presenting a smooth, bright, epitheliated surface, on which cysticerci hang sessile, and filled with clear transparent fluid. The cysticerci die; the lining membrane inflames and secretes pus; the inner surface of the cyst now looks rough and velvety, and the cavity is filled with greenish-yellow, very glutinous, thick pus. This under the microscope is found to be unusually fatty; it presents abundance of free oil-molecules, and almost no pus serum. Minute gritty particles of calcareous matter may be felt on rubbing this pus under the finger, and are readily seen; later on, the proportion of calcareous matter is larger, and there is some shrinking of the cyst-walls; later still, the pus has disappeared, and the earthy matter has largely increased, forming an aggregate of closely-packed calcareous nodules, plates of cholesterine, and granular matter. The walls of the cyst have contracted around this concrete, and the final condition of a scar enclosing earthy matter is attained. In like manner may a tuberculous abscess attain the condition of scar-obliteration, without ever having been an open ulcer. If we find a large quantity of calcareous concretion in a dense puckered scar, we may infer with some probability that there had been no open cavity there, but merely a large vomica, or closed abscess. An open cavity is not favourable to the lodgment of calcareous matter on two accounts—first, because any that might be there would ordinarily be removed by expectoration; and secondly, because the admission of air excites the quick process of suppuration—i.e., the hyperoxidation of exudation-cells—rather than those slow transformations which lead to calcareous deposit. Still, the inference would not be unquestionable, since we do find small calcareous particles adhering to the membraniform lining of open cavities, which might escape ejection; and in addition to these, supposing such an open cavity to contract and close, some of the lymph effused into it during the closure might undergo calcareous degeneration.

The scar-obliteration of a cavity is the most complete and radical cure, so far as that one cavity is concerned. The cicatrix does not become the seat of tubercle, because of its density and non-vascularity; in accordance with the law that tubercle selects by preference the most vascular and least resisting parts.*

Of the absolute frequency with which such healing of a cavity takes place, we have no positive knowledge; probably it is not extremely rare; but of the infrequency with which it *so* takes place that the patient eventually recovers from his phthisis, we have sadly too established a conviction. The possibility of such a cure is quite certain; but that we have a right to anticipate it in any case of undoubted tuberculous cavity in the lung, all experience forbids. Nor is it difficult to see why this must be so. In the first place, the cachexy has to be removed. It is the fashion of the moment to speak as if it were an *easy* thing to alter the tubercular disposition in the constitution. We know better than formerly what to aim at, and how to act, but every honest observer must admit that his practical success falls far short of his knowledge. However, passing by this grand constitutional difficulty, there is much in the local state to baffle us. A cavity cannot heal when its walls are surrounded by tuberculous nodules which soften in succession. A cavity cannot heal when its walls are surrounded by active inflammations constantly repeated; nor when its own internal surface is habitually the seat of inflammation. It cannot heal when placed in the midst of consolidation, which neither fairly suppurates nor yet contracts; and how many cavities are there of which these are not the attendant conditions? —the most opposite conceivable from those of simple abscess. And if a given cavity does heal, as probably one of the early formed in chronic phthisis not very unfrequently does, the cure of this individual cavity is of no eventual benefit, providing other cavities, which will not heal, coexist or supervene. It has happened in rare cases that only one large cavity existed, and that this has healed—a true case of consumption cured in its last stage. But as the rule, where there is one cavity there are several, and where there are several, the healing of one is insufficient, and the healing of many in succession is contrary to observation.

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* Illustrations of Tubercle, by Edward H. Sieveking, M.D., in Association Medical Journal May 27, 1853.

When a cavity will not heal, the next best result is, that it shall cease to enlarge. If it does not enlarge, after a time it will usually become less, will secrete less pus, and constitute a sinus, which, so long as it is kept free from inflammation, may permit of an astonishingly fair amount of health. Under favourable circumstances of hygiene, the period during which a patient will exist, and enjoy his existence, notwithstanding he has a cavity in his lungs, provided the disease be checked, is quite indefinite. I occasionally see a gentleman, aged fifty, whose history proclaims that the present large cavity in his right lung has been there for twenty-one years. Being rich, he resides in a mild climate every winter; yachts or rides out on horseback all summer; lives generously; takes wine and bitter beer, the steel mixture, and occasionally (of late years) cod oil. He keeps his digestive organs in good order, and any accidental increase of irritation in the lung is attended to by mild counter-irritation, intermitting the habitual routine of diet and tonics. This gentleman has expectorated flocculent pus, occasionally streaked, to the extent of from one to two ounces per day, during the entire course of his ailment. He enters into society, and enjoys his life. *He has a pulse of 70, and a cool skin.* With a quick pulse and hot skin, such a case was never seen.

INFLUENCE OF INFLAMMATION ON THE CURE OF TUBERCLE.

Such are the several methods by which nature effects a cure in chronic pulmonary tubercle. How far are they promoted or opposed by the supervention of inflammation?

Fifty years since, when phthisis was made to include all chronic diseases of the lungs which are attended by expectoration and wasting, no doubt was entertained that inflammation was injurious. By disproving the inflammation-theory of the origin of tubercle, Louis and his followers have indirectly given rise to a vague idea that inflammation exercises but little injurious influence upon phthisis. Whilst still more recent pathologists affirm that, under certain circumstances, inflammation is a positive benefit.

There is always a tendency in reasoning, when we adopt one precise opinion, to reject its opposite, irrespective of modifying circumstances. This exclusiveness has ever proved very baneful in medicine. Because tubercle does not ordinarily originate in inflammation, and is consequently not essentially an inflammatory product, it does not follow that it *never* does. Because tubercle is always a constitutional disease, and usually arises spontaneously, or from within, it does not follow that it never arises as a local disease. Because without constitutional treatment we can do nothing in phthisis, it does not follow that by local treatment we may not do much.* Because arrested tubercle shows the results of inflammatory action manifest about it, it does not follow that all inflammation is good. Nor because tubercle never kills without inflammation, is all inflammation of necessity bad.

In speaking of inflammation we are hampered by language. The same word is used generically to express at once an extensive class, and every variety in that class. And when the various processes signified are so alike that no abrupt line of demarcation exists between them, yet so different in effect that at one pole we have merely a conservative modification of healthy nutrition, at the other a process necessarily fatal,—to express them all by the one word inflammation without qualifying it, can scarcely fail to engender confusion of thought. To avoid the merely verbal mistakes so often committed, we must define what we mean by the term, and qualify its application in using it.

Defining inflammation as a triple unity, consisting of a nervous element, a vascular element, and an extra vascular element—i. e., of congestion, exudation, *and of more local irritation than the mere mechanical effect of the vascular disorder will account for*,—I will briefly state what can be said for and against inflammation as influencing pulmonary tubercles.

There are four points respecting the bearing of inflammation upon phthisis which it is necessary to keep distinctly in view. 1. Is inflammation the cause of tubercles in all cases? 2. Is it the cause of tubercles in any case? 3. When inflammation attacks a lung which is already tuberculized, what is the dangerousness of the inflammatory attack *per se?* 4. What is its ultimate effect upon the course of the tubercles?

The first two of these questions admit of a categorical answer. Tubercles generally arise without inflammation. But they sometimes arise with inflammation in such manner that the inflammation seems to have occasioned their production; and also they sometimes follow in the wake of an inflammation in such wise that the inflammation seems either to have roused up a predisposition formerly latent, or to have itself made a predisposition which did not before exist. We see this illustrated by a close analogy in external tuberculosis of the bones, or joints, or lymphatic glands. In one case, tubercle is formed spontaneously, and inflammation follows. In another, an accidental injury excites inflammation, and tubercle follows.

The third question requires a guarded reply. The danger of an attack of pneumonia, when it occurs in a phthisical patient, depends on the stamina of the individual, the stage and extent of his phthisis, and the seat and extent of the pneumonia. It has been shown by Louis, Grisolle, and Walshe, that in itself this pneumonia is "less fatal than primary pneumonia. . . . . The mean duration of the inflammation even is less than when occurring in sound lungs. Some of the most marked examples of rapid resolution I have met with were in phthisical persons."[*]

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* I here refer more especially to the soothing and avoiding of local irritation.

It may be that this secondary pneumonia is usually less acute, less extensive, and consequently less severe, than primary pneumonia. Granting the fact, it by no means furnishes a reply to the fourth query; although, taken in connexion with the habitual non-inflammatory origin of tubercle, with the constitutional character of the disease, with the want of efficacy of antiphlogistics, and the superior utility of tonics and stimulants, in its treatment, it has no doubt assisted in giving rise to the opinion that inflammation is not a prominent source of mischief in phthisis.

If we analyse the particulars of the published cases in which an attack of pneumonia has been considered not to have proved prejudicial to the aftercourse of the pulmonary tubercles, we shall find some one of the following qualifying circumstances present. The cases occurred in hospital practice, and the patients only appeared no worse because they left the hospital immediately on their recovery from the acute attack. Or, the opinion has been founded on the exceptionable testimony of the patient's own history of his case. Or, the tubercles have been few, and in an early stage, and the constitution not gravely affected. Or, the inflammation has been limited to the base, the tubercles to the apex, of the lung inflamed. Or, the tubercles have been manifest only in one lung, and the pneumonia restricted to the other.[†]

But the inflammation of lung we have to consider in phthisis is rarely what would be styled an attack of pneumonia. Such attacks before the later stages are not customary; it is rather the repeated exacerbations of patches of chronic pneumonia, bronchitis, or pleurisy, from which, in its subdued creeping form, the patient is never entirely free. There is no evidence that these little inflammations are ever advantageous. There are abundant instances in which they are injurious. How many patients date the commencement, or first serious aggravation, of their consumption from some attack of what they call inflammation of the chest, influenza, pleurisy, sub-acute bronchitis, or pneumonia? Not that this proves the disease to have then originated, but it does prove that in the patient's own mind the thoracic inflammation bore an unfavourable relation to the phthisis. And the instances in private practice are innumerable in which an intercurrent exacerbation of previously slight pulmonic inflammation has clearly appeared to the medical attendant vastly to accelerate the destructive course of the tubercles. Add those far advanced cases in which an inflammation of the working remnant of lung proves the immediate cause of death, and we must conclude that, although in some cases of phthisis an acute attack of inflammation may seem to do no harm, yet there are far more in which its occurrence is fraught with ultimate danger.

A consideration of our means of diagnosis reminds us that we augur badly of the progress of pulmonary tubercles, entirely by the physical signs of inflammation and its consequences. Dulness and tubular breathing from inflammatory consolidation. Fine moist crackle, from inflammatory hypersecretion in the small bronchial tubes. Moist large sounds, gurgling, splashing, and the rest, from inflammatory destruction of lung-substance. Equally do the bad constitutional symptoms (hectic) point towards sympathy with a protracted inflammatory process.

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[*] Walshe on Diseases of the Heart and Lungs, second edition, p. 517.

[†] See Louis' "most remarkable illustration," case 43, p. 383. Walshe's translation, Sydenham Society's edition; and another of his in Gazette des Hôpitaux, p. 134. 1846.

From theoretical considerations, we should expect that when inflammation lingers around tubercles without any adequate exudation of plastic lymph, it must injure their chance of arrest. For it impairs the vitality of the inflamed texture; and this must be unfavourable to the sleep of tubercles, whose feeble vitality so largely depends on that of the adjoining tissues. There is greater heat around the tubercles; more chemical action going on. The tubercles are more likely to degenerate speedily; and speedy degeneration is synonymous with destructive softening. The inflamed tissue, irritated by the softening tubercles, is prone to undergo suppuration close around the tubercles, thus adding peripheric liquefaction to centric softening. There is now a vicious circle. The softening of the tubercle increases the inflammatory irritation; this in its turn quickens the softening. The fluid which results, unfit for absorption, irritates the vessels which refuse to imbibe it. Ulceration follows, and the work of destruction is fully established. And after ulceration it depends upon the amount of inflammation whether life is to be greatly protracted or speedily terminated.

Pathological observation, in support of this, teaches that all that is mischievous in the course of tubercles is inflammatory. That if no inflammation arises, or very little, tubercles have a natural tendency to soften so slowly that they soften safely, and undergo curative transformations. That, on the contrary, when attended by much inflammation, their softening is rapid, extensive, and dangerous.

If we now turn to the instances cited to prove that inflammation is curative in pulmonary tuberculosis, we find in every such case the inflammation has been merely of the plastic kind; that, namely, which occasions the effusion of a plasma which is fit for organization into a low form of fibrous texture. Now it will be admitted that this is the one form of inflammation which is closest to the process of normal nutrition. It is the first remove from health in the sliding scale of inflammation; it is that which occasions the smallest amount of irritation, whether local or constitutional; it is that which itself responds to the minimum of local irritation; and as a corollary to all this, it is that which is at once an effect and a proof of a sufficiently sthenic state of system, and a sufficiently small amount of local irritation.

It follows, considering what pulmonary tuberculosis is, that this is precisely that form of inflammation which in its pure shape is least common in phthisis; and which, when present, indicates an unusually favourable condition of both lung and constitution for the time being.

Does this benign plastic inflammation, when it affects a tuberculized lung, manifest itself by the ordinary symptoms of pneumonia? All the evidence goes to prove that it does not. It is a silent process, as much physiological as morbid, performed most perfectly when most unnoticed. Referring once more to the entozoic formations in the lungs of sheep, we may here see this plastic inflammation in its pure form unaffected by general cachexy.

It is reasonable to conclude that the nearer the inflammation which environs pulmonary tubercles in man can approach to this type, the better.

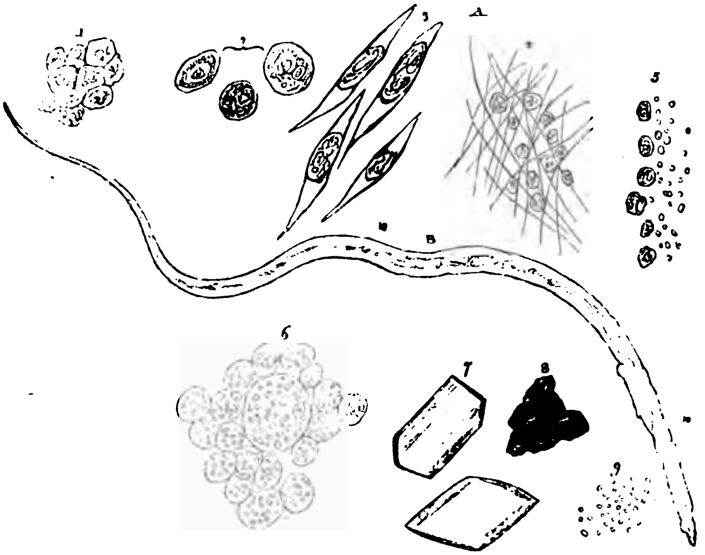

Fig. 31. Plastic Inflammation around Entozoic Formation in Lung of Sheep.

A. Wall of abscess, one-sixth of an inch thick; pink next to lung, pearly as it approached its lining membrane.
B. Contents of abscess, appearing to naked eye a greenish-yellow glucy jelly.

Proceeding from the lung through the wall into the centre of the abscess, we note in order—

1. Healthy lung-epithelium, not fatty.
2. Nucleated cells, forming the pink layer which separated lung from tougher portion of abscess-wall.
3. Large fibre-cells, forming outer layer of tough portion of abscess-wall.
4. Fibrillated network, with cells in its meshes.
5. Innermost layer of abscess-wall, made up of granule-cells and free fat, resting upon 4.
6. Mass of granule-cells, forming the bulk of the contents.
7. Crystals.
8. Amorphous calcareous matter.
9. Free fat.
10. Débris of worm.

Now there is nothing during the lifetime of the sheep to lead us to infer that it suffers pain, distress, or constitutional disturbance during the formation of this boundary of plastic inflammation around the nodules in its lungs.

We conclude that in phthisis, in exact proportion as we have external evidence of the existence of pulmonic inflammation, is such inflammation likely to depart from the plastic or good type. And consequently, that the only good inflammation is that of which both patient and physician are least conscious at the time.

And of such inflammation as declares itself by the ordinary signs and symptoms, in the words of Sir James Clark, "Pneumonic inflammation is one of the worst evils that can befal a patient already labouring under tuberculous disease of the lungs, as it never (*seldom*) fails to increase the mischief, and frequently converts that which was latent, and might have long remained so, into active disease."*

Whatever of inflammation is good, then, is a secret and natural reaction to the local irritation of the tubercles; is always sufficiently great for its purpose; and in the grand majority of cases errs by being excessive and overstepping the bounds of plasticity. In no case, therefore, are we justified in anticipating positive benefit from the supervention of accidental attacks of inflammation. On the contrary, we shall always best promote the benign form of inflammation by calming down, when present, and avoiding all avoidable causes of, the irritative form. *If* we *can* master the inflammation around tubercles, we can save our patient. When the patient dies, it is invariably because inflammation has mastered art.

Throughout these observations, reference has been made to the chronic form of phthisis only—to that which, insidiously undermining the system, creeps on for some time before it unequivocally declares its presence. This form, I think, I have rendered it not improbable, commences in its local shape with degeneration of the epithelium of the air-vesicles. I am aware that in the most acute forms of pulmonary tuberculization each tubercle commences as an exudation, without previous fatty degeneration of epithelium. These cases do not fall under notice in this locality, and my personal knowledge of the microscopic appearances is founded on the examination of two instances only.† Each case at first simulated typhus fever. Each presented tubercular deposits on the cerebral meninges, on the peritoneum, and in the liver and spleen. In each the lungs were highly congested, but for the most part crepitant, and strewn throughout with pin-head tubercles, most of which were firm, but some of only semi-solid consistency. In each were seen glomeruli of various sizes, abundant; simple tubercle-corpuscles, some nucleated; compound tubercle-cells, either not present or rare; no characteristic fatty epithelium, either in the tubercle or in the air-vesicles adjoining.

In chronic tuberculization, it is not assumed that fatty degeneration of epithelium is the precursor of tubercle in any other organs than the lungs and the lymphatic glands.

Of the practical inferences which flow from the preceding survey of the natural history of chronic pulmonary tubercle, considered merely as a local deposit, leaving untouched its constitutional character, I may mention the following.

For the prevention of the disease.—If spontaneous chronic consumption do originate by slow structural degeneration of the lung antecedent to the formation of actual tubercle, in predisposed persons, every attention should be paid to fairly develop the threatened organ by due and sufficient functional exercise of it.

If chronic consumption do also sometimes originate in an attack of inflammation, care must be taken not to carry functional exercise to the extent of inducing irritation of the lungs. The predisposed individual should also carefully avoid all avoidable risks of accidental thoracic inflammation.

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* On Pulmonary Consumption, p. 247. 1837.

† For one of which I was indebted to Dr. W. Budd, in 1849; for the other, to Dr. Brittan, in 1855.

For the management of the developed disease.—If when tubercles exist, whatever their stage, Nature is adequate to their cure, *provided there be time enough allowed,*—which implies that fatal allied disease elsewhere does not arise, that a check is put to the increase of tubercle, that inflammation is kept down, and the strength kept up,—for the successful treatment of every case of phthisis the indication is, to gain time and tone. The lungs must now be spared, by throwing extra work upon the liver and skin, care being taken to maintain these auxiliary organs of respiration in a fit state for duly discharging their vicarious increase of function. The signs of improvement need no comment. The criteria of want of success are only too familiar; a hot skin and quick pulse imply them all, for in phthisis, chronicity is comparative safety—acuteness is death.

ART. II.

The Rate of Pulsation and Respiration in Phthisis, and its Relation to Period of the Day, Posture, Temperature, &c.

By EDWARD SMITH, M.D., LL.B., L.R.C.P., Assistant Physician to the Hospital for Consumption and Diseases of the Chest, Brompton, &c.

THE following investigation extended over a period of four weeks, excluding Sundays, and the subjects were fifteen men, in-patients at the Hospital for Consumption and Diseases of the Chest, Brompton, in various stages of phthisis. The inquiry embraced the rate of pulsation and respiration in each of the three postures of lying, sitting, and standing, and at two periods of the day—viz., at 8 A.M., before food was taken, and before the wear of the day had begun, and at 4 P.M.

The rate of both was taken in half minutes, and error was avoided by, 1st, counting from a long line on the dial; 2nd, re-counting when the number was doubtful; 3rd, entering each result instantly; 4th, maintaining silence; 5th, not engaging the patients' attention; 6th, taking the patients always in the same order; and 7th, rigid punctuality.

At five minutes to the hour all the patients lay down, and at the hour I took the rate of both functions in the lying posture on the first patient, then on a second, third, and fourth. The first, second, &c., patient sat up as I proceeded, and had occupied that posture some minutes, when I returned to them to note the rate in the sitting posture. So in like manner with the standing posture; and by dividing the patients into three sets, the inquiry was not burdensome to any one.

Two sources of error were troublesome. 1st, That of indistinct breath-motion, from its shallowness; and 2nd, the influence of cough, or of attempts to repress it; but care and experience overcame them. I ascertained that cough excites the respiration more than the circulation—viz., to about 10 respirations, and 10 to 15 pulsations per minute from one fit of coughing; but in a few seconds afterwards the effect on both functions had subsided. As, however, the cough was frequent, it was not wise to continue each inquiry longer than half a minute, and in case 90 it was not possible to continue it so long in the lying posture.

The patients, for other reasons, were fed apart from others, but no further interference with their habits occurred.

The observations were upwards of 3000, and were continued in each case, with 7 exceptions, during the month; one case died during that period, and four others continued a part of the month. The quantity of food was ascertained with the greatest accuracy, and the weight of each patient in his shirt was obtained every third morning at a fixed hour, and with the same quantity of excretions within the body, as far as could be pre-arranged. All had softened tubercle, and most of them had cavities, but all were able to take plenty of exercise and food, and to sleep well.

A series of diagrams are appended, which show, 1st, the temperature of the wards and external air, and 2nd, the rate of pulsation and respiration in each posture, and in the mean of all the postures, both in each case, and on the total average of the whole of the cases.

I shall first consider pulsation, and then respiration; and under each head shall discuss the rate, 1st, absolutely, and 2nd, as influenced by various disturbing causes.

PART I. — PULSATION.

The Rate of Pulsation in Phthisis, considered absolutely.

Although the patients were living from day to day under precisely the same circumstances, the diagrams show that in scarcely any instance was the rate the same in any posture at the same moment on two consecutive days. The variation was often 10, 15, or 20 pulsations. The extremes of the rate were 55 and 166 per minute, the larger being three times the lesser number, and even in the same case the variation was 73 pulsations.

The total average was 95·3, and the average extremes were 76·1 and 128·4, or an average difference equal to the whole rate in a healthy tall man. The cases varied greatly in this average, as is shown in the following table, in which they are arranged in the order of frequency of rate.

TABLE I.

No. of case.	85	77	73	93	90	46	Total average.	80	79	107	88	58	95	69	75	51
Total average pulsation	128·4	112·5	104·6	99·8	97·5	96·9	95·3	94·9	92·7	92·6	91·3	89·5	86·4	84·1	83·2	76·1
		3 or ⅕.					7 or nearly ½.							5 or ⅓.		

Thus, the average rate in nearly half of the cases was between 90 and 100, whilst in one-fifth it was under 90, and in only one-third was it over 100. Only eight of the patients were under observation for the whole month, and of these one-third had the average rate below 90, and one-fifth above 100, and consequently the relation is nearly the same in the two classes of cases.

The rate is not dependent upon stage, nor upon the degree in which the system was implicated (although the case at the head of the list had the system more injured than any other), nor upon the degree of progress of the disease. Thus in the same disease, in every stage, and in every degree, there may be great differences in the rate of pulsation.

But before proceeding further, I purpose to consider each case in detail, so that the causes for the difference in the rate, both on the whole month and from day to day, may be sought for; and under each one will be stated his general condition, and that of the chest, with the progress as evinced by weight, pulsation, and quantity of food. They will be considered in the order above given.

Diagram No. 2. Case 85.—(Normal weight in clothes, 148 lbs.; vital capacity, 230 cubic inches. Hutchinson.) A pot-boy, aged twenty-three; 5 ft. 7 in. high; addicted to masturbation; ill ten months; much emaciated. Right lung: Dull down to sixth rib behind and before, with moist râles universally; minimum of mobility; only small patches of vesicular murmur, and very little respiration. Left lung: Dull extensively and chiefly behind; prolonged expiration, crepitation, lessened vesicular murmur, and lessened mobility. Heart, healthy. Respiration, 21 per minute (average). Thus, he breathed a little by one lung only, but as he objected to use the spirometer, we could not ascertain his vital capacity. (See Table II. p. 478.)

The weekly average rate of pulsation was 119·3, 124·1, 129·4, 140·6. His weight in pounds every third day, from May 14th, was 90¼, 89, 89, 88¼, 86¾, 86¾, 86, 83¾, and 85¾. Thus the rate of pulsation increased, and his weight decreased. He had also frequent attacks of epistaxis, and from the end of the third week was manifestly worse.

Diagram No. 3. Case 77.—(Normal weight in clothes, 145 lbs.; v. c. 214 c. i. Hutchinson.) A cab driver, aged eighteen; 5 ft. 5½ in. high; ill twelve months; greatly emaciated, and of lymphatic and nervous temperament. Right lung: Dull universally; no vesicular murmur anteriorly; bronchial respiration; prolonged expiration; clicks. Left lung: Great dulness universally, with moist râles; no vesicular murmur; cavity. Heart, pericardial friction-sound on pressure. Respiration, 30 per minute (average), and very shallow.

Table III.

Diet.	May		June							
	30	31	1	2	4	5	6	7	8	9
Solids (ounces)	20½	20½	15	22	22½	20	17½	17½	16½	17½
Meat ,,	4	4	2	4	3	4	4	4	4	4
Fluids ,,	47	47	50	47	47	52	69	54	54	54
Ale ,,	10	10	10	10	10	10	10	10	10	10
Wine ,,	...	...	...	...	...	...	2	2	2	2
Average pulsations	109	103	110	113	111	113	113	113	116	116

The weekly average rate was 110·8 and 114·2. His weight in pounds every third day, from May 30th, was 88, 87, 86½, 84¾, and 87. There was no complication, nor any marked change.

Diagram No. 4. Case 73.—(Normal weight in clothes, 120 lbs.; v. c. 182 c. i. Hutchinson.) A compositor, aged twenty-two, 4 ft. 9⅝ in. high; sanguine temperament; ill ten months, and much emaciated. Right lung: Dull universally; cavity; prolonged expiration; lessened vesicular murmur. Left lung: Dull universally, with moist râles; prolonged expi-

TABLE II. (Case 85, p. 477.)

Diet.	May																June							
	14	15	16	17	18	19	21	22	23	24	25	26	28	29	30	31	1	2	4	5	6	7	8	9
Solids (ounces)	...	13	21	18	21½	22	23½	16¼	20¼	18¾	23¼	20¾	18½	21¼	23¼	21¼	16¾	20¾	22¼	15	17½	21¼	18	16
Meat ,,	...	...	4	4	4	4	4	3	4	4	4	4	3	4	4	4	3	3	3	2½	4	4	6	3
Fluids ,,	...	59	49	59	58	68	58	42	46	48	48	48	41	52	42	42	49	53	42	27	47	58	47	67
Ale ,,	...	...	8	8	8	8	8	...	8	8	8	8	10	10	10	10	6	10	10	...	10	10	10	10
Average pulsation	113	119	117	126	123	117	130	126	123	114	122	128	131	128	128	123	127	138	144	137	141	140	137	142

TABLE IV. (Case 73, pp. 477—8.)

Diet.	May																June							
	14	15	16	17	18	19	21	22	23	24	25	26	28	29	30	31	1	2	4	5	6	7	8	9
Solids (ounces)	...	18¼	20¼	20¾	17½	18½	16½	17	21	19½	15¾	16¾	16½	17½	19½	19¼	21¼	19	20	22¾	18¼	18½	18¾	17¼
Meat ,,	...	3½	3	3½	3	4½	2	2½	4½	3	2½	2	2	2½	2½	3	2¾	3	2¼	3	4	3½	3½	3
Fluids ,,	...	60	60	65	65	55	45	58	65	45	57	80	70	64	64	56	54	55	54	61	56	75	75	64
Ale ,,	...	8	8	8	8	8	8	8	8	8	8	8	10	10	10	10	10	10	10	10	10	10	10	10
Wine ,,	...	...	...	...	...	...	...	...	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
Average pulsation	104	108	104	103	105	107	101	108	106	101	109	107	107	101	100	97	102	105	105	104	109	109	104	101

ration; lessened vesicular murmur; bronchial respiration. Heart, healthy. Respiration (average), 31. V. c. about 90 c. i. (See Table IV. p. 478.)

The weekly average rate of pulsation was 105·4, 104·4, 103·6, 105·2. His weight in pounds every third day was 86, 86¼, 85½, 86, 84¾, 85, 85, 84¾, 84¼, and 85½. There was no complication or marked change.

Diagram No. 2. Case 90.—(Normal weight in clothes, 162 lbs.; v. c. 230 c. i. Hutchinson.) A bookbinder, aged twenty-one; 5 ft. 8¼ in. high; ill two years; and lost two stones in weight. Right lung: Dull; cavity to third space, and another below; below this, no vesicular murmur; very little respiration; moist râles; minimum of mobility; great flattening. Left lung: Dull; flat; much less vesicular murmur and mobility; bronchial respiration; moist râles; prolonged expiration below the clavicle. Heart, healthy. Respiration (average), 25. V. c. from 80 to 86 c. i. (See Table V. p. 480.)

The weekly average pulsation was 97·8, 101·2, 97·8, 102·4. His weight in pounds every third day was 102¾, 103½, 103¾, 103½, 103½, 104, 104¼, 102¼, 102¾. There was a frequent and irritable cough, chiefly on lying down, but no marked change.

Diagram No. 3. Case 46.—(Normal weight in clothes, 155 lbs.; v. c. 232 c. i. Hutchinson.) A warder in a prison, aged thirty-four; 5 ft. 7¼in. high; lymphatic temperament; fretful; ill eighteen months; lost two stones in weight. Right lung: Dull over clavicle; softening; small cavity; and prolonged expiration at the apex; prolonged expiration and lessened vesicular murmur below. Left lung: The same, but with lessened respiration and vesicular murmur. Respiration (average), 23. V. c. 100 c. i. This is one of the less extensively diseased cases. He was indisposed to make exertion, or to take much exercise.

Table VI.

Diet.	May			June							
	29	30	31	1	2	4	5	6	7	8	9
Solids (ounces)	22¾	26	27	27	27	25½	28½	25½	27½	27	27
Fluids ,,	63	67	63	70	73	68	74	68	73	65	66
Porter ,,	10	10	10	10	10	10	10	10	10	10	10
Average pulsations	103	99	91	97	94	99	95	99	99	94	92

The weekly average pulsations were 98·5, 95·4. His weight in pounds every third day was 115, 113¾, 114¼, 114¼, 114¼. This is the only instance of diminished pulsation in the fourth week. There was no marked change.

Diagram No. 4. Case 80.—(Normal weight in clothes, 145 lbs.; v. c. 221 c. i. Hutchinson.) Servant, aged twenty; 5 ft. 5¼ in. high; spare and active; ill at intervals for years; but little emaciation. Right lung: Dull (moderately); small patches of softening. Left lung: Dull; great flattening; less mobility; cavity; and moist râles at the apex; cavity

TABLE V. (Case 90, p. 479.)

Diet.	May																June							
	14	15	16	17	18	19	21	22	23	24	25	26	28	29	30	31	1	2	4	5	6	7	8	9
Solids (ounces)	...	26	20	23¼	23¼	23½	27¼	20½	28½	27½	25½	23	21½	22	30½	26¾	24¾	34	30¼	34½	26¼	19¼	18½	18
Meat ,,	...	4	4	4	4	4	4	3	3½	3	3½	4	4¼	4	4	4	4	5½	3½	4	5	2	2	4
Fluids ,,	...	42	37	48½	39	43	53	39	54	45	46	50	47	34½	36	47	44	45½	43	39	43	51½	43	39
Ale ,,	...	...	...	...	...	...	3	6	6	...	...	2	6	3	...	8	...	3	5	6	...	4	7	...
Wine ,,	...	3	2	2	2	3	3	1	..	1	...	...	...	1	...	1	1½	½	...	1	1	2	1	...
Average pulsation	97	98	94	94	103	104	102	97	96	102	104	104	98	100	93	96	98	99	104	99	105	103	104	98

TABLE VIII. (Case 79, p. 481.)

Diet.	May																June							
	14	15	16	17	18	19	21	22	23	24	25	26	28	29	30	31	1	2	4	5	6	7	8	9
Solids (ounces)	...	23¾	28¾	28¾	27¾	21¾	27	27	24¾	24	23	12	9½	34½	37	37	37½	29½	24½	23½	24½	28	28	26½
Meat ,,	...	6	6	6	6	6	8	6	6	6	6	...	...	...	...	...	...	6	4	4	5	4	7	7
Fluids ,,	...	67	67	67	77	77	76	56	66	66	68	42	60	43½	55	49½	49½	55	75	60	60	75	65	64
Ale ,,	...	8	8	8	8	8	8	8	8	8	8	...	...	...	...	...	...	...	...	10	...	10	10	5
Wine ,,	...	...	...	...	...	...	...	...	...	...	...	...	...	...	2	2	...	2	2	2	2	2	2	2
Average pulsations	91	92	92	96	98	96	93	91	94	90	95	93	94	89	88	86	85	83	87	95	96	100	92	101

under the second rib; less respiration, but yet much vesicular murmur. Respirations (average), 21. V. c. 100 c. i. This, like No. 46, was also a case of less extensive disease.

TABLE VII.

Diet.	May								June							
	23	24	25	26	28	29	30	31	1	2	4	5	6	7	8	9
Solids (ounces)	31	25	27	26	22½	31½	29	29½	31½	32½	26½	27	32	29½	31½	29
Meat "	5	4	4	4	4	4½	4	5½	5	5	4	4	5	4	8	4
Fluids "	56	66½	48½	66	70	74	68	72	70	63	50	63	74	50	40	63
Ale "	8	8	...	8	10	10	10	10	10	10	10	10	6	...	...	5
Average pulsations	93	86	89	93	92	99	101	90	99	96	95	102	96	93	103	94

The weekly average pulsation was 90·4, 97·2, and 97·2. His weight in pounds every third day was 98½, 98, 99½, 97¼, 99¾, 99½, 99¾. There was no complication, nor any marked change.

Diagram No. 2. CASE 79.—(Normal weight in clothes, 155 lbs.; v. c. 224 c. i. Hutchinson.) A boatman, aged twenty-two; 5 ft. 7¾ in. high; quiet, lymphatic, spare; ill two years, and emaciated. Right lung: Dull, chiefly over clavicle, but moderately over the whole lung; moist râles in the upper lobe; no vesicular murmur above, and but little below. Left lung: prolonged expiration universally; slight moist râles. Heart, healthy. Very much albumen, lithates and oxalates, in urine. Respirations (average), 21. V. c. 76 to 80 c. i. (See Table VIII. p. 480.)

The weekly average pulsation was 94·4, 93·1, 88·1, 95·4. His weight in pounds every third day was 109¼, 109¼, 110½, 111½, 110¼, 110, 110, 111¼, 111½, and 113. He suffered much from diarrhœa in the second and third weeks, which lowered the pulsation; and upon the whole became more feeble, and it is probable that the disease of the lung advanced somewhat.

Diagram No. 4. CASE 107.—(Normal weight in clothes, 145 lbs.; v. c. 203 c. i. Hutchinson.) A publican, aged thirty-five; 5 ft. 5⅛ in. high; nervous temperament; had drunk freely; ill two years, and was emaciated. Right lung: Dull universally; cavity in the whole of upper lobe; moist râles below the fifth rib; no vesicular murmur. Left lung: Dull to fifth rib; bronchial respiration; no vesicular murmur. Heart, healthy. Respirations (average), 25. V. c. 70 to 80 c. i. (See Table IX. p. 482.)

The weekly average pulsation was 94·7, 94·8, 89·6, and 91·5. His weight in pounds every third day was 99½, 99½, 101¼, 101¾, 99½, 99, 100¾, 102¼, 102¼, 102¾. Thus, although this was an extensively diseased and advanced case, he gained much flesh, and his pulsation declined. He had much sickness in the second and third weeks, and kept his bed for a few days.

Diagram No. 3. CASE 88.—(Normal weight in clothes, 148 lbs.; v. c. 216 c. i. Hutchinson.) A shipwright, aged forty-five; 5 ft. 6¾ in. high; active, nervous, spare; ill eighteen months; emaciated. Right lung: Dull, down to fifth rib; cavity down to fourth rib; moist râles; no vesicular mur-

Table IX. (Case 107, p. 481.)

Diet.	May																June							
	14	15	16	17	18	19	21	22	23	24	25	26	28	29	30	31	1	2	4	5	6	7	8	9
Solids (ounces)	...	25¼	22	22¾	24	26¾	20½	16	20	16	9½	11½	21	18½	24	26¾	22½	25	27	24½	21	19½	28	21½
Meat ,,	...	7	6	4	4	6	4	4	6	4	...	...	...	6	5½	7½	4	6	5½	6	7	6½	6	6
Fluids ,,	...	107	97	74	84	97	77	85	99	78	75	77	110	82	92	93	93	85	95	82	67	117	59	89
Porter ,,	...	8	8	8	8	8	8	8	8	16	8	8	10	10	20	20	20	20	20	20	20	40	20	20
Wine ,,	...	3	3	3	3	3	3	3	3	3	3	3	3	2	...	...	...	...	...	...	...	...	...	...
Average pulsations	96	95	94	86	101	95	94	94	95	94	94	96	90	88	91	85	87	92	87	87	94	92	94	93

Table X. (Case 88, p. 481.)

Diet.	May																June							
	14	15	16	17	18	19	21	22	23	24	25	26	28	29	30	31	1	2	4	5	6	7	8	9
Solids (ounces)	...	26¾	24¼	23¼	21½	21½	24¾	26½	22	23	22	18	19	25	22¼	24	18¼	22½	21	19½	18¾	21	22	22
Meat ,,	...	7	4	4	4	6	4	6	6	6	6	4	4	6	7	7	4	5	5	6	6	6	6	6
Fluids ,,	...	51	48	48	48	68	51	38	66	48	58	56	48	47	61	57	47	47	61	62	46	52	52	52
Porter ,,	...	16	16	16	16	26	20	16	16	16	16	16	20	20	20	20	20	20	20	30	20	20	20	20
Average pulsations	87	88	84	94	93	101	88	84	87	85	93	97	98	87	91	82	91	94	88	97	93	101	88	94

mur, and but little respiration below. Left lung: Less dulness over clavicle; prolonged expiration, and lessened vesicular murmur. Heart, healthy. Respirations (average), 21. V. c. 76 to 100 c. i. (See Table X. p. 482.)

The weekly average pulsation was 88·6, 90·3, 92·5, 93·7. His weight in pounds every third day was 116¾, 117¼, 117¼, 115¾, 116, 116, 116, 117, 115, 115¾. He had the complication of troublesome hæmorrhoids from constipation. His pulse quickened somewhat, and although there was no marked change, the impression upon the mind was that the disease was somewhat advancing.

Diagram No. 3. Case 58.—(Normal weight in clothes, 145 lbs.; v. c. 215 c. i. Hutchinson.) A carpenter, aged thirty-one; 5 ft. 5½ in. high; lymphatic temperament; ill ten months; but little emaciated. Right lung: Dull universally; prolonged expiration; moist sound, on full inspiration, down to second rib; lessened vesicular murmur. Left lung: Dull; less respiration; prolonged expiration; pleuritic creaking. Heart, healthy. Respirations (average), 26. V. c. 100 c. i. There was extensive consolidation, with but very little destruction, and it was one of the less advanced cases.

Table XI.

Diet.	May											
	14	15	16	17	18	19	21	22	23	24	25	26
Solids (ounces)	...	29¾	30	33¼	30¾	33½	29½	31¾	...	32¾	32¾	26¾
Meat ,,	...	4	4½	6	6	6	5	6	...	8	5½	3½
Fluids ,,	...	76	86	96	96	96	86	56	...	76	56	76
Ale ,,	...	8	8	8	8	8	8	8	...	8	8	8
Average pulsations	88	93	88	89	95	92	91	94	90	82	87	88

The weekly average pulsation was 90·8 and 88·3. His weight in pounds every third day was 110½, 112¼, 113¼, 115½, and 114½. Thus he increased in weight, and his pulsation diminished, and upon the whole he improved.

Diagram No. 4. Case 95.—(Normal weight in clothes, 148 lbs.; v. c. 216 c. i. Hutchinson.) A farmer, aged forty-three; 5 ft. 6½ in. high; ruddy; ill twelve months, and had lost a little flesh. Right lung: Large cavity, with flattening and immobility to third rib; no respiration at apex, and little below, to third rib; below fourth rib, prolonged expiration; clicks, and moist râles. Left lung: Only harsh breathing in the third intercostal space. Heart, healthy. Rose early; took much exercise. Respirations (average), 23. V. c. 130 c. i. This was the only instance having one lung sound. His system was almost uninjured. (See Table XII. p. 484.)

The weekly average pulsation was 84·9, 85·6, 86·8, and 88·6. His weight in pounds every third day was 135½, 136½, 135¼, 135¼, 135½, 134½, 136¾, 134¾, 134¼, 135¾. There was no complication, nor any change for the worse.

Diagram No. 2. Case 69.—A Life Guardsman, aged twenty-six;

TABLE XII. (Case 98, p. 483.)

Diet.	May																June							
	14	15	16	17	18	19	21	22	23	24	25	26	28	29	30	31	1	2	4	5	6	7	8	9
Solids (ounces)	...	29¾	32¾	34½	37	38	29½	27½	23½	37	33½	27½	31¾	32	32	34¼	30	30	32	30	31¼	33½	42½	35
Meat ,,	...	4	4	4	4	6	4	4	4	5½	8	4	7¾	6	6	8¾	6	8	10	7½	6	8	8	6
Fluids ,,	...	56	56	76	76	56	76	56	76	66	76	76	80	82	62	77	62	52	79	59	72	74	85	80
Ale ,,	...	8	8	8	8	8	8	8	8	8	8	8	10	10	10	15	10	10	7	7	10	...	3	10
Average pulsations	79	85	88	88	83	94	83	85	85	86	86	89	87	82	86	91	91	88	88	86	89	89	88	90

TABLE XIII. (Case 69, pp. 483—5.)

Diet.	May																June							
	14	15	16	17	18	19	21	22	23	24	25	26	28	29	30	31	1	2	4	5	6	7	8	9
Solids (ounces)	...	43½	43½	40½	43¾	43½	49	42¾	48¼	46	44½	43½	45	45½	47	47½	34¼	...	37½	20	36½	38¼	38¼	36
Meat ,,	...	9	9	6	6	6	10	6	11	8	6	6	6	6	6	6	2	...	6	2	7½	11	7	6
Fluids ,,	...	127	129	119	117	117	97	126	66	126	106	106	122	80	106	86	79	...	116	101	108	74	90	80
Ale ,,	...	8	8	8	8	8	8	8	8	8	8	8	20	30	30	20	20	...	20	40	34	20	20	20
Average pulsations	77	79	77	85	85	82	78	85	78	78	76	86	79	81	84	80	91	98	90	91	89	87	84	85

6 ft. 1¾ in. high; ill twelve months, and had lost flesh, although still weighing twelve stone. Right lung: Bronchial respiration, and greatly lessened vesicular murmur to fifth rib. Left side: Dull universally; very little respiration or vesicular murmur; clicks. Heart, healthy. Respirations (average), 16. V. c. lessened during the month from 150 to 137 c. i. This was one of the less advanced and less injured cases. (See Table XIII. p. 484.)

The weekly average pulsation was 81·4, 81·, 85·9, and 88·2. His weight in pounds every third day, from May 14th, was 167¾, 169¾, 172, 171¾, 172½, 173½, 174, 169, 172¼, 171. Thus his weight increased until the end of the third week, when he took cold, and had fever, cough, loss of appetite and flesh, and his left lung then presented moist râles extensively. He gained ground at first, but more than lost it afterwards, and his disease undoubtedly progressed.

Diagram No. 3. Case 75.—(Average weight in clothes, 145 lbs.; v. c. 209 c. i. Hutchinson.) A servant, aged twenty-five; 5 ft. 5½ in. high; very lymphatic and sluggish temperament; ill twelve months, and was emaciated. Right lung: Dull, and humid râles to second rib; pleuritic sounds below; very little vesicular murmur. Left side: prolonged expiration, and lessened vesicular murmur universally; pleuritic sounds. Heart, healthy. Respirations (average), 17. V. c. 130 c. i. Thus the diminution of vital capacity and the extent of mischief were less than in almost any other of the cases.

Table XIV.

Diet.	May											
	14	15	16	17	18	19	21	22	23	24	25	26
Solids (ounces)	...	25	22	21	20	21	22	26	20	24	20	20
Meat ,,	...	6	4	4	5	4	5	6	4	4	4	4
Fluids ,,	...	59	59	69	59	59	67	57	57	57	57	57
Porter ,,	...	8	8	8	8	8	8	8	8	8	8	8
Average pulsations	89	83	81	78	82	80	84	88	79	83	82	88

The weekly average pulsation was 82·1 and 84·3. His weight in pounds every third day, from May 14th, was 100½, 100¼, 100½, 100¾, 101¼. There was a remarkable diminution of pulsation in the 1st week, and the increase in the second, which was found in most of the cases; but there was nothing remarkable in the progress of the case.

Diagram No. 4. Case 51.—(Average weight 139 lbs.; v. c. 188 c. i. Hutchinson.) An omnibus conductor, aged fifty-one; 5 ft. 4 in. high; ill thirteen months, and much emaciated. He passed large quantities of renal casts, and some albumen. Right lung: Dull to second rib; prolonged expiration, and moist râles, on deep inspiration, to fifth rib; very little vesicular murmur. Left lung: Dull on the clavicle, and few humid râles to the second rib. Heart, healthy. Respirations (average), 22½. V. c. 120 c. i. Thus, except for the renal complication, the case was not greatly advanced, nor very extensive.

TABLE XV.

Diet.	May					
	14	15	16	17	18	19
Solids (ounces)	...	14½	13¼	15¼	15¾	14¾
Meat „	...	4	...	...	...	...
Fluids „	...	79	87	97	107	107
Ale „	...	8	8	8	8	8
Average pulsations	93	81	71	71	72	68

The weekly average pulsation was 76·1. His weight in pounds every third day, from May 14th, was 86¼, 84½, and 86¼. This case shows a diminution of pulsation, in one week, of the most remarkable character; and one due, probably, to emotional influences.

The foregoing analysis will enable us to determine, in each case, the stage of the disease, its extent, the degree and rapidity of progress, and the effect of changes in the daily quantity and quality of food; and thus we should be further enabled to determine various questions in reference to pulsation and respiration.

The following comprehensive table contains the elements of the inquiry (deduced from the foregoing analysis) in reference to each case, the cases being arranged in the order of *probable* extent of disease, beginning with the most extensive. (See Table XVI. p. 487.)

There is throughout the whole table a want of entire uniformity; but the cases may be divided into two categories of extensive and limited disease, by drawing a line after No. 107. There is some doubt as to the position of the following case, or the seventh on the list; but as one lung was sound, and as I have remarked that the system seemed to suffer less when one lung only was diseased than when both lungs were affected to a less extent, and as he had quite the aspect of a healthy man, I have included him in the lower division.

From this table the following deductions may be made:

1. In the upper category, viz., Nos. 90, 88, 73, 77, 85, and 107, the disease was very extensive and advanced, and much more so than in the lower category.
2. The general system was much more implicated in the cases of the upper than of the lower category.

The more extensive and advanced cases of disease differed from the others in the following particulars:

1. Fewer patients were of the non-excitable and sluggish temperament.
2. The existing vital capacity was less.

With one exception (No. 79) the less advanced cases had a vital capacity of 100 c. i. and upwards.

There are two modes of estimating the value of any existing vital capacity—1st, as above, by the number of cubic inches remaining absolutely; and 2nd, by regarding the deficiency between the existing and

TABLE XVI. *Showing the relation of the degree in which the lungs were diseased to various questions, and especially of the pulse and respiration.*

—	No. of Case.													
	90	88	73	77	85	107	95	46	80	69	79	58	75	51
Consolidation right	very great	v. great	very great	great	v. great	very great	mod.	little	little	little	great	great	moderate	mod.
Consolidation left	extensive	mod.	very great	very great	great	great	...	moderate	mod.	great	little	great	very little	little
Destruction right	very great (cav.)	v. great (cav.)	cav.	mod. extensive.	mod. extensive.	very great (v. l. cav.)	v. great (lit.cav.)	mod. (cav.)	little	...	mod.	very little	little	little
Destruction left	mod.	...	mod. extensive.	(cav.)	mod. extensive.	...	...	mod. (cav.)	mod. large cavity.	mod.	...	...	...	v. little
Height	5·8¼	5·6¾	4·9¾	5.5½	5·7	5·5¾	5·6½	5·7½	5·5½	6·1¾	5· 7¾	5·5½	5·5½	5·4
Weight	7·4¾	8·4¾	6·2	6·4	6·6	7·2	9·9¾	8·3	7·	12·	7·11½	8·	7·2½	6·2
Temperament	medium	v. excit.	v. excit.	excit.	excit.	v. excit.	ordinary	not excit.	ordinary	ordinary	excit.	not excit.	v. sluggish	ord.
Activity of body	medium	active	active	quiet	active	active	v. active	slow	active	active	rather slow	rather slow	very slow	rather slow
Vital capacity in cubic inches	83·	76–100	90	(very small.)		70–80	130	100	100	130–140	80	100	130	120
Total average Respiration	24·7	18·1	31·	29·7	20·6	24·9	22·6	22·7	20·6	16·	20·6	25·6	17 as low at 11	22·4
Pulsation. Total average Pulsation	97·5	91·3	104·6	112·5	128·4	92·6	86·4	96·9	94·9	84·1	92·7	89·5	83·2	76·1
Difference between extremes	39	58	42	44	73	52	51	52	57	38	50	43	39	49
Difference of daily average	12	17	9	7	31	14	15	12	17	?	18	13	11	15
Increase or decrease of weekly averages	incr. 4·8	incr. 5·1	stationary	incr. 3·4	incr. 21·	decr. 3·2	incr. 3·7	decr. 3·1	incr. 6·8	incr. 6·8	incr. 1· variable	decr. 2·5	incr. 2·2	dec.
Progress	none	mod.	none	none	great	moderate	none	mod.	none	mod.	mod.	none	none	none
Time under observation	month	month	month	nearly fortnight	month	month	month	nearly fortnight	nearly 3 weeks	month	month	fortnight	fortnight	week

the normal vital capacity. If we adopt Dr. Hutchinson's results, the deficiency will be indicated in the following table:—

TABLE XVII. *Showing the cases arranged in the order of diminished vital capacity.*

Case.	Healthy standard. Cubic inches.	Existing vital capacity. Cubic inches.	Amount of diminution. Cubic inches.	Amount of diminution relative to health. Cubic inches.
90	239	80 to 86	153	more than $\frac{3}{5}$ths
79	224	80	144	more than $\frac{3}{5}$ths
88	206	76 to 100	126	$\frac{3}{5}$ths
107	203	80	123	$\frac{3}{5}$ths
46	232	100	132	$\frac{3}{5}$ths
80	221	100	121	$\frac{4}{7}$ths
58	215	100	115	more than $\frac{1}{2}$
73	182	90	92	$\frac{1}{2}$
51	188	120	68	more than $\frac{1}{3}$rd
75	209	130	79	ditto
95	216	130	86	$\frac{2}{5}$ths

No admeasurement of the vital capacity could be made in cases 85 and 77.

If all the elements in this inquiry could be determined with accuracy, there can be no doubt as to the preference which ought to be given to this last mode of determining the question. The arrangement in the above table is not precisely that determined by the table preceding.

3. The average respiration was quicker, as 24·8 in the former to 20·9 in the latter, notwithstanding the exceptional low state of respiration in case 88.

4. The average pulsations were greater, as 104·5 in the former to 88·1 in the latter.

5. The extremes of pulsation, both absolutely and on the average, were somewhat greater.

It would be interesting to determine (in relation to these deductions) the value of very extensive consolidation, as opposed to less extensive consolidation combined with destruction; and that of one lung greatly diseased with the other sound, as opposed to less extensive disease affecting both lungs. The subject is one of insuperable difficulty, from the impossibility of measuring these conditions with accuracy; but I am of opinion that the system suffers more with moderate consolidation and destruction than with more extensive consolidation alone, and with both lungs moderately diseased than with one more extensively diseased and the other perfect.

6. There is no constant relation between lessened vital capacity, or frequency of pulsation (each considered apart), and the variations of respiration and the difference between the two extremes.

DISTURBING INFLUENCES.

A. *Period of the Day.*

The examinations were made at eight A.M. and four P.M. The total average of 792 morning and 714 evening examinations was 91·2 pulsations for the morning and 98·4 for the evening. The average evening excess was thus seven pulsations, but it varied from nothing to fourteen pulsations. The absolute extremes of pulsation were, in the morning 65 and 143·6, and in the evening 70 and 152.

The following table exhibits the averages in each case, arranged in the order of amount of evening excess.

TABLE XVIII.

Case...............	73	46	88	77	80	58	93	Total aver.	107	85	51	79	90	75	69	95
Aver. pulsation:																
Morning	96·6	90·2	85·2	106·6	89·4	85·7	99·8	91·2	89·2	125·	73·1	89·8	97·1	80·8	82·1	87·1
Evening	110·3	103·7	97·3	118·4	100·4	93·5	103·6	98·4	96·	131·6	79·	95·6	102·5	85·7	85·6	85·6
Evening excess	13·7	13·5	12·1	11·8	11·	7·8	7·6	7·2	6·8	6·6	5·9	5·8	5·4	4·9	3·5	-1·5

Thus, in only one case was there no evening increase, and that was an active ruddy farmer, with one lung nearly sound. The remaining fourteen are divisible into two classes, one of which has an average increase above, and the other below, seven pulsations.

This evening excess is commonly associated with, or dependent upon, two conditions—1. Extent of lung disease; 2. Frequency of pulsation.

1. *Relation with extent of Lung Disease.*—This is seen by referring to the order of the cases in the above table with that of Table XVI., and comparing the upper half of each table with each other. Nos. 73, 88, and 77 are found in both tables, and to these must be added No. 93, who was so much diseased that he died from pneumo-thorax during the inquiry. Thus four out of seven cases agree in great extent of disease and great evening excess of pulsation. Nos. 46 and 80 in the last table appear exceptional, and perhaps that may be explained by stating, that although the disease was not extensive, both the lungs were diseased, and in each case there was one or more cavities. No. 90 was much diseased, and yet appears at the middle of the lower half of the table, so that the rule does not always apply.

2. *Relation with Rapidity of Pulsation.*—The relation seems to be in the same degree as that of rapidity of pulse to extent of disease. Thus the totals of the average pulsations in the 7 upper cases in the last table, or those having an evening excess of more than 7 pulsations, are greater than those of the 7 below, as follows.

	Morning pulsation.	Evening pulsation.
7 upper cases	653	727·2
7 lower cases	637	676·

Had it not been for the exceptional position of case 85 in the lower list, this difference would have been very striking.

There are also exceptions to this law. Thus No. 88 in the upper list has not a rapid pulsation, whilst No. 90 has rapid pulsation, and is in the lower list. Our inability to measure with absolute accuracy the extent of mischief may perhaps be the explanation. The converse of the rule does not hold.

TABLE XIX. *Showing the weekly evening excess of pulsation.*

No. of case.	First week. May 15 to May 19. Pulsation.				Second week. May 21 to May 26. Pulsation.				Third week. May 28 to June 2. Pulsation.				Fourth week. June 4 to June 9. Pulsation.				Total Pulsation.	
	Morn.	Even.	Evening excess.	No. of days of evening excess.	Morn.	Even.	Evening excess.	No. of days of excess.	Morn.	Even.	Evening excess.	No. of days of excess.	Morn.	Even.	Evening excess.	No. of days of excess.	Weeks with evening excess.	Days with evening excess.
73	98·	112·8	14·8	6/6	99·1	109·	10·9	6/6	94·1	113·2	19·1	5/5	95·2	115·3	20·1	6/6	all	23 all
88	85·3	91·9	6·6	5/6	84·6	96·	12·4	5/5	84·6	100·4	15·8	5/5	86·4	101·	11·6	6/6	,,	$\frac{21}{22}$
85	115·8	122·7	6·9	·5/6	123·1	125·2	2·1	4/6	124·6	134·2	9·6	5/5	136·8	144·4	7·6	6/6	,,	$\frac{20}{23}$
46	...	...	...	...	...	...	...	...	91·4	105·8	14·4	4/4	89·1	101·6	12·5	6/6	,,	10 all
93	96·	103·6	7·6	4/5	...	...	...	...	...	...	...	...	...	...	...	...	,,	$\frac{4}{5}$
77	...	...	...	...	...	...	...	...	104·2	117·5	13·3	3/3	109·	119·4	10·4	6/6	,,	9 all
80	...	...	...	...	84·3	96·6	12·3	4/4	92·5	102·	9·5	5/5	91·6	102·8	11·2	6/6	,,	15 all
58	86·5	95·1	8·6	4/6	85·	91·9	6·9	4/5	...	...	...	..	...	...	...	...	,,	$\frac{8}{11}2 =$
107	90·	99·5	9·5	5/6	91·	98·6	7·6	5/6	88·3	91·	2·7	4/5	87·8	95·2	7·4	5/5	,,	$\frac{19}{22}2 =$
51	73·1	79·	5·9	5/6	...	...	...	...	...	...	...	...	...	...	...	...	,,	$\frac{5}{6}$
79	89·	99·8	10·8	6/6	90·	96·	6·	4/6	89·6	86·6	—3·	2/5	90·6	100·2	9·6	5/6	3/4	$\frac{17}{23}$
90	93·6	102·	9·4	6/6	99·7	102·7	3·	5/6	95·6	100·2	4·6	3/5	99·6	105·1	5·5	5/6	all	$\frac{19}{23}$
75	80·5	83·7	3·2	5/6	81·1	87·5	6·4	6/6	...	...	...	...	...	...	...	...	,,	$\frac{11}{12}$
69	81·1	81·7	6·	3/6	80·8	81·1	—·7	3/6	81·9	88·9	7·	4/4	84·7	91·7	7·	4/6	,,	$\frac{14}{22}$
95	84·6	85·2	6·	4/6	86·7	84·6	—2·1	4/6	89·2	84·4	—4·8	2/5	88·	89·2	1·2	4/6	2/4	$\frac{14}{23}$

3. Activity of habit does not account for the evening excess, for No. 46 was sluggish, and is yet found in the upper class; and Nos. 69 and 95 were very active in and out of doors, and yet one is in the lower class, and the other has no excess.

4. This evening excess corresponds somewhat with the temperature of the day, and it will be shown that temperature does increase pulsation when acting at considerable intervals, as of a day.

5. *The Relation of Evening Excess of Pulsation to the Progress of the Disease.*—To this end it is needful to examine the excess from week to week, and also from day to day, as is effected in Table XIX. (p. 490).

Thus every case (except two, Nos. 79 and 69) having a total excess had an excess in each week. No. 95, which had no total excess, had an excess in two of the four weeks. In 2 cases the excess was notably lessened, and in 1 it was increased. Thus No. 79 had a diarrhœa, and No. 107 had vomiting, and in both the pulsation was lowered and the evening excess lessened; whilst No. 69 had an attack of inflammation, and the pulsation and evening excess were increased.

There is a general relationship between increasing and decreasing pulsation and increase or decrease of the evening excess, and especially between the evening increase or decrease and the excess; but until the average pulsation amounts to 90 per minute, the evening excess is but small. The one is not a measure of the other.

Apart from this change in pulsation, there does not appear to be any relation between the increase of the evening excess and the progress of the disease.

B. *Posture of the Body.*

This influence is great, and capable of tolerably exact definition.

On the total average in 1500 observations the pulsation was as follows: lying, 87; sitting, 95·5; and standing 104·1; showing an excess of sitting over lying of 8½ pulsations, and of standing over sitting of 8·6 pulsations, or a total excess of 17 pulsations in the standing over the lying posture. The excess in the sitting is just midway between that of the standing over the lying posture, and might therefore be the one to be selected by writers when describing pulsation. The extremes of pulsation in the three postures were very great. Thus lying, 55 and 138; sitting, 62 and 150; and standing, 75 and 166. The greatest excess recorded in one day was 29 in the sitting over the lying posture, 32 in the standing over the sitting, and 44 in the standing over the lying posture. Thus, in some cases of phthisis the mere excess of pulsation from posture of body much exceeds the half of the total pulsations in health.

Table XX. (p. 492) shows the increase in each of the cases, the cases being arranged in the order of greatest increase in the standing posture.

Thus, in only one trifling instance was there no excess, and that instance is remarkable as offering the precise number of pulsations in the two postures of lying and sitting. The diversity was very great—viz., from 8 to 23⅓ pulsations in the standing over the lying posture. The pulsation was highest in the standing posture in every case. The amount of increase in the sitting over the lying posture was greater than in the standing over the sitting posture in 11 of the 15 cases, as 130·6 are to 125·9. The exceptions were Nos. 79, 58, 90, and 69.

TABLE XX. *Showing the average increase of pulsation in each case and in each position.*

Number of Case	88	79	95	85	77	75	93	58	46	80	107	51	73	90	69
Excess:															
Sitting over lying	14·6	9·6	11·	11·	12·1	10·2	7·4	8·	9·3	9·7	8·2	7·5	5·5	3·4	...
Standing over sitting...	8·7	12·7	10·3	9·8	8·4	9·2	11·6	10·5	8·3	7·8	6·3	6·1	4·9	5·7	8·2
Standing over lying ...	23·3	22·3	21·3	20·9	20·5	19·4	19·	18·5	17·6	17·5	14·5	13·6	10·4	9·6	8·2

The variation in this excess from posture is not accounted for by age, temperament, amount of lung disease, frequency of pulsation, or height of body, all of which comparisons may be made by reference to the preceding tables.

On the weekly averages, the pulsations in the sitting posture exceeded those in the lying, except in the third week in No. 69. The amount of the excess varied from 1 to 17·6 pulsations, and in the same case in consecutive weeks the increase varied sometimes to the extent of nearly 8 pulsations, whilst in other cases it was absolutely stationary. The increase in the standing over the lying and sitting postures was universal, and had extremes so wide as 2·3 and 15·9 pulsations in the latter, and 7·1 and 28·1 in the former, whilst the weekly variation was from 0· to 10 pulsations. Thus the amount and progress of variation in one position is no guide to determine the like in other positions, and no case exhibited any peculiar characteristic equally in all the positions.

The average pulsation in the lying posture was almost identical with the mean of all the postures. Thus, in three instances it was identical to a decimal point, and in twenty-eight of forty-five weeks the variation was not more than 1 pulsation. In one-half of the cases the pulsations in the sitting posture were slightly higher than the mean pulsations, and chiefly in the cases having the greatest excess in the standing posture.

The daily returns present much greater diversity. In only one case (No. 46) were the increased pulsations in the sitting and standing postures found in every day's examination. Of a total of 502 observations, 443 (or seven-eighths of the whole) had an excess in the sitting over the lying, and 469 an excess in the standing over the sitting postures. Thus, the excess in the standing is a little more constant than in the sitting posture.

The Influence of Period of the Day over this Increase.—This influence is much greater than could be inferred by reference to the disturbing causes of the day, such as exertion, anxiety, food, and temperature. The common effect was to increase the pulsations, and Table XXI. (p. 493) shows the number of times in which that increase was observed in each posture and in each case, compared with the total number of observations in each case.

Thus, in only 1 case did the evening pulsations in all the positions exceed those of the morning. In the lying posture, 4 obeyed the rule in every examination, whilst there were only 2 in the sitting and 1 in the standing posture. Regarding the whole as one series of 238 observations, 208 had an evening excess in the lying, 201 in the sitting, and 173 in

TABLE XXI. *Showing the total number of observations in each case, and the number of times in which an increase was observed in each posture.*

Number of Case	90	95	79	73	85	88	69	107	80	75	58	46	77	51	93
Lying	17	11	16	23	20	22	15	20	14	11	10	10	9	5	5
Sitting	17	13	17	23	19	21	13	18	13	11	10	10	8	4	4
Standing	13	7	16	23	12	20	14	17	14	6	6	9	8	4	4
Number of evening observations	23	23	23	23	23	22	22	22	15	12	11	10	9	6	5

the standing posture. Thus, whilst the rule is established, it is much less frequently obeyed in the standing and sitting postures; and this is best illustrated by case 95, in whom, on the total average, there was no evening excess.

What is the reason for the lessened constancy in the evening increase in the standing posture? In the morning the heart is vigorous, and enabled to propel the blood in opposition to gravity; but as the day wears away the body wears too, and the heart's action in a measure fails, and the failure will be first seen in that position which calls for the greatest power of the heart—viz., the standing posture. Hence probably this might be employed as a test of the progress of hourly wear and tear.

The extent of this increase on the average and in the extremes in each case is given in Table XXII. (p. 494.)

Thus, in the lying posture the average excess varied from 2 to 22 pulsations, in the sitting posture from 1 to 17 pulsations, and in the standing posture from 0 to 12 pulsations. The relative excess of the sitting and standing postures is less than in the morning, and this may be owing either to a disproportionate increase in the lying or decrease in the standing postures. If we add together all the pulsations in each position for the morning and the evening separately, and note the difference (as in the following table), we shall find that, taking the sitting posture as a medium, it is chiefly owing to the lessened elevation in the standing posture.

TABLE XXIII.

	Morning.	Evening.	Difference.
Total excess of sitting over lying	148·3	107·9	40·4
,, ,, standing over sitting	144·9	102·7	42·2
,, ,, ,, lying	293·2	200·6	82·6

The same fact is well illustrated in the next table, which shows the amount of morning excess in the increase in the standing posture, the cases being arranged in the order of the evening excess of pulsation.

TABLE XXIV.

Case	79	88	95	75	93	77	58	80	85	46	107	51	90	69	73
Excess, even.	22·	21·6	19·8	16·8	15·9	15·8	14·7	13·8	13·4	12·5	12·1	9·6	6·5	6·5	5·7
,, morn.	22·5	25·1	22·8	21·9	22·	25·2	22·2	12·1	24·4	22·7	16·8	17·6	12·8	9·9	15·2
Incr. of morn. ex. over even.	·5	3·5	3·	5·1	6·1	9·4	7·5	...	11·	10·2	4·7	8·	6·3	3·4	9·5

TABLE XXII. *Showing the average and extreme pulsation in each case in the morning and evening, and in the three postures.*

Case.	—	Total weekly average.	Lying.			Sitting.			Standing.			Excess of					
			Med.	Extrem. in one day.		Med.	Extrem. in one day.		Med.	Extrem. in one day.		Sitting over lying.	Greatest in one day.	Standing over sitting.	Greatest in one day.	Standing over lying.	Greatest in one day.
85	Morn.	125·	113·1	93	134	124·6	104	142	137·5	112	162	11·5	19	12·9	32	24·4	42
	Even.	131·6	122·1	110	138	132·7	114	150	139·5	120	166	10·6	20	6·8	28	17·3	44
77	Morn.	106·6	93·1	90	98	108·8	102	118	118·3	111	122	15·7	24	9·5	13	25·2	28
	Even.	118·4	110·3	105	114	118·8	114	125	126·1	122	134	8·5	14	7·3	12	15·8	23
73	Morn.	96·6	88·9	82	90	96·8	84	111	104·1	98	114	7·9	18	7·3	23	15·2	22
	Even.	110·3	110·5	106	122	113·6	104	122	116·1	100	129	3·1	10	2·5	10	5·6	16
90	Morn.	97·1	90·7	78	104	97·	86	110	103·5	92	112	6·3	16	6·5	18	12·8	20
	Even.	102·5	100·	83	110	101·6	95	108	106·5	96	117	1·6	25	4·9	14	6·5	14
93	Morn.	96·	85·8	78	92	94·6	90	100	107·8	98	116	8·8	13	13·2	24	22·	32
	Even.	103·6	96·1	92	108	102·	96	116	112·	98	130	5·9	8	10·	16	15·9	32
46	Morn.	90·2	78·5	68	85	90·9	88	94	101·2	94	106	12·4	24	10·3	16	22·7	32
	Even.	103·7	97·5	89	100	103·8	96	112	110·	102	120	6·3	8	6·2	12	12·5	18
80	Morn.	89·4	78·5	65	91	90·4	80	104	99·6	90	110	11·9	21	·2	18	12·1	32
	Even.	100·4	93·2	79	112	100·7	94	114	107·	97	122	7·5	17	6·3	15	13·6	18
79	Morn.	89·8	79·3	72	98	88·4	74	104	101·8	91	110	9·1	29	13·4	28	22·5	30
	Even.	95·6	85·	74	96	95·	81	118	107·	94	102	10·	24	12·	30	22·	41
107	Morn.	89·2	80·	73	90	90·4	81	98	96·8	85	106	10·4	20	6·4	18	16·8	33
	Even.	96·	90·1	80	98	96·	87	116	102·2	93	114	5·9	18	6·2	15	12·1	22
88	Morn.	85·2	71·6	64	80	86·7	77	98	96·7	85	112	15·1	24	10·	23	25·1	40
	Even.	97·3	85·4	74	109	99·6	82	118	107·	90	132	14·2	19	7·4	18	21·6	36
58	Morn.	85·7	77·6	65	88	86·5	80	93	99·8	83	104	8·9	15	13·3	19	22·2	30
	Even.	93·5	86·1	80	94	93·1	84	102	100·8	92	108	7·	14	7·7	16	14·7	18
95	Morn.	87·1	76·	57	83	86·6	74	96	98·8	84	108	10·6	17	12·2	21	22·8	31
	Even.	85·6	76·2	68	88	87·7	78	96	96·	86	106	11·5	18	8·3	17	19·8	30
69	Morn.	82·1	78·6	66	97	79·3	68	97	88·5	80	102	7·	8	9·2	27	9·9	24
	Even.	85·6	84·1	70	98	83·4	70	92	90·6	75	104	—·7	9	7·2	20	6·5	17
75	Morn.	80·8	69·9	63	80	80·6	78	96	91·8	76	100	10·7	19	11·2	16	21·9	35
	Even.	85·7	76·8	64	92	86·5	74	96	93·6	84	102	9·7	18	7·1	12	16·8	24
51	Morn.	73·1	64·5	55	86	72·8	62	92	82·1	76	98	8·3	19	9·3	14	17·6	21
	Even.	79·	73·5	62	84	80·3	70	96	83·1	76	104	6·8	12	2·8	18	9·6	20

In only one case was the evening increase equal to that of the morning. The cases in which the morning excess was very little were for the most part the less advanced cases—viz., 80, 79, 95, 88, and 69; and those in which it was considerable were for the most part greatly advanced—viz., 85, 46, 73, and 77. Thus it appears that when the disease is greatly advanced, and the vital capacity at a minimum, the rate of pulsation is less amenable to disturbing influences, although the total pulsations are more numerous in the evening than in the morning. There are many exceptions to this rule.

The combined effect of posture in all the cases with the amount of evening excess, is given in Table XXV. (p. 495).

Thus the highest average numbers and the highest extremes, in all the positions, is found in the evening; but the difference in pulsation from

TABLE XXV.

	Morning.	Evening.	Excess of Evening.
Mean pulsation in all the cases.	91·2	98·4	7·2
Lying	81·7	92·4	10·7
Extremes.....................	55 134	62 138	
Sitting	91·9	99·6	7·7
Extremes.....................	62 142	70 150	
Standing	101·8	106·5	4·7
Extremes.....................	76 162	95 166	

position, and the difference between the extremes, are less in the evening. The morning increase in the sitting and standing postures is 10 each, or a total increase of 20 pulsations; whilst in the evening it is only 7·2 in the sitting, and 6·9 in the standing, or a total of 14 pulsations.

C. *Influence of Temperature.*

This is shown upon diagram No. 5. In the lower part are represented the pulsations in the three positions of the body, with the temperature of the wards at eight A.M. and four P.M. In the upper part are the total pulsations for each day of the month, with the mean temperature of the wards and of the external air, as also the degree of dryness of the air at Greenwich. The totals of the three postures have been used, in order to show the daily differences more readily.

The following facts may be obtained from the upper part of the diagram:—

1. The total pulsations increased from 539 to 627 throughout the month, from May 14th to June 9th. The weekly averages were 558, 563, 586, and 609, and consequently there was a weekly increase. Two days broke the rule grossly—viz., May 16th and 24th, and I cannot offer a reason for it.

2. The external temperature increased from 42° to 72° in the same period. Thus the lines of pulsation and external temperature, in each of the two first weeks, begun low on the Monday, and increased together to the Saturday (falling from the Saturday to the Monday). In the third week both were low on the Monday, but fell lower on the Tuesday or Wednesday, and then rose together to the Saturday; thence they rose together through the Sunday to the Monday, and suddenly rose greatly on the Wednesday and Thursday, after which both fell to the end of the week.

3. The internal temperature rose from 60° to 73°, and the weekly averages were 62·4, 65·4, 63·6, and 66·5. The variations were to a less extent than those of the external air, but, with the exception of the third week, they support the rule now laid down. In the third week both temperatures were very low, and there was this correspondence with the

pulse, that the pulsation did not increase in that week in the proportion of the increase of the preceding and succeeding week.

4. The influence of temperature is not proportionate to the number of degrees. Thus, so long as it remained at or below 60° externally, and 64° internally, the influence of its variation was inconsiderable. (See the first week and the second week to the Thursday.) On the other hand, when it exceeds those degrees, the effect is great, and increases in a vastly increasing ratio with increase of degrees. There were two remarkable ascents both of temperature and pulsation—viz., on the Friday and Saturday in the second week, and the Wednesday and Thursday in the last week; and on those days I noted (see the explanation of the diagrams) that the effect on my own system was hot and oppressive. Thus, whilst the temperature feels agreeable, the pulse is but little influenced, but when and whilst it feels oppressive it is greatly increased.

A temporary fall of temperature lowers the pulse, but if it occur after a continued rise of temperature, the pulse does not fall so low as before the rise. Thus, in the third week, both the external and internal temperature fell as low as it was on May 14th; but although the pulse fell also, it never fell so low as on that day.

We now refer to the lower part of the table.

In the morning the pulsation was more under the control of the internal than of the external temperature, and upon the whole corresponded with it. The daily pulsations rose from 261 to 300, and the successive weekly averages were 269, 272, 282, and 290.

Three points are worthy of notice as especially confirming the rule:—
1. The pulse increased from the second Saturday to the Monday, because the morning temperature was exceedingly high on those days, and it is probable that it was yet higher on the intervening Sunday; on the Monday morning it was insufferably hot and oppressive in the wards.
2. It fell from the third Saturday to the Monday, because the same temperature then fell also. (Both of these are opposed to the state of the pulse and temperature on the same days on the upper half of the diagram.)
3. The highest pulsation and temperature in the fourth week were on the Thursday.

In the evening the lines both of temperature and pulsation differ much from the morning. The pulsation increased from 280 to 327, and the weekly averages were 289, 291, 305, and 319, and thus supported the rule.

The facts peculiar to the evening are as follows:—1. The two exceptional days—viz., May 16th and 24th—before mentioned, are much more marked than in the morning. 2. The increase in pulsation in the fourth week began one day earlier, for the highest temperature in the wards was on the Wednesday evening and Thursday morning. 3. The third week was very exceptional, for not only was the pulsation then low with the low temperature, but the two lines in their course were directly opposed to each other, and chiefly so in the first three days. 4. The temperature, and pulsation too, increased from the third Saturday to the Monday; and upon the whole, the character given to the upper part of the diagram is chiefly due to the evening pulsations. 5. It is manifest that the evening internal temperature was more under the control of the external temperature than the morning, but the morning pulsation more closely corresponded to the line of temperature.

Thus the total pulsations, and also those of the morning and evening separately, prove incontestably that pulsation is increased (within limits) by increasing temperature, and decreased (within limits) by decreasing temperature.

The cases differed much in themselves in respect of the closeness of the relationship now indicated. Nos. 73 and 79 were almost equivalent to thermometers, whilst No. 51 (who had Bright's disease also) presented a direct opposition to temperature during the first week of the inquiry, when the temperature was very low. With this one exception, all the cases supported the rule. No case presented the same close relationship in every week; but, in order to show the weekly relationship in all the cases, I have arranged the relationship under six heads—viz., "very great," "great," "moderate," "little," "rather opposed," and "opposed."

TABLE XXVI. *Showing the degree of relationship between temperature and pulsation in* 11 *cases in each of four weeks, at* 8 A.M. *and* 4 P.M.

	Eight A.M.				Four P.M.			
Week	1st	2nd	3rd	4th	1st	2nd	3rd	4th
Very great	2 } 5	2 } 4	4 } 8	3 } 7	1 } 4	} 5	2 } 6	3 } 7
Great	3	2	4	4	3	5	4	4
Moderate	3	5	2	3	2	5	2	3
Little	2	1	...	1	4	1	...	...
Opposed (rather)	...	1	1	...	...	...	3	...
Opposed	1	...	...	...	1	...	...	1

Thus in but three instances were the lines opposed; whilst a "very great" and "great" relationship is found in more than half the cases in all the weeks. It is evident that the relationship is greater in the morning, and in the third and fourth weeks both morning and evening—that is, when the temperature was more elevated. This increased relationship in the evening is not due to evening increase of pulsation.

The cases may be arranged in the following order in reference to their agreement with this rule, beginning with that in which the relationship was the closest:—Nos. 73, 79, 107, 95, 88, 90, 69, 85, 77, 46, 58, 80, and 57.

Why certain persons do not bear heat well.—On two occasions I inquired into this subject. On May 30th the weather was very chilly, and I found that six patients felt better and six felt worse with the cold, but I omitted to note their numbers. On June 6th the weather was very hot, and six patients bore it badly, and five were but slightly influenced. The six who bore it badly were Nos. 79, 80, 90, 85, 107, and 88; and the five who bore it well were Nos. 73, 77, 69, 46, and 95. I subsequently ascertained that those who bore the heat badly had the pulsations greatly increased by it, but the others were but slightly affected. Thus is was evident that in the former the heat increased the wear and tear, and induced exhaustion.

The effect is shown in the following table, which includes the two oppressive periods—viz., May 25th and 26th, and June 5th, 6th, and 7th, both at eight A.M. and four P.M.:—

TABLE XXVII. *Showing the effect of high temperature on pulsation in each case.*

Case.	Eight A.M.						Four P.M.					
	May 25th & 26th.			June 5th, 6th, & 7th.			May 25th & 26th.			June 5th, 6th, & 7th.		
	Gen. expression.	Pulsation.		Gen. express.	Pulsation.		Gen. express.	Pulsation.		Gen. express.	Pulsation.	
		Incr.	Decr.		Incr.	Decr.		Incr.	Decr.		Incr.	Decr.
88	moderate	8	...	great	13	...	v. great	19	...	v. great	18	10
79	very little	3	...	ditto	14	...	mod.	9	5	ditto	17	8
85	moderate	9	...	ditto	11	...	v. great	18	...	none	...	5
80	ditto	8	6	v. little	6	7	ditto	15	...	v. great	17	...
107	none	...	6	great	10	...	great	11	...	little	6	...
58	?	5	7	...	...	...	ditto	13	...	...	...	...
73	very great	15	...	none	4	4	none	2	2	great	11	...
75	very little	4	...	...	...	...	mod.	9	...	...	...	...
69	ditto	4	...	none. Had fev.	...	12	great	12	...	v. little	4	...
90	none	1	...	great	17	8	v. little	3	...	none	...	5
95	ditto	...	4	little	5	...	mod.	9	...	little	5	...
77	...	...	...	v. little	5	3	...	...	...	none	2	2
46	...	...	...	none	...	6	...	...	...	v. little	6	4

It is evident that the evening effect was greater as the evening temperature was greater. In dividing the cases into two categories the first six must be classed as those most influenced by temperature; and the following arrangement is probably very near to the truth:—1. Those who suffered from the great heat—Nos. 88, 79, 80, 107, 85, and 58. 2. Those who did not suffer from the great heat—Nos. 46, 77, 95, 90, 69, 75, 73.

Each case is arranged in the order of intensity, beginning with the most intense; and thus 88 suffered the most, and 46 the least.

If the two series now given be compared with the cases as influenced by heat, a perfect correspondence will be found, except case 90, who suffered from the heat, and yet the increase of pulsation was but little; and hence it may be inferred that the increase of pulsation would, on a large scale, be a measure of the suffering from great heat.

There are thus three expressions which indicate the variation in the effect of heat upon the system:—1. The extent to which the pulse is influenced by it; 2. The regularity with which the pulse varies with the varying degrees of heat; and 3. The degree of ease with which oppressive heat is borne by different patients. All these differ in individuals.

PART II. — THE RATE OF RESPIRATION IN PHTHISIS.

This was determined by the same inquiry as that of pulsation, and is represented upon the same diagrams.

Absolute Frequency.

The total results of upwards of 1500 observations show the average rate to be 23 per minute, with extremes of the averages of 16 and 31, as shown in the following table:—

TABLE XXVIII. *Showing the average rate of respiration in each case, arranged in the order of frequency.*

Case	73	77	93	58	107	90	46	95	51	79	80	85	88	75	69
Average rate	31·	28·7	27·3	25·6	24·9	24·7	22·7	22·6	22·4	20·6	20·6	20·6	18·1	17·	16·

Thus, in four cases the rate exceeded 25, and in three it was below 20 per minute. The extremes in single observations were much greater—viz., 11 in No. 75, and 43 in No. 73. So that in the average extremes the greater was double of the lesser, whilst in the absolute extremes it was four times the lesser.

The average weekly variations varied from 2 to 5 respirations; and in 9 out of 12 cases the rate diminished as the month advanced, as is seen in the following table:—

TABLE XXIX. *The weekly average rate of respiration in* 12 *cases.*

Case	88	107	75	77	79	69	58	46	95	90	85	73
1st week	21·2	25·4	19·6	...	22·7	18·9	27·3	...	23·3	21·1	20·4	31·5
2nd „	18·4	27·1	14·4	...	21·4	16·1	24·	...	22·8	22·2	21·5	31·
3rd „	17·9	23·7	...	31·6	18·6	15·	...	24·3	22·	23·1	20·8	29·5
4th „	17·2	23·6	...	27·9	19·1	14·2	...	21·2	22·6	22·7	20·	32·2

In the first 9 cases there was a decrease, whilst in the last 3 the rate was stationary, or slightly increasing. Thus whilst the disease was generally progressing, the respiration declined; and in case 85, in which the progress was very great, the respiration was stationary.

There is a general relationship between frequency of respiration and extent of disease, as is shown by the following order of the cases, beginning with the most intense:—

TABLE XXX.

Order of extent of disease	Nos. 90	88	73	77	85	107	95	46	80	69	79	58	75	51
„ frequency of respiration	„ 73	77	58	90	107	51	95	46	80	85	79	88	75	69

See Table No. XVI.

NOTE.—When any number of cases are bracketed together, on account of the identity of their returns, their order may be varied without altering their values; and this has been done in the foregoing and succeeding tables of comparison.

There is also a general relationship with frequency of pulsation in the cases, considered for the whole period, as is proved by the following comparison:—

TABLE XXXI.

Order of frequency of respir.	Nos. 73	77	93	58	90	107	46	95	51	79	80	85	88	75	69
„ „ pulsation	„ 85	77	73	93	90	46	80	79	107	88	58	95	69	75	51

No. 85 is the most singular exception, since, with a medium frequency of pulsation in one day of 152, and a total monthly increase of 30 pulsations, the respirations were only 20 per minute, and rather declined through the month.

The relation was very various in instances of temporary increase of pulsation. Both were increased together on the following occasions:—The evening increase; the increase from great temperature on June 6th; case 73, on various occasions; case 80, in the third week, in the evening; case 107, on May 24th (morning); and cases 58 and 46. Both fell together in case 51. There was direct opposition or indifference in cases 69, 95, 88, 75, 77. In case 85 there was no increase in respiration with increase in pulsation; and so, also, in case 88, on May 19th, although the medium increase was 26 pulsations.

There is an inverse relation between rapidity of respiration and height of body, as in the following table, in which the order of respiration begins with the most frequent, and of height of body with the least:

TABLE XXXII.

Order of height of body Nos.	73	51	107	80	77	58	75	95	88	85	46	79	90	69
„ „ in feet	4·9¾	5·4	5·5⅓	5·5½		5·5½		5·6⅓	5·6¾	5·7	5·7⅓	5·7¾	5·8½	6·1¾
Order of freq. of respir. Nos.	73	77	58	107	90	51	46	95	80	85	79	88	75	69

Cases 85 and 95 here agree with this rule, and case 88 is less exceptional; but there are several exceptions. The tallest and the least agree with the rule.

There is some general relation between frequency of respiration and vital capacity. In the following table the vital capacity is represented both by the quantity existing and the amount lost from a state of health:

TABLE XXXIII.

Order of frequency of respiration	73	77	58	107	90	46	51	95	79	80	85	88	75	69
„ existing vital capacity, beginning from least	85	77	107	88	79	90	73	58	46	80	51	95	75	69
„ loss of vital capacity, beginning from greatest	not known	90	79	107	46	88	80	58	73	51	75	95	...	

The agreement is thus somewhat greater with the existing vital capacity. Cases 77, 80, 75, and 69, agree perfectly, and 107, 90, and 46, nearly; but there are many exceptions. It must, however, be remembered, that the determination of the vital capacity is liable to much error.

There is also a similar general relationship between rapidity of respiration and excitability of temperament. Thus:

TABLE XXXIV.

Order of frequency of respiration	73	77	58	107	90	95	51	46	80	85	79	88	75	69
Activity of temperament from greatest	73	88	107	77	85	79	90	95	80	69	51	58	46	75

The estimation of the temperament is liable to fallacy; but as Nos. 73, 77, 107, 51, and 80, agree perfectly, and Nos. 90, 95, and 75, nearly, there must be the general correspondence now sought for.

Thus, a review of the foregoing comparison leads us to accord a certain amount of belief to the statement that there is a general correspondence between rate of respiration and the following subjects—viz., extent of disease, frequency of pulsation, short stature, lessened vital capacity, and excitable temperament; but in all there are exceptions, and in none is one the measure of the other.

DISTURBING CAUSES.

A. *Period of the Day.*

On the total average the evening respirations were one-eleventh more frequent than those of the morning—viz., 21·8 and 23·6. The extremes were very great—viz., 12 and 32·3 in the morning, and 11 and 40·6 in the evening. The evening excess varied in the different cases as follows:

TABLE XXXV.

Order of evening excess. Cases	73	93	80	46	77	58	85	Total average.	75	107	95	90	69	88	79	51
Amount of evening excess	5·9	3·6	2·9	2·3	2·2	2·1	2·0	1·8	1·4	1·1	·8	·6	·5	·4	·3	—·2

Thus, 1 only had no excess; in 1 it was nearly six respirations; in 6 it was under one respiration; and in half of the cases it was under two respirations. The total average was 1·8 per minute. The two or three cases at the head of the list must have some exceptional peculiarity. As two of these had the most frequent rate of respiration, and three at the foot of the list, Nos. 69, 88, and 79, had the least frequent, there may be some relation between rate of respiration and evening excess; but the middle cases vary greatly in reference to this rule. There is no clear correspondence between this evening excess and excitability of temperament, lessened vital capacity, frequency of pulsation, extent of disease, or height of body; but in the two latter there are some approaches to it.

There is a relationship between evening excess of respiration and pulsation. Thus—

TABLE XXXVI.

Order of excess of respiration.—Cases	73	93	80	77	46	58	85	75	107	88	79	90	95	69
„ „ pulsation	73	46	88	77	80	58	93	85	107	51	79	90	75	69

No. 88 is very exceptional, since, with an average evening increase of 12 pulsations, the increase of respiration was only ·4.

The weekly variations in the evening excess of respiration are shown in the next table.

TABLE XXXVII.

Case	73	80	46	77	58	85	75	107	95	90	69	88	79
Average, 1st week	5·4	...	...	...	1·9	2·4	2·1	·2	1·	=	·3	—·8	·1
„ 2nd „	6·1	3·4	...	...	2·3	2·2	·8	2·4	1·	1·2	·4	·3	—·8
„ 3rd „	6·3	3·2	2·7	4·7	...	1·5	...	1·8	1·	1·6	·9	—·2	1·5
„ 4th „	6·1	2·2	1·8	3·7	...	2·3	...	2·	·5	—·6	·6	2·	·6

Thus, 3 of 13 cases had no excess in one or more weeks. No. 88 is remarkable as having an excess of 2 respirations in the last week, whilst there was none in the first and third, and but a very slight excess in the second week. This was not due to increased rate of respiration, for the rate fell 4 respirations between the last and the first week.

There is no uniform correspondence between weekly increase or decrease, whether of pulsation or respiration, and increase or decrease of evening excess.

The uniformity of evening excess is much lessened by referring to the daily experience of each case. Thus, of 814 observations, only 458 (or little more than a majority) had the excess.

B. *Posture of Body.*

The effect of posture is to increase the rate of respiration in the sitting and standing postures, but not with uniformity. Thus, of 502 observations, 300 (or three-fifths of the whole) showed an excess in sitting over lying, and 237 (less than a majority) an excess in standing over sitting. The average returns were as follows:

Lying 22· ...... Sitting 23· ...... Standing 23·4

Therefore, in neither the sitting nor the standing posture is the excess to one-half of the proportionate extent noted under pulsation.

The amount differed much in the several cases. Thus—

TABLE XXXVIII.

Order of sitting over lying. Case	77	51	58	73	79	85	75	88	90	46	93	80	107	69	95
Amount of sitting over lying	3·5	2·8	2·4	1·7	1·5	1·3	1·2	1·2	1·1	·9	·6	·5	·3	·1	—1·7
,, standing over sitting	2·6	—1·3	1·5	·4	·4	·5	·3	—·9	·4	—·2	1·9	—	=	=	—·5

Thus, in 1 case there was no increase, but a large decrease from posture, and that was the healthy-looking ruddy farmer with one lung nearly sound. In 1 only was the increase in the standing over the sitting greater than that of the sitting over the lying postures. In 3 the increase was equal, whilst in 4 cases there was no increase in the standing over the lying posture. 1 case showed the greatest decrease from the standing posture, and at the same time nearly the largest increase from the sitting posture; whilst another case exhibited the greatest increase in both the sitting and the standing postures. In 9 of 15 cases the average increase in the sitting posture was over 1 respiration.

The influence of period of the day in modifying the influence of posture of the body is to increase it. Of 238 observations in each position, there was an increase at four P.M. of 179 in the lying, 154 in the sitting, and 125 in the standing, or in more than a majority of the observations in each of the positions. The excess was the most constant in the lying posture; and in the standing posture the constancy was much greater than the amount of excess. In this respect it accords with the returns from pulsation, and also in the following one—viz., that whilst the rate is greater in the evening, the excess of one position over the other is less than in the morning. Thus, on the total average excess of sitting over lying, 1·8 morning, ·9 evening; of standing over sitting, ·5 morning, ·4 evening; and the total of standing over lying, 2·3 morning, and 1·1 evening. Four cases were exceptional—viz., 77, 6· morning, 7· evening; 58, 2·9 morning, 4·8 evening; 85, 1·9 morning, 2·6 evening; and 69, ·4 morning, and ·5 evening. But in no instance was there such an exception in reference to pulsation. In two of the above exceptions—viz., 77 and 58, the increase from position on each of the postures was very great.

TABLE XXXIX.

	Sitting over lying.		Standing over sitting.		Standing over lying.	
	Morning.	Evening.	Morning.	Evening.	Morning.	Evening.
No. 77	3·2	3·6	2·7	3·3	6·	7·
No. 58	2·2	2·5	·7	2·3	2·9	4·8

On the contrary, 7 cases (Nos. 95, 88, 80, 46, 107, 69, and 51) had no total average increase in the standing over the lying posture; whilst in others that increase was so great as 19 respirations. The total extremes in the increase were—sitting over lying, 11 morning, 17 evening; standing over sitting, 8 morning, 12 evening; and standing over lying, 11 morning, and 19 evening. Thus the extremes were the greatest in the evening.

C. *Influence of Temperature and Dryness of the Air.*

This is very remarkable, and tends to lessen the rate of respiration. The relations are shown in diagram No. 5, constructed, as already stated, under the head of pulsation; but in each department there are two lines representing respiration—viz., No. 1 for comparison with temperature and humidity, and embracing only the returns of those cases which were under observation for the whole month; and No. 2 for comparison with pulsation, since both of these lines embrace all the cases, whether observed for a month or a shorter period.

The line of respiration, on the one hand, and those of pulsation, temperature, and dryness, are directly opposed, both in the whole month and in each week, except the third week. Thus, 1st. The respirations declined from the beginning to the end of the month; 2nd. They declined from the Monday to the Saturday in the first, second, and fourth weeks. The line of dryness usually followed that of temperature (except during rain); and both attended upon the line of pulsation; and all had a course opposed to that now stated in reference to respiration. As, therefore, the temperature, dryness, and pulsation increased, so the respirations decreased. The third week was exceptional, and in that the respiration rather followed the line of humidity than that of low temperature; and in that week only was there rain. This exceptional character was, however, much less marked in the evening than in the morning returns; and it is very evident that in both the morning and evening investigations the pulsations were kept low and the respirations high by the great cold and humidity of that week, as compared with their course in the coterminous weeks.

The above statement has reference to the fact that commonly the line of dryness of the air corresponds with that of temperature; and hence it is difficult to consider one apart from the other. Whether, therefore, the influence over respiration is exercised by humidity or temperature, or both combined, it is scarcely possible to determine; but the returns of the third week seem to imply that there is an inverse relation between humidity and respiration, whatever may be the relation with heat.

RATIO OF THE RESPIRATION TO THE PULSE.

The ratio of the total averages is 1 : 4·1, and it varied in the different cases from 1 : 6·2 to 1 : 3·3, or a doubling of the lesser ratio. Thus—

TABLE XL.

Order of ratio. Case	85	88	69	75	80	79	46	Total ratio.	90	95	77	107	93	58	51	73
Amount of ratio, as 1 to	6·2	5·4	5·2	4·9	4·6	4·4	4·2	4·1	3·9	3·8	3·8	3·7	3·6	3·5	3·4	3·3

In four-fifths of all the cases the ratio was less than 1 : 3, and more than 1 : 5.

In 85 the lessened ratio is due to enormous rapidity of pulsation; whilst in 88 and 69 it is rather due to slowness of respiration; and No. 73 had the highest ratio from the rapidity of his respirations.

The ratio lessened as the month advanced and the disease progressed, as was shown by the increasing rate of the pulse and decreasing rate of the respiration. The extent of this decrease is seen in the following table, which gives the weekly averages, the cases being arranged as in Table No. XXIX.

TABLE XLI.

Case	88	107	75	77	79	69	58	46	95	90	85	73
Ratio, 1st week, 1 to	4·3	3·7	4·2	...	4·1	4·3	3·3	...	3·6	4·6	5·8	3·3
,, 2nd ,, ,,	4·9	3·5	5·8	...	4·3	5·0	3·6	...	3·7	4·5	5·8	3·37
,, 3rd ,, ,,	5·1	3·8	...	3·5	4·2	5·7	...	4·	3·5	4·2	6·2	3·5
,, 4th ,, ,,	5·4	3·8	...	4·1	5·	6·2	...	4·5	3·9	4·5	7·	3·25

Thus, in 10 of 12 cases the ratio decreased, and particularly in Nos. 69, 85, 75, 88, 79, 77, and 46, in the order in which they are placed; and in some degree this order was that of diminution of respiration and increase of pulsation. It also includes all the cases of evident progress except 107, and also two in which I could not detect progress; and I am disposed to regard the varying ratio as evidence of the general (not momentary) progress of disease. This fact may be tested by the returns from No. 69, since the period of sudden accession and the decline of that increase may be determined with accuracy—viz., from May 31 to June 7. The following are the varying ratios at 8 A.M. and 4 P.M.

TABLE XLII.

Date	May 30	31	June 1	2	3	4	5	6	7
Ratio at 8 A.M., as 1 to	5·2	5·6	6·1	6·4	omitted	5·8	6·5	6·0	5·7
,, 4 P.M. ,,	6·0	omitted	6·4	omitted	ditto	6·4	6·3	6·2	6·3

It thus appears that the ratio declined until the acme of the attack, and thenceforward increased. There is no correspondence between the ratio and height, age, temperament, and vital capacity, and but little with frequency of pulsation, in the cases; but the ratio is the greatest in the cases of most frequent respiration.

On the total average the ratios at 8 A.M. and 4 P.M. were identical (4·17), but in 8 of 15 cases the ratio was a little less in the evening, as is seen in the following table :—

TABLE XLIII.

Case	88	51	46	79	77	107	58	90	85	93	73	95	75	80	69
Ratio at 8 A.M., as 1 to	4·6	3·1	4·1	4·4	3·8	3·6	3·4	4·5	6·3	3·7	3·4	3·9	4·95	4·6	5·2
,, 4 P.M. ,,	5·1	3·5	4·3	4·6	3·9	3·7	3·6	4·6	6·	3·5	3·2	3·7	4·8	4·5	5·2
	Evening decrease.								Evening increase.						Equal.

There is no relation between this evening change in ratio and evening excess either of pulsation or respiration; but there is a slight tendency to a relation between excess of evening ratio and evening excess of respiration.

Posture of body influences this relation, and the ratio is the greatest in the lying posture. Thus, lying, 1 : 4; sitting, 1 : 4·15; and standing, 1 : 4·63. The considerable diminution in the standing posture is rather due to the lessened rate of respiration in that posture than to increase of pulsation. Thus, the erect posture and muscular exertion increase the pulsation disproportionately to the respiration, and lessen the ratio. This rule is supported by the returns from each case. Thus—

TABLE XLIV.

Case	95	88	75	80	79	69	107	46	85	51	93	58	73	90	77
Ratio lying, as 1 to	3·	4·3	4·5	4·2	4·3	5·	3·4	4·	5·9	3·3	3·3	3·5	3·3	4·4	3·6
,, sitting ,,	3·8	4·8	4·8	4·6	4·4	5·	3·7	4·2	6·	3·27	3·6	3·5	3·3	4·4	3.8
,, standing ,,	4·3	5·5	5·3	5·	4·97	5·6	4·	4·6	6·4	3·4	3·7	3·7	3·4	4·5	3·6

In every case but one the ratio was greater in the lying than in the standing posture; and there was a decrease from the sitting to the standing posture. In 6 cases the ratio was the same in the lying and in the sitting postures; and thus practically the sitting and the lying postures have the same ratio, and are both equally opposed to the standing posture. The diminution in the standing posture is exceedingly well marked in Nos. 95 and 88, and they had great difference of pulsations from posture; whilst No. 95 had scarcely any increase of respiration, and No. 88 had a decrease in the evening. In the last 5 or 6 cases neither pulsation nor respiration were much influenced by posture. Period of the day had but little influence over the ratio in the three positions. Thus—

TABLE XLV.

	Lying.		Sitting.		Standing.
Ratio at 8 A.M., as 1 to	3·94	...	4·1	...	4·5
,, 4 P.M. ,,	3·97	...	4·2	...	4·4

The following is a summary of the preceding communication.

1. The average rate of pulsation was 95·3, and of respiration 23· per minute. The extrêmes were 55 and 166 pulsations, and 11 and 43 respirations.

2. The rate of pulsation was not dependent upon the stage or progress of the disease, or the degree in which the system had become implicated.

3. The more extensively diseased or advanced cases of disease differed

from the others in that fewer of them had a non-excitable temperament, they had a less vital capacity, and a more frequent average rate of pulsation and respiration, with wider extremes.

4. The weekly average variations in respiration were from 2 to 5 respirations, and at that period of the year the rate of respiration declined, whilst that of pulsation increased.

There is a general relationship between frequency of respiration and extent of disease, frequency of pulsation, short stature, lessened vital capacity, and excitable temperament; but there are many exceptions.

5. The average rate of both pulsation and respiration is greater at 4 P.M. than at 8 A.M., to the extent of 7·2 pulsations and 1·8 respirations, and the extremes are greater. The evening excess of pulsation was usually associated with extent of lung disease, frequency of pulsation and temperature, but not with activity of body. Probably there is some relation between the evening excess of respiration and frequency of respiration; but there is no indisputable relation between it and excitability of temperament, lessened vital capacity, frequency of pulsation, extent of disease, or height of body. The evening excess of respiration varied much from week to week, and more particularly from day to day, in which latter a bare majority only had the evening excess.

Posture of body affects the rate of both functions so as to increase it in the sitting, and still more in the standing posture. The total average increase in pulsation was 8½ in the sitting, and a further 8½ in the standing posture. In the morning the difference was 10 pulsations in each position, whilst in the evening it was 7 in each position. The rate of pulsation in each posture was thus greater in the evening, but the difference from posture was less, and chiefly in the standing posture. The extremes in each posture were less widely apart. The variation in the increase from posture was not dependent upon age, temperament, amount of lung disease, frequency of pulsation, or stature.

The exceptions to the increase were most numerous in the sitting and standing postures, and more particularly in the evening.

The rate in the sitting posture is the mean pulsation in all postures.

The average increase of respiration in the sitting posture was 1, and in the standing ·4; and only three-fifths of the observations showed the excess in the sitting, and less than half in the standing postures. Thus, the effect of posture is less, and is less constant in respiration than in pulsation. It was increased in the evening, but most in the lying posture; and the increase was the most constant in the lying posture, whilst in the standing posture the constancy was greater than the amount of the excess. The cases differed much in the amount of the excess, and in some it amounted to even 19 respirations.

6. The influence of increasing temperature was to increase the dryness of the air and the rate of pulsation, and to lessen the rate of respiration. Whilst the temperature was externally under 60°, and internally under 64°, the rate of pulsation was not greatly influenced; but when above this the influence was great, disproportionately to the number of degrees. Those persons who do not bear heat well have great increase of pulsation from heat, and those who bear it well have not a similar increase. Falling temperature lowers pulsation and increases respiration; but if it

succeed to an elevated condition, the functions do not soon attain to the same state as with the same temperature previous to the elevation.

The rate of respiration is manifestly influenced by the dryness of the air, and is inversely as the dryness.

7. The ratio of the respiration to the pulse was 1 : 4·1; but in four-fifths of the cases it was on the average less than 1 : 3 and more than 1 : 5. It lessened as the temperature or the disease progressed, and was due to the rate of respiration rather than to that of pulsation. Probably the diminution of the ratio may be a test of the progress of the disease. It was scarcely different at 8 A.M. and 4 P.M., but was lessened in the sitting, and still more in the standing, postures. Thus the erect posture, muscular exertion, and wear and tear, lessen the ratio of the two functions.

Deductions, or Inferences.

1. There is no rate of pulsation or respiration indicative of phthisis, or of any stage of phthisis; but whilst the rate of both is on the average above that of health, it may be, in any stage, much lower than that of health.

2. In cases of phthisis, whether greatly advanced or not, there is as much food taken, on the average, as would have been in health, with the same degree of exertion; and hence it may be inferred that the appetite and digestion are also good;—yet the weight of the body diminishes. Hence the food must be consumed more rapidly than in health, or be less perfectly assimilated and deposited, or the material having been only deposited it is more quickly removed.

3. In the more advanced cases less air is admitted into the lungs, and the rapidity of both respiration and pulsation is greater, and there are wider extremes in the rate. Thus, the changes of aëration and assimilation must be less perfectly performed, whilst all the functions in the body are more frequently and rapidly performed, and the wear and tear probably greater.

4. It is probable that one sound and one extensively diseased lung is less injurious than both lungs diseased to a much less extent; and that moderate consolidation and destruction together is worse than more extensive consolidation alone, and the more so still if both lungs are moderately affected.

5. At four P.M. all the functions are more frequently performed than at eight A.M., and consequently more exhaustion will follow. This increase is chiefly due to food, temperature, sun-light, and exertion; and it is more so when the lung disease is considerable. The proportionate diminution in the respiratory function is greater in the evening.

6. The effect of posture is much greater than in health (Dr. Guy's observations in reference to the latter being accepted); but chiefly in the morning, and in the influence of the sitting over the lying posture. In health, the excess in the sitting posture was about 3½, and in the standing over the sitting nearly 9; whilst in phthisis it was 7 in each in the evening, and 10 in each in the morning. Hence the sitting and standing postures in phthisis call for more expenditure of power, and tend to produce more subsequent exhaustion, than in health; and the lying posture would save the strength. The effect upon respiration is much less, and

especially in the standing posture. Hence the latter posture further tends to exhaust the system by increasing the blood-motion, and not also the entrance of air into the system.

7. High temperature, with the accompaniment of dry air, also tends to rapid exhaustion by greatly increasing the blood-motion and greatly lessening the introduction of air; and, on the contrary, low temperature and moisture increase the aërification of the blood and lessen the rapidity of the blood-current. Hence, in phthisis, a moderately cool and moist air is the most conducive to health, and the hot summer season must induce exhaustion.

8. No one should be sent to a hotter climate who bears heat badly; but if he bear it well, and need a milder air, he will not be more exhausted, and particularly if the air be rather moist.

DESCRIPTION OF THE DIAGRAMS.

Diagram No. 1, exhibits the average rate of pulsation and respiration in each of the postures of lying, sitting, and standing, on a total of 3100 observations, on all the cases combined, and on every day at eight o'clock A.M., and four P.M., for the whole month. The temperature of the ward at eight o'clock A.M., and four P.M., and the mean external temperature of the day for the same period, is placed at the head of the Diagram.

Diagrams Nos. 2, 3, and 4, are similarly constructed, but they exhibit the like facts in each case separately. In No. 2 are the cases Nos. 69, 90, 79, and 85. In No. 3, the cases Nos. 88, 58 and 46, 75 and 77. In No. 4, cases 95, 107, 73, 51, and 80.

In each of the above Diagrams, the date of each day is placed at the head and foot of the Diagram, and the value of any of the lines on each day is ascertained by referring to the scale of Nos. on either side. All the lines on the left of the central line refer to the examination at 8 A.M., and on the right to 4 P.M. The same kind of line which indicates one posture in the lines of pulsation is used for the like posture in the lines of respiration; and the same plan is carried out through all the Diagrams.

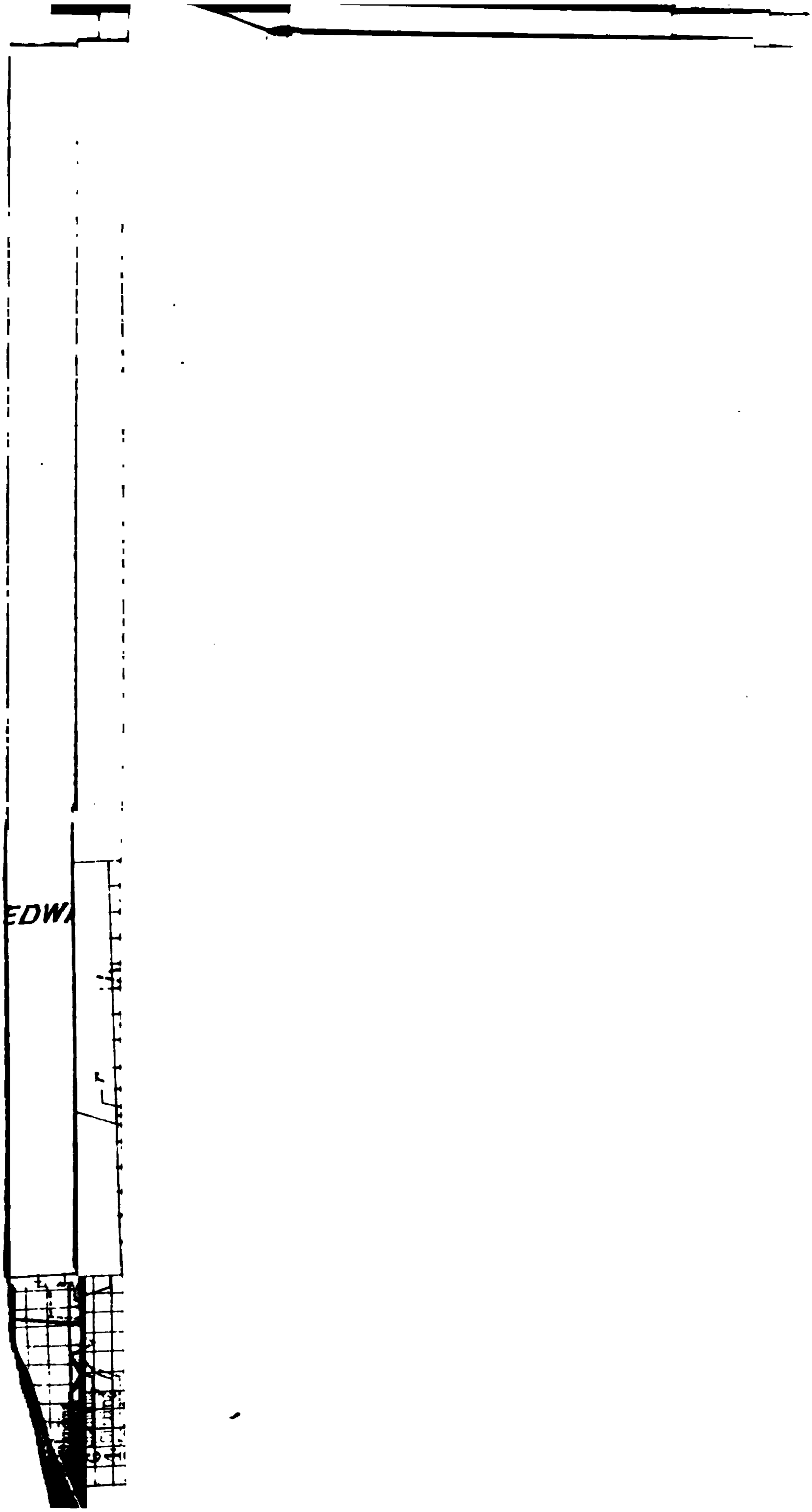
When two cases consecutively occupy the whole month, they are placed consecutively upon the same Diagram, so as at first sight to appear like one case only; but the point of termination of one case and the commencement of the other is clearly indicated. This may be seen on Diagrams 3 and 4.

In reference to the parts of the Diagrams indicating the temperature, it was intended to place in each day the expression of the effect upon my own feelings, but the necessary smallness of the squares prevented the adoption of this course. The effect is therefore given in this place; and the reader may readily refer the description to the diagrams:

May 14. Dry, dull.
15. Dry, dull.
16. Dry, comfortable.
17. Dry, warm, comfortable.
18. Ditto.
19. Ditto.
21. Dry, dull.
22. Little rain, dull.
23. Dry, dull, warm.
24. Ditto.
25. Dry, oppressive.
May 26. Dry, oppressive.
28. Rain in the night, comfortable.
29. Rain, cold, dull.
30. Ditto.
31. Much rain, cold, dull.
June 1. Rain, comfortable.
2. Warm, comfortable.
4. Dry.
5. Dull, hot.
6. Very hot, oppressive.
7. Rain in the night, very hot.

Diagram No. 5 shows at the lower part the total average pulsations and respirations in the three postures, combined with the temperature of the wards on each day at 8 A.M. and 4 P.M. In the upper part, the total average at 8 A.M. and 4 P.M., combined in the three postures, is represented as obtained by adding together the returns on the lower part of the Diagram: it therefore represents the average daily rate; but each return must be divided by 6 (the three postures taken twice over), in order to gain the true mean daily rate. There are also added, the mean internal and external temperature; and the degree of dryness of the air is indicated by the dew-point.

There are two lines indicative of respiration: that marked No. 1 is intended for comparison with temperature and humidity, and No. 2 with pulsation.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PART FOURTH.

Chronicle of Medical Science.

HALF-YEARLY REPORT ON FORENSIC MEDICINE & TOXICOLOGY.

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I. Toxicology.

The past six months have been rendered notorious by the additions that have been made to practical toxicology. Arsenic, antimony, strychnia, have all been invested with new terrors, owing to their application to criminal purposes. We shall select out of the long calendar of poison cases before us, such as are most peculiar in their physiological and pathological characters.

Slow Poisoning by Arsenic: the Wooler Case.—The most complete history as yet given of the Wooler case is from the pen of Dr. Christison. In the beginning of May, 1855, Mrs. Wooler, a rather delicate woman, was attacked with pain and vomiting soon after an ordinary dinner. On the 8th of May she was seen by Dr. Jackson, who found her suffering from "gastro-intestinal irritation," and treated her with bismuth. She had a sickly look; a small frequent pulse; flatulence; frequent slight tickling cough, or rather hawking without expectoration; an occasional discharge of mucus from the bowels, with tenesmus and griping; redness of the eyelids and lining membrane of the nostrils; loss of appetite; and great failure of strength. In three or four days more there were anxiety, restlessness at night, and greater weakness; increased griping, tenesmus, and mucous discharge, now also streaked with blood; dryness or tightness in the throat, with hoarseness of the voice; and renewal of the vomiting.

The symptoms continued, in spite of treatment, till the 28th of May, when the mouth was found to be sore, and the throat so uneasy as to impede swallowing. Two days later, the stools, previously bilious, assumed a fatty appearance, owing to the presence of pus, as proved by the microscope. The vomiting and purging were now worse than ever, and the vomiting seldom occurred except after taking food or medicine. The tongue was red and fiery; the mouth and lips excoriated; the anxiety and restlessness very great.

On the 4th of June there were the same symptoms, and a further aggravation of them; but the stethoscope this day also betrayed slight tubercular infiltration at the summit of both lungs, most advanced in the right side—indolent, however, in both. Naturally, tuberculosis, affecting the abdomen as well as the chest, was for a time suspected; and cod-liver oil, with opiate injections, constituted the treatment. Mr. Henzell, however, on this day, began to conjecture that the symptoms he saw were those of slow arsenical poisoning.

On the 8th of June the nostrils were very red, the mouth and lips were much excoriated, and a source of great distress. The tongue was also red and sore. There was uneasiness in the gullet, some sore throat, a tickling irritation at the top of the windpipe, and hoarseness. The anus was excoriated. The patient complained of pain in the stomach, urgent thirst, want of appetite, and frequent vomiting; of tenesmus, griping, and diarrhœa; of hiccup; of intense anxiety, restlessness, and general distress. The pulse was usually above 130, and feeble. The stools had been ascertained by the microscope to contain pus globules and blood discs for three days before.

On the 10th of June the urine, which was scanty, high coloured, and high in density, was ascertained to be albuminous, and to deposit blood discs and casts of uriniferous tubes.

On the 13th of June the face and arms presented an eruption, which gradually put on the character of eczema. The symptoms otherwise continued much the same, and still, as from the first, presented a paroxysmal tendency in point of severity.

On the 14th of June, Mr. Henzell obtained a metallic deposit from the urine by Reinsch's process, which he, however, could not at the time satisfactorily make out to be arsenic. The ammonio-citrate of iron was given as an antidote, and seemed for a time to mitigate the symptoms. The pulse fell from 160 to 120, but the countenance appeared more sunk, and the restlessness was excessive in spite of frequent doses of opium.

On the 23rd, the restlessness and weakness were extreme; the pulse feeble and intermitting; the edge of the tongue ulcerated, and the palate covered with papulæ, or pustules; the hands cold and moist; the vomiting severe; the diarrhœa less so. On this day the patient first mentioned to her attendants a sense of stiffness, numbness, and tingling, which she had felt in the arms for two or three days before. Prior to the 23rd, the urine had presented very much the character formerly described; but on this day what was presented for examination abounded in oxalate of lime crystals, and showed neither albumen, blood, nor tube casts. This was obviously a different urine, substituted accidentally or intentionally. Next day it presented its usual character, except that the albumen had disappeared. In that of the 22nd, Dr. Christison (who had meantime been consulted by the medical attendants) found arsenic unequivocally by Berzelius' modification of Marsh's process.

On the 26th, all the symptoms got worse, especially the vomiting and the tingling and numbness of the hands. The pulse was 144 or 150, very small, and weak. She was evidently sinking. In the subsequent night she was seized with paroxysms of tetanic spasm, gradually increasing in severity and duration, and at length becoming almost incessant. At half-past ten on the morning of the 27th she died, retaining all her mental faculties to the last.

Such were the symptoms; treatment of necessity had little effect. The post-mortem appearances are not less striking.

There was slight tubercular infiltration in the apex of each lung, and, in the left, a small cavity. The trachea and large bronchial tubes were much injected and red. The heart was small, pale, otherwise healthy. The liver was slightly enlarged, saffron coloured, friable, fatty. The interior of the stomach was slightly vascular in its greater curvature; but the smaller curvature presented groups of small vessels gorged with blood, so numerous at its larger end as to render the redness almost universal, and like a sheet of blood under the mucous coat, which was soft and friable. The duodenum was vascular internally, and full of black matter. The jejunum was much in the same state. The ileum was redder still, and throughout the lower third denuded of its mucous coat in many patches, varying in size from a shilling to a crown, and here and there involving its whole circumference. At the latter points the peritoneal coat was bare, thin, and very easily torn. Many mesenteric glands were prominent and black. The colon was everywhere vascular; numerous small ulcers pierced the mucous coat in the ascending and transverse portions; and the rectum was similarly but less extensively affected. The pancreas was vascular, the kidneys and spleen congested, the uterus healthy.

Arsenic was found in the liver, the heart, the lungs, the intestines, the rectum, in a dirty blood-liquid from the peritoneal sac, and in a mass of viscera, consisting of portions of the stomach, the liver, spleen, and kidneys.

The question of the mode of administration of the poison still remains a mystery; but a medico-legal point has been mooted, in regard to the proceedings of the medical men concerned in this case, which is of great interest. It seems that Dr. Haslewood, Dr. Jackson, and Mr. Henzell, the said medical men, suspected poisoning on the 8th of June; but they did not disclose their suspicions openly up to the day of the lady's death, on the 27th. This proceeding called for a severe censure from the judge, who was of opinion that their suspicions ought to have been communicated either to the husband, or in the event of the husband being the suspected party, to a magistrate. Dr. Christison takes a different view, and thinks that in such cases the patient ought to be made the recipient of the suspicion.—*Edinburgh Medical Journal*, January and February, 1856.

Poisoning with Arsenic, with remarkable delay in the appearance of the symptoms.—A strong, healthy woman, about twenty-two years of age, of a rather excitable temperament, had been subjected to great mental agitation, through the threatened withdrawal of an acknowledged admirer, and had suffered from hysterics. She stated herself that she had eaten and drunken but little throughout the week; and on Thursday, the 29th of March, 1855, she took no food or drink, except one cup of coffee at breakfast. She spent the day in walking about without food. At nine o'clock in the evening of the same day she retired to her own room, and was heard, by the two occupants of the adjoining chamber, to be gagging and choking so violently and in such a manner, that one of them knocked at her door and inquired if there was anything the matter with her. She returned an evasive answer, and remained apparently quiet throughout the night. She kept in bed the next morning, and refused her breakfast, but attracted no special attention until nine o'clock, when the same sound of gagging and choking was heard in her room a second time. In the course of the succeeding two hours, an hysterical paroxysm came on. It was then ascertained that she had taken poison, and Dr. Hershey was sent for.

He arrived at eleven A.M., fourteen hours after the first dose had been taken, and two hours after the second. Nothing definite could be learned from her admissions or complaints. She lay in a state of partial cataleptic stupor, occasionally varied with slight muscular spasms. The usual effects of irritant poison were so entirely absent, that Dr. Hershey was induced to order an antispasmodic draught. Of this draught she took some four tablespoonfuls, the first fluid which she had taken for at least thirty-six hours. No change occurred until one P.M., when violent pain and vomiting suddenly came on. The most frequent, and in this instance, at that time, the only positive symptoms of arsenical poisoning had at last presented themselves, sixteen hours after the first powder had been swallowed, and four hours after the second.

As arsenical poisoning was proved, the hydrated oxide of iron was given, and continued in divided doses, to the amount of five ounces; but notwithstanding this, and the free use of the sulphate of morphia and cold mucilaginous drinks internally, and with depletion with cups, and the subsequent application of cataplasms and a blister externally, the pain and vomiting increased in severity until the afternoon of Sunday, the third day. She then appeared to be so utterly prostrated that no hopes were entertained of her recovery, either by herself or her physicians. A paper containing white arsenic was found, and an apothecary was visited, who stated that she had bought the poison of him on the Thursday.

The history given by the patient was, that, with the intent of ending her life, she secretly provided herself with arsenic, retired to her room, after a whole day of fasting and agitation, and attempted to swallow a teaspoonful of the dry arsenic powder, and was so irritated in the throat by it as to alarm her neighbours. She coughed out a part of it, but managed to retain about a teaspoonful. She lay down, as she expected, to die, but spent the night without change or sleep. The next morning she swallowed another half-teaspoonful in the same manner as before, and with the same difficulty. She felt no pain until she began to take freely of drinks.

On the Sunday evening, the vomiting and pain ceased, and reaction commenced, accompanied with extreme feebleness, cool moist skin, temporary cataleptic spasms, inflammatory tenderness of the pharynx and whole intestinal region, going off with tormina, tenesmus, bloody stools, and strangury, followed in a few days by an acne-like eruption on the skin. The patient was restored to complete health in three months, no impairment of digestion remaining behind.

Cases of this kind, where the symptoms of the poison are so long delayed, are of great interest in criminal inquiries. The evidence in the above case is faulty in an important particular. It affords no chemical proof that arsenic was swallowed. The evidence from symptoms is however strongly in favour of the truth of the statements of the patient.—Dr. HARTSHORNE, *in the Philadelphia Medical Examiner*, December, 1855.

Poisoning by Oxalic Acid, taken in the solid state.—A case, in which oxalic acid was swallowed intentionally in the solid state, is published by Dr. Barker. Death in this instance was the result. "It appeared in evidence, that the deceased was only sixteen years old, and lived with his brother at Luton, who had charged him on the 4th instant with abstracting money without his knowledge and consent. This charge the accused at first denied, but afterwards acknowledged to be correct. He was of a passionate and revengeful disposition.

"After this, the lad proceeded at first to Dunstable, and afterwards to the town of Markyate Street, where he purchased one pennyworth of oxalic acid, between seven and eight o'clock in the evening. Between eight and nine o'clock the same evening he was found lying in a lane, moaning, and with some coloured fluid (as if from vomiting) close by him.

"Mr. W. A. Hubert, surgeon, of Markyate Street, was passing by the lane at the time he was found, and was called to him. While he was with him, he vomited fluid similar to that which was upon the ground. He was insensible, pulseless, and his lower jaw was spasmodically closed. Cold water dashed upon the face restored sensibility, and relaxed the jaw. The boy said that he had taken oxalic acid; *that he had eaten it, and did not make any solution;* and that his intention was to kill himself, because he had been charged with taking money.

"The deceased was carefully conveyed in a cart to his residence in Luton, when Mr. Frederick Clarke, surgeon, of that place, was sent for. He saw him between ten and eleven o'clock the same evening; found him but partially sensible, very drowsy, and in a state of collapse. He had vomited bloody matters, as well as some white fluid, which did not contain any crystals. His tongue and lips were unusually pale, but there was no excoriation. He could be sufficiently aroused to state that he had purchased half an ounce of the acid; that he had taken about a quarter of that quantity in the solid state; and that he was sorry that he had taken it.

"He died about half-past three A.M. on the 5th. The following were the post-mortem appearances, as detailed by Mr. Clarke at the inquest. The tongue was dotted over with white specks; the œsophagus was not inflamed. The stomach was extensively disorganized, and had the appearance of gangrene in some parts. The mucous membrane was detached in some parts, and, in consequence, the muscular coat exposed to view. The verdict was *felo de se.*"—*Association Medical Journal*, November 30th, 1855.

Poisoning by Strychnine: Battle's Vermin Killer.—Mr. Nunneley, of Leeds, has recorded a case of great moment as illustrating some peculiar points in cases of poisoning by strychnine.

The person, a woman, aged twenty, in whom the case occurred, was a stranger, at Leeds, and died under suspicious circumstances during the night. A short time before her death she said she had taken poison, but gave no clue as to its nature or how she had taken it. After her death, "on examining her dress pockets, two empty small paper packets were found, the outer and inner bags of one packet of 'Battle's Vermin Killer,' according to the printed label upon them; they were folded up, the inner within the outer. There was also found a box of pills and a bottle of medicine, out of which one dose had been taken. These, it was ascertained, had been obtained for her a day or two before from some one at Huddersfield for a venereal complaint, and were of a simple character." The girl was refused a bed by the landlady of the Bean Jug public-house in Leeds, when a poor man, who lived near, out of sheer pity took her to his own cottage. She sat up till half-past twelve, and then retired to her chamber, which was small. "She did not eat or drink before going to bed, when she appeared to fall asleep, at least Mrs. Sheard (the man's wife), who said she lay awake for an hour, thought so, and was quite certain the girl did not get out of bed or move much about, and that there was no water or other fluid in the room, nor any vessel to drink out of. The man retired to bed after the woman thought her asleep. At a quarter to two o'clock Mrs. Sheard was awoke by the moaning of the girl, who was then, or immediately after, severely convulsed; the convulsions continued at frequent intervals for an hour. For another hour she was comparatively free from convulsion, and talked calmly and distinctly about her parents, and the conduct of her lover, and her having done something wrong, but refused to say what. During this time she drank freely of cold water, which was fetched from the room below, when a violent convulsion came on, in which she died. From the evidence of Mrs. Sheard it was very difficult to determine the exact form of the convulsions, as she did not at the time appear to consider the throwing about of the arms as convulsions; but from what Mr. Nunneley could make out, he had no doubt the lower extremities were principally rigidly extended, while the upper were strongly convulsed. It was owing to a more violent convulsion not very long before the girl's death, that Mrs. Sheard became seriously alarmed, and sent her husband to seek a medical man; during his absence death occurred, the girl continuing sensible until the last minute or two." Mr. Nunneley supposes that the poison taken was "Battle's Vermin Killer," which is sold in small packets, each packet consisting of an inner and outer paper envelope; it is of a blue colour, and appears to be composed of strychnine mixed with some farina, and coloured with Prussian blue; the quantity of strychnine being, so far as Mr. Nunneley could ascertain, about three grains to the whole packet. He is inclined to think that she had put the poison upon a small portion of bread and butter, and eaten it so soon as she found all quiet in the room, under the impression that she should die suddenly from it.

Mr. Nunneley "saw the body about twelve hours after death. The muscles were then flaccid, so that the want of rigidity mentioned was not owing to any post-mortem change; besides, the weather was not very hot. The immediate cause of death was clearly asphyxia, as all the appearances in the body showed. The softening of the medulla spinalis, which some experimenters have mentioned as being seen after death from strychnine, certainly was not found in this case.

"A post-mortem examination was made thirty hours after death. The body was well formed and plump, but very dirty, with considerable evidence of severe syphilitic disease. The left labium was greatly swollen, ulcerated, and indurated. The urine escaped through a large ulcer in the vagina, which communicated with the neck of the bladder.

"Not more than the usual post-mortem discoloration was present. There was much foam about the mouth and nostrils. There was scarcely any rigidity—less than usual in the lower extremities; none whatever in the upper arms, chest, or body; little in the thighs, hands, and feet; the legs and lower arms being somewhat rigid. On removal of integuments, the muscles seemed to be very flaccid.

"*Chest.*—The *lungs* were emphysematous, from rupture of air-cells. The posterior part of the lower lobes was congested, and rather, but not excessively, œdematous; otherwise they were quite healthy. The bronchial tubes were filled with frothy mucus, as were the larynx and upper part of trachea, the lining membrane of which was so congested as to be quite crimson in colour. The small muscles of the larynx and glottis were more firmly contracted than those of any other part of the body. No effusion into the pleuritic cavities or pericardium.

"The *heart* was perfectly flaccid, containing, on the right side, a large quantity of dark fluid blood, with some little soft pale fibrinous clots in the ventricle; on the left side, only some small quantity of dark soft coagulum. The structure of the heart was healthy.

"*Abdomen.*—The *stomach* was distended with flatus, but containing not more than three ounces of a pale bluish-green coloured thick gruelly fluid, without any appearance of other food. Near the larger end were some few spots of extravasation in the mucous membrane, which in other parts was pale and natural in texture."

The intestines were pale, containing little except some bilious mucus and flatus. The spleen was congested; the other abdominal organs normal.

"*Head and Spine.*—The vessels of the scalp, dura mater, and pia mater, were much distended with dark fluid blood. Some serous effusion was present in muscles of the pia mater, upon the convolutions; none in the ventricles. Substance of both cerebrum and cerebellum perfectly natural, except, perhaps, having more vascular points on being sliced. Medulla oblongata firm and natural. Spinal veins congested; the medulla spinalis itself natural, except perhaps (but of this Mr. Nunneley was doubtful) rather softer than usual at the bulbous expansion about the seventh cervical vertebra.

"An analysis was made one hundred and fourteen hours after death, and eighty-four after examination, when strychnine was most decidedly detected in considerable quantity, by the method mentioned in toxicological works. The interior of the stomach was well washed with distilled water, which, being added to the contents taken from the stomach, and acidulated with sulphuric acid, was boiled for some little time, and filtered; then neutralized with lime, and again filtered, and evaporated to dryness; a portion of this residue, on being dissolved in spirit, gave, on the addition of a small quantity of nitric acid, the characteristic brown tint of strychnine."

Mr. Nunneley opines that if a dose of strychnine taken into the stomach "be sufficient to cause death within four hours, it may commonly be found; and that it does not necessarily undergo decomposition for a considerable period after death, the present case furnishes conclusive evidence."—*Association Med. Journal*, Jan. 26th, 1856.

Lard as an Antidote to Strychnine.—Dr. W. N. Pindell relates that, being annoyed by some dogs, he determined on poisoning them. A piece of meat containing one grain of strychnine was placed on the ground beside some lard. A dog was seen to eat both the meat and the lard, without being poisoned. The next night, pieces of meat were laid down with strychnine; the following morning, three dogs were found dead. In nine instances, in which lard was given with the strychnine, the animals did not die; in eleven, where no lard was given, all died. Half a grain was sufficient to produce death; but three grains failed when lard was used.—*American Journal of the Medical Sciences*, October, 1855.

The Frog Test for Strychnine.—Dr. Marshall Hall records in the 'Lancet' the results of two experiments. He dissolved one part of the acetate of strychnine in 1000 parts of distilled water, adding a drop or two of acetic acid. He then took a frog, and, having added to one ounce of water $\frac{1}{100}$th part of a grain of the acetate, he placed the frog in the solution. No effect having been produced, $\frac{1}{100}$th of a grain was added; in an hour another $\frac{1}{100}$th of a grain; making in the whole about the thirty-third part of a grain. In a few moments the frog became violently tetanic, and though taken out and washed, died in the course of the night.

A second frog was placed in one ounce of distilled water, to which the $\frac{1}{100}$th part of a grain of acetate of strychnine was added. At the end of the first, second, and third hours, other similar additions of the acetate were made, no tetanic symptoms having appeared. At the end of the fifth hour, the frog having been exposed to the $\frac{1}{50}$th part of a grain, tetanic symptoms came on, and, under the same circumstances of the removal and washing, the frog died.

Dr. Hall thus was able to detect the $\frac{1}{50}$th part of a grain of strychnine. He adds, in a subsequent note, that in two further experiments, the $\frac{1}{500}$th and $\frac{1}{1000}$th part of a grain were detected.—*Lancet*, and *Veterinarian* for February, 1855.

Poisoning by Vapour of Carbon.—Action of the Actual Cautery as a Stimulant.—Dr. Faure, in making experiments on the effects of the actual cautery as a stimulant in cases of asphyxia, has been led to make some observations on the phenomena of asphyxia by the vapours of carbon.

A number of dogs and cats were sacrificed, sometimes in a room used as a laboratory, sometimes in a closed vessel containing about 125 cubic feet, into which the gas was introduced by means of a tube. The symptoms produced are modified by certain conditions, which are pointed out.

A. When the atmosphere is excessively hot and dry, the peculiar effects of poisoning by carbon vapour are marked; the symptoms are those of suffocation, exactly resembling those produced by sudden but incomplete impediment to respiration by a mechanical obstacle. Sometimes the animal dies in convulsions; at other times death is attended by a regular and progressive diminution of respiration. The blood is bright red, and fluid.

B. When the temperature is moderate or low, there are generally no convulsions; death takes place by a gradual impairment of respiration. The first symptom is headache, which soon becomes intense. There are almost always vomiting, and evacuation of urine and fæces. The action of the heart and lungs gradually becomes weaker; and it is not possible to determine the exact moment at which death occurs.

Cases of this kind are met with when asphyxia occurs in a room too large for the air to become much heated; when there is a fissure capable of establishing a communication with the external atmosphere; or when the vapour is evolved at a distance, and has to reach the apartment through holes in the wall, under the floor, through tubes, drains, &c. In several recorded cases, the vapours emanating from a fire at a distance have at first produced mere headache and other troublesome symptoms; and these, increasing in intensity with the repetition of the cause, on the third or fourth night have terminated in death. In another instance, recorded in the 'Annales d'Hygiène,' the fumes arising from the burning of some woodwork in connexion with the fire-place, produced symptoms of asphyxia in several persons who visited the room of a lady who had been found dying from suffocation.

Dr. Faure has often asphyxiated animals, by burning only a small quantity of carbon at a time, so that the temperature was not sensibly changed. They remained apparently well for about an hour or an hour and a half; they then began to turn round, fell down, and after a prolonged convulsive attempt at respiration, they died.

In this second class of cases, the blood is sometimes quite fluid; sometimes there are soft coagula in the right side of the heart.

Sometimes the vapour of carbon, inhaled under the same conditions, produces very different effects. One individual or one animal is killed; while another, in the same apartment at the same time, will escape, or may die under a different class of symptoms, and present different post-mortem appearances.

This is important in a medico-legal point of view. Two persons who have attempted to commit suicide by asphyxia, are found in a room; one is dead, the other is scarcely affected. The suspicion arises that the latter has murdered the former, and has found some means of preserving himself from injury. Cases of this kind have been recorded.

Some years ago, a man was found lying dead on a bed. A girl, who inhabited the same room, stated, that after a dispute with him on the previous night, he had struck her with a knife, and that she had fallen insensible, with her face against the door. In the night, on recovering her senses, she found the man dead, and charcoal still burning. She tried to hang herself, but the rope broke, and she fell down. When discovered, she complained of violent headache and *malaise*, and felt quite stupefied.

The question in this case was, Did the girl really remain in the room all night, or did she leave it for a time, either by the door or by a trap-door which led into a garret? Dr. Faure says that, however apparently well the doors and windows of a room may be closed, there will still be a current of air from without, especially when the atmosphere of the room is heated. Again, when the hot air in a room approaches the ground, it becomes condensed, and leaves room for a powerful influx of the air from without. The girl may then have breathed, while she was lying near the door, an almost pure atmosphere, in spite of the intensity of the vapour with which the room was filled.

In the investigation of cases of this kind there is much difficulty, depending in great measure on the circumstance that persons have been found asphyxiated in rooms where an entire pane of glass was wanting in the window; while, on the other hand, a single current of air, in a favourable direction, has preserved life in a well-closed room. M. Malgaigne, in making some experiments on himself, was surprised at the absence of results, until he was able to account for them by the presence of some small chinks through which air entered.

Symptoms.—At a temperature of from 70° to 85° Fahr., that at which suicidal asphyxia most commonly occurs, the symptoms appear in the following order:

A. Headache, general *malaise*, noises in the ears, loss of muscular force, dryness of the throat, tendency to vertigo and to turn round, vomiting, and loss of consciousness. The action of the heart is at first accelerated, but soon becomes slow; there is generally evacuation of urine and fæces in abundance. The skin is quite insensible to pricking or pinching; but the least contact of a hot iron arouses the patient. The patient may recover spontaneously on exposure to the open air.

B. The action of the heart is alternately raised and depressed; the pupils are insensible to light; the conjunctivæ are unaffected by irritants; a large quantity of sanguineous froth escapes from the bronchi; there are cries and convulsive movements. Anæsthesia is still more complete; the hot iron produces no effect on the limbs and on the lower part of the body, but is still felt under the clavicles and in the axillæ.

C. The movements of the heart become more and more rare. In dogs, whose normal pulse is from 89 to 92, they decrease to 15 or 18. The thoracic movements are almost invisible; but from time to time there is a deep inspiration, generally accompanied by a groan. At last, the nostrils alone move very feebly; the entire body is insensible to the red-hot iron; and death is inevitable.

In commenting *seriatim* on these phenomena, Dr. Faure remarks on the great importance of attending to the amount of *cutaneous sensibility*, in regard to prognosis. Insensibility appears to possess two degrees, in the first of which it resists ordinary irritants, but not the red-hot iron; while in the second the hot iron is not felt. At first, the skin of the limbs may be pinched with impunity, while signs of pain may still be elicited by pinching under the clavicles or in the axillæ; and, at a later period, the application of the red-hot iron produces the same comparative effects.

The fact that the sensibility to the application of intense heat remains longest in the upper part of the chest, has been used in the recovery of asphyxiated persons by MM. Deconfrevon, Florent Cunier, Aken, and others.

There is a great difference between the action of mechanical irritants and of the red-hot iron. Dr. Faure has often removed portions of integument from the chest without producing pain, while the least contact of the cautery was sufficient to arouse the animal. Often dogs which have overturned the furnace have remained insensible on the burning carbon, and yet a slight burn with the hot iron has made them cry out: provided, however, that the cautery were applied in the open air, for it might be applied to an animal within the gas-holding chamber without producing the least sign of consciousness.

As sensibility is lost in a direction proceeding from the extremities towards the upper part of the chest, it returns in the opposite direction. This, Dr. Faure has been able to observe by means of the actual cautery.

At the last limit, when the cautery has no action except on a very limited point, it does not produce pain, but a series of muscular movements, resulting in an inspiration; and it is only after the recurrence of several of these that signs of pain are felt.—*Archives Gén. de Méd.*, Janvier, 1856.

Softening of the Stomach: suspected Poisoning by Sulphuric Acid.—On the 28th of June, 1853, the Rev. M., a Hungarian exile, died at F——, in the duchy of Nassau, having for ten days previously been under treatment for dyspeptic symptoms, attended with gastric tenderness and diarrhœa. Shortly before, his suspicions were aroused that poison had gradually been administered to him by one of his relatives with whom he dwelt. This suspicion was increased by the circumstance that another relative had taken a portion of the suspected coffee to a physician for analysis, and that the latter had found, on chemical examination, a large quantity of sulphuric acid. In consequence of this, a judicial investigation was set on foot. In the mean time, certainly two months after the discovery of the sulphuric acid in the coffee, Mr. M. died; and a post-mortem examination was made by three physicians, who were purposely kept in ignorance of the facts above related.

The body was that of a man aged from seventy to eighty. All the cavities of the body presented evidences of senile atrophy.

In the head, there was passive (venous) hyperæmia of the membranes; the substance of the brain was hard, and the ventricles dilated; and there was extensive serous effusion both in the ventricles and between the cerebral membranes. This effusion Dr. Santlus (who reports the case) believes not to have been apoplectic, but the result of the senile atrophy of the brain, which, according to Rokitansky, gives rise to congestion and effusion in order to fill the vacant space. The quantity of the effusion was an argument against its having been the result of putrefactive changes.

The lungs had adhesions both to the pericardium and the pleura. They were voluminous, and dotted with dark-blue spots, especially posteriorly. They were crepitant, and had a soft feel, "like eider-down." At the upper part of both lungs, the pleura was in parts contracted and hard; the lower and lateral parts of the lungs were crepitant on section, collapsed, and contained only a moderate quantity of thin watery red fluid; the posterior parts of the lungs were not collapsed, and contained a large amount of frothy blood; the posterior part of the right lung sank in water. The pleura was adherent to the upper part of each lung, and was thickened; but there were no traces of tubercle or other morbid deposit.

The pericardium was healthy. The heart was of the size of a fist; its walls were thin; the valves were healthy; and the aorta was free from morbid deposit. The blood contained in the heart was dark and fluid, presenting only some small loose coagula.

On opening the abdomen, the bladder was found full of a large quantity of urine, having a strong smell. The transverse colon lay in front of and concealed the stomach. The intestines, especially the large, were full of gas. The ascending colon presented six or eight dark-grey spots on a single prominent convolution. The stomach was very soft, and tore easily, allowing a large quantity of fluid contents to escape. It was tied at the pyloric end, and removed, with about half of the œsophagus. The lining membrane of the stomach was discoloured, and easily separable. At three fingerbreadths from the cardia, on the anterior surface, was a greenish spot, which, on incision, was shown to be due to ecchymosis. The mucous membrane, at its passage from the œsophagus to the stomach, was of a dark-red colour, extensively detached, and contained some very large dark-red vessels. This vascular development produced an appearance of dark-red spots, especially in the œsophageal part of the cardiac region. At this part was an erosion of the size of a farthing [a small kreutzer], penetrating as far as the muscular coat, and leaving a conspicuous deficiency in the mucous membrane. This was the only perforation of the kind observed in the whole stomach. In the neighbourhood of the perforation the mucous membrane appeared as if it had been burnt; the discoloration extended towards the pylorus, and ended in a dirty green-coloured spot of the size of a small hand. At the upper part, towards the small curvature, the colour became clearer, and passed into a light muddy red, showing in parts the colour of a healthy mucous membrane. Over single parts of the detached mucous membrane were scattered small, clear, red, sanguineous spots, especially in the discoloured portion already described as extending from the cardia to the pylorus. In the lower part of the cardia, corresponding to the greater curvature, and opposite the œsophageal opening, all the coats of the stomach were so decomposed that they could be broken up between the fingers, and were so thin that, including the peritoneal coat, they were not thicker than paper. The serous coat of the stomach, in general, was also much softened.

The duodenum presented no morbid appearances beyond some red spots here and there. The large and small intestines were healthy. The liver, spleen, and kidneys presented no appearance worthy of especial notice, except that the left lobe of the liver was discoloured at its lower part.

The stomach, with a portion of the œsophagus and a piece of the left lobe of the liver, were removed for chemical examination. These parts were tested successively for sulphuric, nitric, hydrochloric, and phosphoric acids; iodine, bromine, and various metals, earths, and alkalies, without finding any traces of these. Some portions of the stomach and liver were reserved, in order to be tested for organic poisons; but of the result of this examination we do not find any account.

After a comparison of the morbid appearances observed in this case with those of softening and post-mortem perforation of the stomach, as described by various pathologists, Dr. Santlus concludes in the following words:—"That poisoning, although possible, cannot be judicially proved to have occurred in the case of Mr. M., in the absence of manifestations peculiar to poisoning in the symptoms, post-mortem examination, and chemical analysis; and that the supposition of poison is further opposed by the fact that the deceased died in a state of senile atrophy, of which the softening of the stomach was only a strongly-marked instance."—*Henke's Zeitschrift für die Staatsarzneikunde*, 1855.

Poisoning by swallowing finely-divided Hair.—Dr. A. A. Dornseiffen relates the following case:—A woman, in good circumstances, returned from Java to Holland in 1840. A short time before her return she began to feel pain in the stomach and intestines, which was increased by pressure. At first the pain was not increased by eating, but latterly the act of taking food produced vomiting. Her appetite gradually was lost; and, from being plump and good-looking, she became, soon after her return, thin and pale. For four years, the pain by day and night was insufferable, and could only be relieved by bending the body forwards. In 1844, after eating oysters, the patient had vomiting and diarrhœa; after which the pain was diminished. It continued, however, in various degrees of intensity, till 1850, when a physician determined to treat her for tænia, by decoction of pomegranate root and castor oil. In the evacuations were found a number of finely-divided hairs, from a few lines to an inch and a half in length. When these had been evacuated, the pain at once ceased, and the patient regained her former good health.

The woman remembered that, shortly before her return from Java, being ill, she had given to her by a Malay girl a drink named *jambu;* this was thick and opaque, and she did not know any other way in which she could have swallowed the hairs. She believed that she had been poisoned by the hairs of the tiger, which are said to be used for that purpose by the natives of India. The hairs, however, on being examined, more resembled those of the mane or tail of a horse or ass, or of an animal of the hog kind—perhaps the babyroussa.—*Nederlandsch Weekblad*, Sept. 1854; and *Schmidt's Jahrbücher*, 1855.

On Oils, as promoting the Poisonous Action of Cantharides.—The solubility of cantharidin in oils has led Orfila, Taylor, Christison, Mitscherlich, Oesterlen, and other toxicologists, to promulgate cautions against the use of fatty matters in poisoning with Spanish flies. On the other hand, Clarus, in his "Handbuch der Specielle Arzneimittellehre," considers that there are no grounds for this caution. Professor Schroff, of Vienna, who has performed a number of experiments on the action of Spanish flies and cantharidin, has published the results of the administration of these substances in combination with oil. Three rabbits—two of four and six months old respectively, and one full-grown—had the poison given to them. To the first was administered 15½ grains of powdered cantharides, rubbed up with olive oil; to the second, 7¾ grains, prepared in the same way; and to the third was given 1½ grain of cantharidin, similarly prepared; to each were also administered several table-spoonfuls of olive oil. The results, compared with cases in which the poison was given without oil, were the following:

1. The symptoms during life were identical in both classes of cases.

2. Death occurred soonest in the cases in which oil was given. Fifteen grains of cantharides with oil caused death in four hours; without oil, in five hours. Seven and a half grains with oil destroyed life in nineteen hours; without oil, in twenty-six hours. One and a half grain of cantharidin, with oil, killed the animal in four hours; without oil, in from seven to ten hours.

3. The post-mortem appearances give evidence of less action of the poison on the parts with which it comes in contact, where oil is given, than when the poison is taken alone. In the three rabbits poisoned as above related, there was no vesication of the tongue; and the inflammation of the stomach and intestines was less than in cases of the other class. On the other hand, the signs of inflammation of the urinary system were more strongly marked where oil was given. The bladders were contracted and empty; the kidneys were much injected; and the urinary mucous membrane presented on its surface a large quantity of epithelium, nuclei, and blood-corpuscles.

The practical deduction from these experiments is the confirmation of the prohibition of the use of oil in cases of poisoning by cantharides.—*Wochenblatt der Zeitschrift der K. K. Gesellschaft der Aerzte zu Wien*, Nos. 48 & 49, 1855.

On Poisoning by Turpentine Vapour.—M. Marchal de Calvi related to the Academy of Sciences in Paris, on December 10th, the case of a woman who had lived for some days in a newly painted room. The first symptom she experienced was colic, but soon she became prostrated; the face was deadly pale, the eyes sunken, the lips could scarcely be moved, the breath was cold, the voice was lost, the limbs were cold, the pulse almost imperceptible, the countenance anxious. The intellect, however, remained perfect, and the patient felt as if she were about to die. Under the use of external and internal stimulants she rallied, but did not perfectly recover for a month.

Some experiments made by M. Marchal in conjunction with M. Mialhe, tend to show that vapour of turpentine produces poisonous effects on men and animals. The conclusions at which the author arrives, are: 1. That white lead is fixed in paint, and is in no way concerned in the production of the poisonous symptoms arising from inhabiting a newly-painted room. 2. These symptoms are due to the vapour of turpentine. 3. The danger is the same whether the base of the paint be lead or zinc. 4. There is danger of poisoning by turpentine so long as the paint is not perfectly dry, and it is safest not to inhabit a newly-painted room until all smell has disappeared. 5. Poisoning by turpentine enters into the same category as poisoning by the emanations from flowers. 6. The emanations from flowers act in two ways—idiosyncratically or as poisons. 7. The action of turpentine is chiefly depressing. 8. Energetic stimulation constitutes the best treatment. The peristaltic action of the bowels should be excited. The two last observations, being formed on insufficient data, are not absolute.—*Gazette Médicale de Paris*, December 20th, 1855.

Poisoning by Sulphuret of Carbon among Workmen in India-Rubber Manufactories.—At the meeting of the Academy of Medicine in Paris, on January 15th, M. Delpech stated that he had arrived at the following conclusions with regard to the workmen in India-rubber manufactories.

1. That such workmen are liable to accidents, which consist in (*a.*) loss of appetite, nausea, vomiting, diarrhœa, or constipation; (*b.*) disturbance of the intellectual functions, hebetude, loss of memory, extreme restlessness, and unaccountable violence; (*c.*) more serious disturbance of the nervous functions—cephalalgia, vertigo, disturbance of sight and hearing, impotence, and various forms of paralysis.

2. That experiments made on men and animals, who are affected in the same way, lead to the conclusion that the symptoms are due to the inhalation of the vapour of sulphuret of carbon.—*Gazette Médicale de Paris*, January 19th, 1856.

Antidotes for Salts of Copper.—Dr. Scharder of Göttingen has recently conducted several experiments in relation to antidotes for copper salts. *Hydrated proto-sulphuret of iron*, which is recommended by M. Mialhe, did not prevent the death in the night of some rabbits, although given in sufficient quantity to decompose the three and six grains of acetate of copper which had been administered. The sulphuret alone was alike inactive in rabbits. The autopsy of the rabbits only showed that the heart and great bloodvessels were filled with liquid black blood. *Hydrate of magnesia*, contrary to the results obtained by M. Roucher and others, was not found efficacious. *Sugar* had the same negative results. *Prussiate of potassa* was tried in several cases on dogs and rabbits: the results appear doubtful. *Albumen and milk* taken in excess provisionally neutralize the poisonous copper salts; but the caseate or albuminate of copper formed must be purged off.—*Deutsche Klinik*, No. 4, 1855, and *Chemist.*, June, 1855.

Poisoning by Gamboge.—A delicate Parsee female, aged nineteen, took about three drachms of pipe gamboge for the purpose of self-destruction. Five hours afterwards she was collapsed from the violent purging and vomiting, which commenced about two hours after taking the drug. The matters ejected were of a deep yellow colour, and the pain and suffering very great. Stimulants were given and frictions employed, and the collapse passed off; but the straining, griping, and evacuations of yellow mucus continued for some days. She ultimately recovered.—*Transactions of the Medical and Physical Society of Bombay*, 1855.

II. MISCELLANEOUS.

Suicide and Suicidal Mania.—M. Brierre de Boismont has published an interesting work on 'Suicide and Suicidal Mania.' After a notice of the doctrines of the ancients regarding the practice of self-murder, he proceeds to speak of its causes, its physiological and symptomatic characters, nature, and treatment.

Predisposing Causes.—Among these are hereditary tendency, especially with the insane; also climateric and meteorological influences. Suicide is more common among males than females, and in large centres of population than where the people are thinly scattered; it is, however, frequent enough in rural districts. It occurs at all ages, very rarely in youth, most frequently at the age of from thirty to fifty. Contrary to the doctrine of Esquirol, suicide takes place also in old age. It is more frequent in unmarried and widowed persons. Certain professions and education, and still more poverty and immorality, favour the tendency to it.

Exciting Causes.—These are drunkenness, want, misconduct, insanity, domestic grievances, love, gambling, pride, *ennui*—the latter being a sign of an antiquated and palled civilization. In the early ages of our era, Seneca and St. Chrysostom pointed out the tendency to suicide arising from a disgust of life; and in the present age, Rousseau, Goethe, Chateaubriand, and Lamartine reveal in their works the same state of feeling. And not only are restless and dreamy minds—poets and artists—subject to *ennui*, but even such men as Dupuytren and Napoleon have experienced the feeling. In opposition to Esquirol, M. Brierre holds that *ennui*, even with a tendency to suicide, does not constitute insanity, unless there is manifested derangement of the moral and intellectual faculties.

Last Sentiments of Suicides.—Some commit suicide coolly and resolutely; others only after repeated hesitation. Very few resist the desire to make known their feelings at the time of leaving the world. The social feelings are manifested in adieux to families and lovers. Many suicides acknowledge themselves the authors of their own death, and explain their motives for the act. Some express pain at not having been able to overcome bad habits; they leave to their enemies messages of forgiveness and reconciliation; and give directions regarding their funerals. Others utter bitter regrets of life—complaints, recriminations, insults, threats. Sometimes the writings found after death show that the reason for suicide has been most trifling; in others insanity is apparent. Among the sane, the motives assigned are taken from passions, desires, regrets, &c.—in a word, from ordinary phenomena of life. With the insane, the tendency to suicide is manifested by hallucinations, illusions, delirious ideas—by, in fact, a true diseased state.

The number of suicides is greatest in Paris, and greater in large towns than in rural districts. In France, there is one suicide in every 13,461 inhabitants; the number is at its maximum in the north, decreases in the east, west, and centre, and is least in the south. More suicides are committed by day than by night, and more in summer than in winter.

Physiological and Symptomatic Characters of Suicide.—In sane persons, suicide is in youth almost always instantaneous, and is determined by emotions. Melancholic characters are most predisposed to it. In adult and old age, the signs of a suicidal tendency are derivable from the character of the individual, his temperament, ideas, education, profession, organization, and degree of sensibility or irritability. The expression of the countenance often reveals the fatal resolution. A first attempt at suicide is no guarantee of its non-recurrence.

In the insane, the temperament, character, antecedents, and hereditary tendencies must be taken into account. Most suicides are committed under the depressive form of insanity—as in melancholia. Hallucinations and illusions are very common: such as the notion of having enemies; of being poisoned or persecuted; exaggerated dread of hell, of the police, imprisonment, judgment, &c. The refusal of food and drink is often associated with the notion of poison. Lesions of general and cutaneous sensibility exist in a large number of insane suicides. Suicide is sometimes acute. It may be preceded by homicide: it may arise from irresistible morbid impressions. In the insane addicted to suicide, science may effect a tolerably large proportion of cures.

Nature of Suicide.—M. Brierre de Boismont seeks to show that suicide is often a voluntary act, maturely reflected on and coolly executed, in perfect freedom of mind. In discussing the influence of ideas and beliefs, he cites the instances of a large number of celebrated men who have thought of suicide, and have gone so far as to commence its commission; but he by no means excludes the influence of insanity in producing this kind of death.

Treatment.—In the sane, religion, morals, the performance of duties, and a wise control over the passions, are the best preventives. The avoidance of sadness, the possession of a family, and the exercise of a profession, are for youth the best means of combating the suicidal tendency. Reasoning, moral measures, and amusement, may be successful in adult age. Solitude is often a cause of suicide in the old, and the tendency must be removed by surrounding the individual with a family. Imitation—a sort of moral contagion—contributes to suicide: hence entertainments and books which treat of it should be avoided. Punishment appears to have no effect on suicide, especially among civilized nations. M. Brierre thinks that confession and the cloister have rescued many from the committal of suicide.

In the insane, the tendency to suicide must be combated by isolation, therapeutic agents, and coercive measures. Prolonged baths and repeated shower baths are generally indicated in suicidal maniacs. Cold affusions, tonics, antispasmodics, dry friction, bleeding, blisters, &c., may be also employed with advantage. When food is refused, it is sometimes necessary to administer it through an œsophageal tube. At the period of convalescence, a visit to the country, travelling, amusements, gymnastic exercises, and intellectual and manual labour, hasten and perfect the cure. Recovery from suicidal mania may be due to a physical or moral crisis. A diversion of the moral feelings may produce a cure in some cases where the disorder is stationary. Children born of suicidal parents require to be subject to a preservative treatment, consisting in a special intellectual education, directed with wisdom and perseverance by proper persons.

The acute period of suicidal mania being once passed, *family life* is of great service. By the term family life, M. Brierre means, not the return of the patient to his own family, but the assembling of patients of both sexes, under the constant inspection of one of the chiefs of the establishment. This intimate and familiar life, presenting as it does almost the normal appearance of society, has great advantages, especially with melancholic monomaniacs, over measures of seclusion. Persons engaged in the treatment of the suicidal insane should recognise as their objects—the exercise of a constant influence over their patients by reasoning, advice, and exhortation; the fulfilment towards them of the duties of the consoler and the friend; and the continued manifestation of marks of interest, benevolence, sympathy, and devotion. These functions do not require high mental qualifications so much as the possession of good moral qualities. This course of treatment results in arousing the patients from their morbid ideas, and in bringing them back to the realities of life; and by it M. Brierre has obtained permanent cures in cases where all other means had failed.—*Gazette Médicale de Paris*, February 23rd, 1856.

Death by Hanging: Question as to Period: Evidence afforded by post-mortem Changes.—The following case elicited some difference of opinion among medical jurists:—Duroulle, a person who had studied medicine and law, and who lived in a village in Normandy, was accused of having, by the aid of his maid-servant, with whom he lived in concubinage, caused the death of his wife by strangulation. He had frequently ill-treated his wife, who had, notwithstanding, made a will in his favour. On February 27, 1854, at about 9 P.M., according to the statement of the accused persons, Madame Duroulle, after a slight altercation with her husband, went into a garret; and in a short time a noise was heard as of a heavy body falling on the floor. A girl named Neveu, the accomplice of Duroulle, went up, and found Madame Duroulle lying on her face on the ground. On raising her, she discovered that she had a cord round her neck, and was dead. Duroulle placed the body on a mattress, removed the cord, and sent for the mother of the girl Neveu, and for a physician.

It seems to have been ascertained that the cord was one used for drawing curtains together; that one end of it was attached in the garret at a height of one mètre and eighty-five centimètres (about two yards) from the ground. Two ends of the cord—broken, according to some witnesses,—cut, according to others—hung from the pulley, at a distance from the ground of one mètre and forty-five centimètres (somewhat more than a yard and a half). Immediately beneath, there lay on the ground a small quantity of fæcal matter, and of urine which had not been absorbed.

In the indictment, it was asserted that Madame Duroulle had died, not on the 27th, but on the evening of the 26th. Witnesses stated that at this time they had heard cries of distress in the interior of the house. Duroulle was accused of having strangled his wife, and of then having kept the body clothed and covered, so that on the evening after death it retained traces of warmth.

M. Devergie, who gave evidence on part of the defence before the Imperial Court of Rouen, stated it as his opinion, that the death of Madame Duroulle had been caused by the application of a cord round her neck; that the event took place on the 27th February; and that the appearances denoted that she had committed suicide.

On the other hand, M. Tardieu, with the same evidence before him, arrived at the following conclusions:—1. The death of Madame Duroulle could not be certainly and exclusively attributed to hanging. 2. There was no positive proof of suicide; but the state of the body gave more than one reason for believing that the act of suspension had been performed by other hands. 3. The appearances presented by the body, especially the want of rigidity and the partial cooling, would in no way warrant the supposition that she had died recently when the body was first examined. On the other hand, these appearances would serve to denote that death had occurred at an earlier period. He believes that the body may remain warm for twenty-four hours and longer. Cadaveric rigidity may set in when a high degree of warmth is still present, and may last for so short a time as to entirely cease before the body is quite cold.

M. Devergie asserts that the post-mortem phenomena constantly follow in the same order, differing only in the rapidity of their course or their duration. This fixed order, according to him, is:—1. Progressive diminution and loss of warmth. 2. Cadaveric rigidity. 3. Cessation of rigidity and relaxation of the muscles. 4. Putrefaction. The circumstances under which the period and degree of these vary are chiefly connected with the kind of death and the atmospheric temperature; and, in certain given cases, it becomes possible to solve the following problem:—One of these phenomena being present in a case of hanging or strangulation, to determine the period at which death took place. At the time of Madame Duroulle's death, the weather was cold; and yet, on the evening of the 27th, her body was still warm, and there was but little cadaveric rigidity. Hence, M. Devergie concluded that she could not have been dead for twenty-five or thirty hours.—*Journal de Médecine et de Chirurgie Pratiques*, August, 1855.

On the Hydrostatic Lung-Test.—At a meeting of the Société de Biologie of Paris, in August, 1855, M. Blot communicated some facts in support of an assertion which he has several times made—that the hydrostatic test is insufficient, in some cases, to distinguish the lungs of children born alive from those of still-born infants.

A rhachitic female gave birth, at the seventh month, to a child which was apparently dead, but which was resuscitated by artificial respiration and the other means usually employed. After twenty minutes, the respiration, from being irregular, became regular, and was performed in the ordinary rhythm for an hour. The inspiration, though incomplete, was sufficient to maintain life; the expiration was noisy and plaintive. At the end of an hour, the breathing ceased, but the action of the heart continued for some time longer.

On opening the chest, the lungs were found collapsed, lying along the vertebral column; their anterior edges were turned outwards. The pericardium and thymus lay uncovered in the median line. The lungs scarcely filled two-thirds of the chest; they were of a brown colour, like an adult liver, and did not crepitate when pressed between the fingers.

When placed in water, the lungs, whether entire or in portions, sank rapidly to the bottom of the vessel. On pressing between the fingers a portion which had sunk in water, extremely fine bubbles of air escaped, without giving the least sensation of crepitation.

On the surface of the lungs were seen (especially anteriorly) some pulmonary cells lying under the pleura. These cells apparently did not crepitate, and sank in water; their colour, although less deep than that of the rest of the lungs, was brown.—*Gazette Médicale de Paris*, November 3rd, 1855.

HALF-YEARLY REPORT ON MICROLOGY.

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Part II. — Pathological Micrology.

BLOOD AND BLOODVESSELS.

Enumeration of Blood-corpuscles.—Pury, of Neufchatel, relates a case of leukhæmia,[*] in which the blood was examined at four separate times within the space of ten weeks.

In the first examination the colourless corpuscles were as 142·8 to 1000.
In the second ditto, „ „ 83·3 to 1000.
In the third ditto, „ „ 45·7 to 1000.
In the fourth ditto, „ „ 52·6 to 1000.

The blood of the splenic, jugular, and portal veins, was found to be coagulated, and of a greyish-yellow colour, and contained a large number of colourless corpuscles. In the splenic they were as 52·6 to 1000, in the jugular as 25·0 to 1000, confirming the observations of Funke, Kölliker, Moleschott, Vierordt, and Welcker, who found the splenic blood to be richer than other veins in the colourless corpuscles. It will be remembered that Moleschott considers the proportion of colourless to red corpuscles in healthy ordinary blood to be as 2·8 to 1000.

Cases of leukhæmia are also detailed by Heschl of Krakau,[†] and by Dr. Wallace of Greenock,[‡] which, however, we can only thus advert to.

On the Pathological Condition of the Smaller Cerebral Vessels.—Under this title we have an Inaugural Dissertation at the University of Würzburg, by H. Mosherr, 1854. He first describes the physiological condition of the cerebral capillaries, as described by various authors, noticing the frequent separation of the outer coat from the others, which may be owing to the interposition of pathological products, or only the result of the addition of water. He then considers the fatty degeneration of the vessels, recurring to the descriptions of Paget, Virchow, Brummerstadt, Hughes Bennett, Rokitansky, Wedl, &c. He takes exception to the opinion of Bennett, who considered the fatty particles connected with vessels about apoplectic clots to be indicative of inflammation. The author had examined twenty-eight cases of fatty degeneration, in various degrees; he describes the general widenings, or only partial swellings out, in both of which cases the opposition to the stream of blood is diminished. Oftentimes the partial swellings are occasioned by accumulations of fat being formed between the structureless and the inner membrane, apart from the formation of yellow golden pigment, owing to the rupture of the inner membrane, whence they were designated by Pestalozzi, in 1849, "spurious aneurysms." The peculiar necklace-like appearance assumed by these arteries often, is either owing to regularly succeeding widening of the whole calibre, or by the pressure of fat heaps inside the adventitious tunic, which is often so thin that slight pressure suffices to burst them, and squeeze out the fat. Where the degeneration of the small vessels begins is a very difficult question. It can hardly be said whether the granules belong to one coat or the other, and this is owing to the transparency of the elements, and the impossibility of distinguishing the limits of the single cells. In larger vessels, and in advanced life, one recognises the middle tunic as the degenerated part. In some cases observed by Virchow and the author the outer coat contained much irregularly divided fat, the muscular coat had fat granules, in part more disseminated, in part arranged in pearl-like rows, so that the elongated nucleus of the fibre-cell appeared free, the rows of glittering molecules being limited by the cell walls. In allusion to the view of Virchow, that there might be fattily degenerated areolar tissue bodies, the author said he had never observed any central grain in the conglomerates, and again the areolar tissue is very sparing about the smaller vessels. At one time the arterial, at another time the venous system was affected, but the former chiefly; in some cases both were equally affected. The arteries and veins of the grey matter were more free from the disease than those of the white part.

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[*] Virchow's Archiv, p. 289. July, 1855.

[†] Ibid., p. 353.

[‡] Edinburgh Monthly Journal.

After giving a table, showing the enlargement of arteries and capillaries in eclampsia and encephalitis, the author ends by relating a case of varicose ektases, in which the brain on section presented bloody points like extravasation, which were found to be vesicles filled with blood corpuscles; their walls were thin and nucleated, and were continuous with capillaries.

Form of Metamorphosis of Nerve and Muscle into Areolar Tissue.—A paper with this title appears, by Dr. Billroth.* After alluding to the observations of Blunette and Schröder van der Kolk, on the changes which muscle and nerve undergo in the neighbourhood of carcinoma, he goes on to speak of this change occurring, not as a specific carcinomatous degeneration, but essentially as a transformation of the muscles and nerves into areolar tissue. In hard carcinoma of the breast it unites intimately with the fascia of the pectoral muscles, and this again with the muscular substance itself, so that the muscle is drawn into the mass, and from the first point of growing together is arranged in a radial direction. One still distinguishes the fascia a long time after this growing together has taken place; but at a later period the tissues form such a firm cicatrix that one can, neither by the naked eye nor microscope, distinguish any of the original elements. The muscle passes right into the tumour, loses its dark red colour, and at last assumes a white glittering colour, but often the bundle-like arrangement is preserved. The like occurs in cancer of the lip. In investigating microscopically these spots of transition, a very careful tearing is required. First of all, a number of small cells and nuclei come into sight, and the muscular fibre is found to be very brittle, easily tearing transversely where the fibres immediately pass into the carcinomatous cicatrix, but one seldom can follow a free fibre very far. The muscular fibre first becomes less cross-striped in places assuming a more homogeneous and stringy appearance, and at the same time a new formation of tolerably dark oval nuclei arises in or under the sarcolemma of the fibres, which take on a completely homogeneous glittering look. Whilst this change is progressing, new cells are formed between the fibres, and the tissue becomes so coherent that single fibres can only seldom be recognised, and the substance thus formed is no longer cleavable like muscle, but friable. The newly-formed nuclei compress the muscular substance, and afterwards appear to dwindle as the substance arising from the metamorphosis becomes much less nucleated than it was during development. The fore-mentioned process is the one most frequently met with, but yet there are many variations; for instance, the fibres may maintain their breadth, losing their cross-stripes, they may assume a fine punctate bright appearance, with only a scanty formation of nuclei. In other cases the covering is filled with such a mass of nuclei that it appears as if the muscular substance passed into the new formation, and perhaps itself served as a material for new formation. But these forms are seldom proportional, and may possibly be a deception, as this material does not correspond to single fibres, but only depends upon the coherence of the nucleated and cellular material deposited between the muscular fibres, which on mechanical grounds also assumes a cylindrical form. Along with these nuclei one sees a good number of fine spindle-cells, unaffected by acetic acid, which must be regarded as proceeding out of the cells deposited between the muscular bundles. This metamorphosis of muscular fibre is not peculiar to the neighbourhood of cancer. The author relates a case of a boy, part of whose lip was excised in the Berlin Hospital, for the removal of a tumour, and the labial muscles adhering, the rete were found to be metamorphosed into strong nucleus-holding areolar tissue and elastic fibres. In other cases, such as the diffuse cavernous tumours, the transformation of muscle into areolar tissue may be seen.

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* Virchow's Archiv, p. 260. July, 1855.

Just as it is with muscular fibre, so do nerve fibres pass into a kind of matrix, whilst elongated nuclei form in their sheaths. A firm cancer was removed from the mamma. It had grown into the pectoral muscle, which was removed with it. At the place of transition of the sound muscle into the tumour, in a portion kept in acetic acid for twenty-four hours, an abundance of nerves more clear and numerous than usual was seen. In a thick nervous trunk, ray-like extensions of the primitive fibres in a lateral direction were seen which, partly single, and partly united with small secondary branches, proceeded into the muscle. Here and there the bright-dark contours of the primitive fibres were seen, but for the most part the nerve substance had passed into a kind of matrix, and only a row of nuclei placed alongside each other indicated the original course of the fibre. One clearly sees in single places that the nuclei were imbedded in the sheaths of the nerve fibres, which were also in great part destroyed by reagents. This degeneration was advanced also in the neurilemma of the larger nerve branches. Our author considers that the pain often felt in cancer of the mammæ arises from the above-described new formation of nuclei in the sheaths of the primitive nerve fibres, by which the nerves are manifestly exposed to great pressure; and this the more likely, as the cancerous growths are almost free from nerves themselves. Probably something of this kind occurs in the fibroids of the skin and periosteum. The substance resulting from the above degeneration of the muscles and nerves becomes brittle, and swells up on maceration in weak acetic acid, as also in weak alkalies, being therefore not completely analogous to ordinary areolar tissue.

On the Dracunculus of Bombay.—Mr. H. J. Carter had his attention drawn to the subject* by seeing a boy, aged four, suffering from the guinea worm in the hospital of the Central School—a very rare occurrence in this school. He had lived in a house close to the School of Industry, of which his father was superintendent sergeant. His mother had a guinea worm in her ankle; other people in the neighbourhood were affected by them, and twenty-one out of fifty of the boys in the School of Industry had been affected in the past year. In all who had been affected the worms had come out from the knee downwards. The boys were living in an enclosure taken in from the shore, and were supplied by two wells, one for bathing, the other for drinking. Mr. Carter, who, in examining the various confervæ from different tanks in Bombay, found minute worms resembling the young of the guinea worm, determined to examine some of the silty confervæ from the pit where the boys bathed, and found it to contain numbers of this worm, besides other animalcules. These worms almost exactly resembled the young ones removed from the leg of one of the schoolboys, and may be described as follows, one description answering to both for the most part:—They were slightly diminishing towards the head, which is sub-pointed, not presenting the papillæ seen in the fully developed animal, even when seen under the microscope. Posteriorly they diminished in breadth a little in front of the junction of the middle with the posterior third, down to an almost imperceptible end. They were colourless, and filled with a granular material, disappearing towards the head and tail, which were thus transparent. In size the young guinea worms were $\frac{1}{33}$rd of an inch in length, and $\frac{1}{833}$rd in breadth, and the tank worm was equal to and less than the guinea worm. In vol. 1 of the Journal, Dr. Forbes had stated that he had found an animalcule in the mud of the tank about Dharwar exactly resembling the young guinea worm; and to further test the probability of the tank and guinea worm being alike, Mr. Carter examined with the microscope the confervæ in the tank of the Central School, which was like that in the well of the School of Industry, but he found no portion of a single worm of the characters mentioned; whilst dracunculus is so uncommon in the Central Schools, that there have only been three cases in the last eight years, and these doubtless contracted during visits to friends, the guinea worm requiring a whole year for its development. The tank of the Central School is supplied by the rain falling during the monsoon. The author deduces the following inferences:—

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* As related in the Transactions of the Medical and Physical Society of Bombay, p. 45. 1853–4.

1st. That the dracunculus may follow bathing in water where the tank worm is found.

2ndly. That it is bathing, and not drinking the water, which gives the dracunculus—the worm finding its way by the pores of the skin.

3rdly. That it may easily be exterminated.

Dr. Forbes has proved by an experiment that gastric juice is fatal to the young guinea worm; he gave the young of a guinea worm in water to two young pups, and found on examination a short time after that all were quite dead. They also die as soon as the water in which they are, putrefies, or becomes dry. Though the tank worm must during the hot weather be confined to the tanks, after the commencement of the rains, mosses and algæ on old walls and trees are found to contain the same kind of worm. Hence it probably exists generally during the rain, though more in some localities than others. The author adds a description of the adult female guinea worm.

Progressive Atrophy of Muscular Fibre.—Virchow* relates at length a case of a man, aged forty-four, who was affected by progressive muscular atrophy. He had been affected, when aged twenty-one, with almost complete lameness of the extremities, supposed to be of rheumatic origin. His father had been similarly affected when aged forty. In this case the lameness began in the legs and spread upwards. The intestines and urinary bladder remained natural until his death. The muscles of the extremities were very emaciated, and of a pale reddish-yellow colour, some being entirely degenerated. Under the microscope they exhibited areolar tissue and fat-cells containing granular material, partly corresponding to the old muscular bundles in an uninterrupted way, partly not so. In some muscles the microscope also showed the presence of slender vesicles of 0,009—0,01 millimetres broad, containing very small fat-corpuscles. Here and there were elongated nuclei, and in some places small round nuclei, showing a double contour on addition of acetic acid. These were partly single and partly heaped together from 2 to 7 in number, partly in files. These vesicles at times appeared quite isolated, with round extremities, and many had a more candate character. In other places, where the muscle was redder in colour, the vesicles were broader, containing more numerous granules, mostly oval, and of 0,075 millimètres in length. The signification of these structures was difficult to decipher. Often there were evident fat cells, surrounded by a membrane entirely uplifted from the fat drops, and with an oval nucleus. Areolar tissue bundles existed, with spindle-shaped very delicate corpuscles, which were mostly connected at their extremities; also spindle cells, broader, and filled with fine fat granules, which gradually became larger, and finally passed into large oval cells, containing large fat drops as well as the fine granules.

Finally, there were decided fat cells, only differing from ordinary ones in that along with a large fat drop they contained many smaller ones. Hence it appeared to Virchow that a new formation of fat-cells had taken place out of areolar tissue corpuscles. Where the muscle was still more unaltered, the primitive bundles were delicately pale, with finely granular contents and incomplete striæ. The arteries of the diseased muscles had fine granular fat in their walls. The nerves contained less fibres than usual, and on longitudinal as well as transverse section very broad intervening spaces were seen occupied by a very richly nucleated tissue; the nuclei were long, delicate, something pointed, almost like nuclei of organic muscle fibre, and in every direction much finely-granulated fat existed. The various nerves did not appear atrophied to the naked eye.

The spinal marrow, as well as the roots of the nerves, were healthy in look, but on section, even to the naked eye, a remarkable variation was seen, beginning at the upper cervical region and proceeding downwards, becoming gradually more marked, and most remarkable about the lumbar swelling. In all these places one saw in the posterior fibres of the chord, and more decidedly, near the posterior longitudinal fissure, a clearish grey, somewhat translucent mass, instead of the white nerve substance, which so extended into the under part of the medulla as to reach the posterior horn of grey substance. Here it so united with the grey matter that an obvious limit could not be seen. In general the degeneration began at the posterior longitudinal fissure, and proceeded thence into the substance of the posterior fibres. As seen by the microscope, only the posterior fibres, and not the horns, were affected. The change was of the same nature as that in the peripheric nerves, only that some broader nerve-fibres existed grouped together, which on transverse section were separated from each other by a distance of 0,005 to 0,012 millimètres. Between them existed a very soft friable granular material, containing thickly-strewed corpora amylacea, and also many granulated nuclei, chiefly oval, and here and there enclosed in round elongated cell membrane. No fat was visible, and the bloodvessels had a natural look. On the addition of chromic acid, instead of finely granular substance, much shreddy, firm and fine fibrillated material was seen.

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* Virchow's Archiv, p. 537. Oct. 1855.

On Mucous Polypi of the Antrum of the Jaw. By H. **Luschka**, of Tubingen.[*]—After entering minutely into the anatomy of the mucous membrane lining the cavity, in which he takes exception to the views of Todd and Bowman, who consider the periosteum lining its cavity to be in fact a sub-mucous areolar tissue, he speaks of the presence of the mucous glands, which are said by authors to be absent. In many glands the main tube, as well as the adjoining ones, possess bulgings out in the adult, and it is the presence of these bulgings which Luschka considers to be the most frequent cause of the cysts often found in the antrum. The cyst formation which is so frequently found sessile or pedunculated must be distinguished from the soft productions known as hypertrophies of the mucous membrane. The sunk cysts contain a sago-like material, partly soluble in potash, and consisting of a fine molecular mass, free fat, aggregated fat globules, and a few round cells. The more external cysts contain, along with a clear fluid, yellow cheese-like masses, in which, along with other components, fat crystals and corpora amylacea are often to be seen. In the smaller cysts one may, by the use of alkalies and acetic acid, peel off a pellicle, which in most cases may be considered the altered gland membrane. In the larger cysts there exists a wall composed of connective tissue elements in various stages of development, which contains on its inner surface an epithelium of flat and round cells, incomplete and fattily degenerated. So-called soft polypi, less frequent than the cysts, are found growing from the mucous membrane, which are to be considered as growths or hypertrophies of the sub-mucous areolar tissue, having either a spongy areolar tissue inside, or a framework enclosing irregular spaces filled with a gelatine-like substance. Luschka had found these at least five times in sixty bodies, and they generally had a clavate or pear-shaped form, sometimes a flat lobular form attached by their broad end, and generally from ½ to 2 centimètres long. These polypi were so situated generally as to barricade completely the opening of communication with the middle nasal fossæ, and so cause an accumulation of the mucus. Generally speaking, only a single polypus is found, but on one occasion six were found, which, along with a number of cysts and tough mucus, filled the antrum.

On the Animal Starch and Cellulose Question.—Virchow[*] has given the results of further investigations on this subject. He divides the substance into true and false corpora amylacea, relying on the reaction of iodine and sulphuric acid, and on the fact that the true corpora amylacea are not soluble in hot alcohol, æther, &c., and are destroyed by concentrated acids and alkalies. Among the false bodies he classes:—1st. The brain-sand, probably the same which Busk described as being found in the corpus callosum, and which was coloured externally of a yellowish-red hue by iodine. 2nd. Various gelatinous and albuminous grains spoken of as colloid-grains in certain tumours. 3rd. The concentric epidermal globules often found in the thymus gland and cancroid tumours. 4th. The bodies found in coagulated blood described by Gulliver, Gerber, and Hassall. 5th. The medullary matter described by Virchow himself on a previous occasion. 6th. The leucine grains obtained from extract of milk.

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[*] Virchow's Archiv, p. 409. Oct. 1855.

[†] Ibid., vol. iii. Heft 1.

According to Virchow the following are the places wherein true amyloid degeneration is certainly to be found. They are—1st. The nervous system. Besides the fore-mentioned parts, the spinal ligament of the cochlea, and many parts of atrophied brain and spinal marrow, show it. He had found it in the gelatinous and cellular softening of these structures, and he mentions its discovery by Busk in one case almost throughout the brain and the choroid plexus; by Willigk in cicatrices of brain; and by Rokitansky in atrophied parts of brain and other structures. 2nd. In the spleen. In the follicular cells and pulp, the thickened walls of arteries, especially circular fibres, and in the trabeculæ. 3rd. In the liver. In the waxy degeneration, chiefly in the cells, but also in intervening tissue. 4th. In the kidneys, which are pre-eminently the subject of the degeneration. The Malpighian bodies and the arteries leading to them become first affected; then the areolar tissue in the neighbourhood of urinary tubes of papillæ; and then the other parts. Virchow says, that in most organs where they are found we have undoubted changes of the tissue elements, and that probably there is a "conversion into vegetable matter."

These starch bodies, chemically as well as morphologically, are very allied to starch bodies of plants. Busk says he has often seen in the smaller ones a dark cross by polarized light, whose arms intersect each other in the middle of the grains at an angle of 45°, the majority only showing a simple dark line. It seems necessary to guard against error by the remembrance that in several false amylaceous bodies a yellowish-red colour, called by Meckel iodine-red, is found by addition of iodine; and this is the case also with all blood-holding parts. The later addition of sulphuric acid will be requisite to determine the presence of true amyloid substance. This yellow or iodine-red appearance is compared by Busk to the appearance produced in unripe cellulose, such as is wont to occur in the lower plants. But in plants we have quantities of cellulose mixed with gelatinous substances, so that in the treatment with iodine and sulphuric acid we have all sorts of immature colours, indicating a mixture of blue and red, brown and yellow. Such a play of colours takes place in the spleen, specially in the amyloid substance from the pulp and follicles, but in no case does the blue or blue-red come forward with such clearness as in the Malpighian bodies and afferent arteries of the parenchyma of the kidney. Our author concludes that sooner or later the albuminous substance of the tissues disappears, and is replaced by amyloid substance. In those instances where the substance differed still more from starch proper it becomes more like cellulose proper; and the organs affected show that peculiar look called waxy or lardaceous. This same idea is acknowledged by Virchow to have arisen also at Edinburgh independently of himself. Generally the indurated organs are enlarged, leaving no doubt of the deposit of new matter. The co-existence of the same alteration in the spleen, liver, and kidneys leads naturally to the recognition of a common cause, a constitutional disturbance.

Since the above was written by Virchow in the 'Archiv,' he has made another communication on the same subject; but before speaking of this, we will mention a communication made* by Mr. Carter, entitled the "Extensive Diffusion and Frequency of Starch Corpuscles in the Tissues of the Human Body." In this, it will be seen, a different view is maintained on certain points. This observer saw the starch bodies in a tumour involving the optic nerve, and also the pinal gland of man and sheep; and, since then, made extensive experiments, examining in succession thirteen human bodies out of the clinical wards of Professor Bennett, of Edinburgh. He met with two kinds of starch, one resembling wheat, the other potato starch; and he found them in the liver, spleen, kidneys, brain, pancreas, mesenteric glands, suprarenal capsules, Pacchionian bodies, mesentery, lungs, ovaries, scrofulous matter, pus, urine, epidermis, blood, and other places, in organs as well healthy as diseased. In one case he found them around an apoplectic clot, but could not find them in any other part of the brain. In a case of diabetes, the other organs presented an unusual amount, but the liver was *free* from any. He never seems to have found them in the muscular structure of the heart. In the sheep, oxen, and lower animals, they were found in the same indiscriminate way; and the author says that they have hitherto been mistaken for fatty oil globules, to which, from form and refractile powers, they have much resemblance. He considers them as of physiological, not of pathological interest, being ordinary constituents of the body, and, as he calls them, "the thermogenic magazines," analogous to fatty substance, and capable, possibly, of conversion into grape sugar and carbonic acid, or into the lactic acid of the gastric juice.

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* Edinburgh Medical Journal. August, 1855.

In the second paper by Virchow,* to which we have alluded, the author thinks he has made considerable advances on the subject. In all the cases in which he found the cellulose, chronic and extensive disease of the osseous system existed; and he thinks these diseases exercise a determinate influence on the production of the waxy "degeneration"—the disease, especially caries and necrosis, inducing a deficit of nutrition and cachexy, thus robbing the spleen, kidneys, &c., of their natural elements, and disposing them to take on the degeneration. He has never met with the amyloid substance in the bones, but has so done in the cartilage of the joints of an old person with senile arthritis.

TUMOURS, MORBID DEPOSITS, EXCRESCENCES, &c.

Fibroid in the Heart's Substance. By LUSCHKA.†—After alluding to the so-called chondroid of Albers,‡ and the description by Rokitansky, of irregular fibrous masses in the substance of the heart, the author gives a case of a boy, aged six, who died of croup and pleurisy, and in the walls of whose heart a fibroid mass was found. It was of the size of a hen's egg, of oval shape, and situated at the outer surface of the walls of the left ventricle. No change in the endocardium existed. The tumour was of the consistence of ordinary uterine fibroids, giving, when cut into, a gristly noise, of white colour and tendinous look. A manifest but irregular meshwork existed, supported by bundles of fibres; the meshes being elongated, angular, or rhomboidal. The substance within the meshes was of a dull-white appearance, and slight consistence; and the whole was more or less surrounded by a membrane, proving, to the author's mind, that it was a true fibrous tumour, and not the massing together and conversion of the remains of inflammation; and the investing membrane was specially visible exactly at the spot where the tumour on the outside was in close contact with the visceral pericardium. The structure of the connective-tissue membrane separating the deposit from the sub-serous cellular material, was of thickly felted threads and bundles, with very much elastic fibre, both in a complete and incomplete condition; and small bloodvessels were traceable through into the tumour. The microscopical examination of the interior showed homogeneous as well as fibrous streaks and bundles of connective tissue, and also numerous connective-tissue cells elongating into fibres, and in various stages of development. A few so-called elastic nucleus fibres were seen. The softish material enclosed within the meshes behaved like structureless intercellular substance, containing simple and occasionally net-like united cords, as seen in uterine fibroids; and on the addition of acetic acid, gave the appearance as of fine elastic, partly auricular, partly spirally-arranged fibres.

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* Archiv, p. 361. July, 1855. † Ibid., p. 343. ‡ Atlas, iii. Tab. 10.

Progressive Papillary Excrescences of the Skin.—Dr. Stubenrauch, the author of an Inaugural Discourse at Giessen on this somewhat rare form of tumours, formerly known as Papillary Nævus, illustrates the subject by two preparations in the Giessen Museum. Before detailing the cases, he reviews the various forms of hypertrophied papillæ, amongst which warts, condylomata, and epithelioma, are found. Other hypertrophies of the papillæ, as ichthyosis, whose method of appearance is different, are to be classed in a different category. Though agreeing in elementary composition, these growths show, some an innocent, others a malignant character. For instance, epithelioma differs from warts and condylomata by its tendency to affect deeper parts than the mere skin, by its infection of lymphatic glands, its ready destruction, &c. The papillary nævus agrees with all these growths in its seat of origin; according with the warts and condylomata as to its innocency, but, owing to its segregation, &c., resembling to a great extent epithelioma, which, arising generally from warty processes, differs from warts, nevertheless, in their greater softness, sensibility, and greater amount of blood contents. The prominences become ragged, the epidermis splits, the chinks become covered with numerous epithelial cells, which are united by adhesive matters, and owing to the growth of new papillæ, cauliflower-like growths arise. The regenerative spaces in the papillæ and subcutaneous cell-tissue are characteristically present in epithelioma. To the above epithelioma the papillary excrescence has by far the greatest resemblance, but yet its whole course causes it to differ from it.

The author then describes the two cases which serve as part of his dissertation. The first was the upper extremity of a girl, aged eighteen, amputated half way down the arm. The patient had never menstruated, and always been sickly, and her arm began to be diseased when she was a year and a half old and suffering from strumous ophthalmia. The first thing observed was an outgrowth of papilpilary excrescence, at the upper part of the arm. In the course of the next twelve years the remaining part of the upper and fore-arm became involved, the excrescence advancing in a serpentine direction, the older parts affected getting well, whilst the new ones became worse. The hands and joints of the fingers became affected and greatly enlarged, and the parts of skin not involved being thickened and contracted, and the limb was then amputated. The excrescences had a varied appearance, being chiefly grouped in a cauliflower way: soft, red, covered by a thick epithelium, and resting on larger and smaller bases. In places they were polished, and often pressed together. The larger growths having a smaller base were enlarged at their upper surface, club-like, the smaller ones approximating to cones; and during life they were of a deep-red colour. They varied in size up to three-quarters of an inch, the transition from the greater to the smaller papillæ being very gradual. The epidermis, which was thickened, was easily removed, and penetrated not only between the single excrescences, but also between the secondary papillæ, which were formed by the splitting up of the single excrescences; and during life the recesses between the papillæ were of a yellow colour, filled with pus-like fetid material, containing abraded epithelium. On minute examination, the fibres of the corium, from which the excrescences arose, were seen to pass into them, but were not traceable into the secondary papillæ, which only showed a granular structure. The bloodvessels between the fibres at the base of the large excrescences divided in a fork-like way, and sent their twigs to each secondary papillæ, within which they formed loops; so that the whole excrescence, owing to the plentiful vascular supply to the secondary papillæ, and by the increased size of the capillaries, assumed the aspect of teleangiectasis. Very often two vessels in a secondary papillæ existed, forming numerous non-anastomosing convolutions. Nothing like abnormal tissue was found either in the papillæ or the neighbourhood; and it was remarkable that both the skin on which grew the hypertrophies, and that of the fore-arm from which they had retired, presented neither sudorifics, nor sebaceous or hair follicles. The areolar tissue was free from fat, and the corium thickened in many places to the fourth or fifth lines, and both were infiltrated with serous fluid. The muscles and nerves, &c., in the neighbourhood, and lymphatic glands, were natural, as were the arteries; but the superficial veins were increased in size. The bones of the carpus and the phalanges were thinned and crumbling.

In the second case detailed, in which the leg of a man, aged fifty-one, had been amputated below the knee, the course was the same, but the papillæ were more hypertrophied and more closely grouped together.

The author considers the above to differ from condylomata in not being of syphilitic origin, and from epithelioma in not affecting lymphatic glands, and in sparing bones and fasciæ, and in not having globular epithelioma.

Dr. Wernher, of Giessen, also has a paper on "Progressive, not Cancer-like, Papillary Tumours of the Cutis."[*]

Atheroma, an Encysted Epithelioma.—Dr. Wernher, of Giessen, has a paper with this title.[†] He alludes to the views of Kölliker and others, that atheroma was the result of closed, largely-developed, sebaceous follicles; and of others, like Paget, who rank them amongst the dermatoid tumours, and consider that some may be only closed hair follicles, as Astley Cooper had described them to be; and of Bruno, who places them between sero-cystic and dermo-cystic tumours. The latter considers them, like Paget, to arise in two various ways—partly out of sebaceous glands of the skin whose mouths have been closed, and whose secretion has so accumulated; and partly as quite new formations; but yet specially attributes their origin to the cuticular sebaceous follicles. Wernher says there are tumours situated in the skin — the cysted tumours of Cooper—having their sebaceous contents pressed out through an outlet, but other tumours are wanting in any open or stopped-up outlet. These latter are considered originally to have had such an outlet, which has become closed, or to have arisen as an imitation of the sebaceous glands in the neighbourhood of the cutis, with which they never were connected by means of an outlet. No one knows how the closure takes place, as the tumours exist in parts which have neither been wounded, ulcerated, nor inflamed Bärensprung alone met with them under cicatrices, and imagined that the duct of the glands had been wounded; but the author seemed to think that the testimony in proof of the obliteration is too scanty. He quotes an instance from Lebert and Bruno, in which an obliterated duct had been dissected out. Smaller atheromata, for the most part, are not hollow or fluctuating, and it is only as they grow that, by the solution of their contents, they become hollow spaces, or cysted tumours. Many are found where no hair follicles or sebaceous glands exist; and Schuh found them between and under the muscular layer of the temple, under the mucous membrane of the tongue, in bone, in the pia mater, &c. Wernher then relates ten cases, which we must pass over.

After the relation of the cases, the author proceeds to say that he considers atheromata to be simply pathological imitations of closed sebaceous glands; and this he concludes, not merely from the fact that they may exist in places where no hair or sebaceous glands are found, nor from the fact of any difference in their contents, but rather from the fact that they have quite a different structure and method of development from those glands. The atheromata seem never to consist of a simple sac, with a fibrous tunic covered by epithelium on the inner surface (the so-called cystic tumours of Cooper are excluded); they all showed either a much more complicated structure, or the remains of an earlier, more complicated arrangement in numerous transitions. At first they are solid and hard. The fibrous tunic forms nucleated epithelial cells, constantly produced in new layers, and gradually pushed towards the centre. At the same time they lose their nuclei, become flattened, shrivelled, and often filled with calcareous granules. Often the centre is so calcified as to resemble a white friable nucleus. The calcified central layers finally soften down into at first a crumbly, and then a fluid pultaceous mass. In the compound atheromata the epithelial spheres entering into the formation of horny layers also incline towards the fluid centres, and either more and more diffuse themselves so as to be indiscernible, or remain in such a state that they float in the pultaceous parts. No trace whatever of any excretory duct is to be found, although the smaller atheromata are always to be found immediately beneath the cutis. Even where a fibrous chord of connexion does exist between them and the cutis, it is not necessarily an obliterated duct. In some cases, it may be from expansion of the tumour, the skin and the surface of the sac inflame, unite, and an opening is formed simulating an excretory duct. A number of granules, or large, sharply defined, yellow small masses, appear in a structureless ground, become surrounded with laminæ, grow into epithelial globules, just as happens in epithelioma; the basement membrane in which the commencing globules are imbedded being covered by delicate pavement epithelium when not covered by a thicker horny layer. The single epithelial globules grow into larger epithelial spheres, and become surrounded by a fine tunic, bedecked with a pavement epithelium. The spheres may become calcified, just like the horny layers of the main sac. The epithelial globules are also to be found along with the large epithelial spheres, lying thickly together in every stage of development. Sometimes they unite to form thin plate-like masses, the older ones being towards the inner side. At first, the globules and spheres are separated from the fluid contents by a fibrous layer, which they burst, and oftentimes those whose covering has been destroyed are seen mixed with those as yet encysted. In most cases the horny plate is commenced by the coalition of numberless epithelial globules and spheres, which may be seen by the microscope on the outer surface of the layer, as small granules jutting out, often of very large size and number.

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[*] Henle and Pfeuffer's Zeitschrift, Band vi. Heft ii. p. 109. 1855.

[†] Virchow's Archiv, p. 221.

Sometimes the single concentric globules are separated by long spaces, in which the concentric arrangement of the cells into single globules cannot be seen. It would appear as if the formation of the epithelial spheres in the wall of the main cyst could proceed almost perpetually. Where the epithelial spheres lie in great numbers, the remaining parts of the capsule form very thick irregular fibrous layers, penetrated by calcareous crystals. The development of the primary sac, like that of the secondary spheres and cavities, results from an epithelial globule, which forms its own fibrous investment; but our author does not even venture to guess at the origin of the primary globule.

As regards the similarity between the compound atheromata and epitheliomata, the author says in both cases we have growths of epithelial formation, as well in the structure of the skin as in non-cuticular places—in both cases there is an uncertain growth and successive after-development, if once the germ be sown—in both cases the disposition of the formative material to form aggregations and laminated spheres exist, out of which arise the characteristic "globules epitheliales." In both cases, also, the disintegration of the epithelial formations forms a white pultaceous mass, consisting chiefly of thickened shrivelled epithelial cells, cholesterine plates, and amorphous clumps of lime and albumen, which in the one case fills the apertures and interspaces of the cauliflower-like growth, and in the other the cavity of the holes of the sac, serving as a common investment. The most important anatomical difference consists in the fact that the so-called atheromata are enclosed in a firm sac, in whose cavity it develops itself, which is wanting in epithelioma, whose growth in the surrounding areolar tissue is not hindered by any such limits; that furthermore the epithelioma, inasmuch as it is developed on the surface of the skin, is wont to be united with the growth of the dermoid tissues above, specially the papillæ. These anatomical differences are, nevertheless, so unessential that they serve as characters of varieties, but do not exclude the near relationship of forms.

One allows that cysted and non-cysted scirrhus, if only their characters otherwise agree, are identical diseases. Wernher hesitated not, therefore, to describe the so-called atheroma as encysted epithelioma, and to rank it among the epithelial cancers.

On Pearly Tumours (*Cholesteatomata of Müller*) *and Cancroid.* By R. VIRCHOW.* —Virchow considers cholesteatomata to be, like hair and fat cysts, new formations, and very strictly to be separated from atheromata or tegumentary tumours, which arise from pre-existing tegumentary formations. They are newly-formed cystoids. He speaks to cancroids as being nearly related to cholesteatomata, which are tumours of an alveolar structure, in which the walls of the alveoli are formed of cells, the pseudo-cancer of Lebert, and so differing from the true cancer, wherein they are of areolar tissue; the alveoli in both cases containing cells or nuclei with fluid intercellular material. He also alludes to fibroids which have a kind of alveolar structure with fibrous contents. In opposition to Bennett, who considers all tumours whatever, which have a cancerous look, but not a cancerous structure, such as enchondroma, to be cancroid, and to others who consider cancer of the lip to be only a papillary and epidermal growth of the surface, Virchow regards cancroid as consisting in the formation of cavities or alveoli, in the interior of diseased tissue and organs, which become filled with epidermis-like cells. These cavities are large and to be distinguished from the alveoli of true cancer microscopically by their arising within old structural elements, without that newly-formed layer of areolar tissue which constitutes the wall of the alveolar cancer. If one imagines large round spaces filled with nucleated and non-nucleated cells disposed concentrically, and their interior to pass into a granular pultaceous state, he would then have the pearl-like structure of cholesteatoma.

Virchow considers the history of pearly tumours to be made clear, specially the similarity of development between them and atheromata and cancroid (epithelioma), the young cholesteatomata pearls at a certain stage not differing from simple atheroma pearls and cancroid pearls, all of which are globular lamellar structures of flat epidermal cells (*globes epidermiques*), and the ordinary pearls of atheroma deviate only from those of cholesteatoma and cancroid in not containing inside such peculiar glistering, spherical, and oval structures in general. Laminated epidermal spheres can no longer, then, be considered a special or specific property of cancroid, the more as they all occur in places in which epidermis is formed in growing masses, equally whether it pre-exists, as in the outer skin, or whether it is found independently, as in thymus and mammary glands, testicle or fistulous ulcers, or cerebral membranes, &c. This is proved by those cases in which globular laminated accumulations arise from relatively such disturbances as is the case in skin warts, in which the deep depressions of the superficial integuments between the papillæ gradually produce in themselves epidermal globules. This generally occurs about the nails, but also in hair sacs, with their dilatation; especially in lupus does one find structures of this kind in the hair sacs. There seems to be three groups of epidermal structures. 1st, The ordinary epidermis, with its projections into hair sacs and glands of skin, as also the transitions of true canals and mucous membranes. 2nd, Those formed by the transformation of gland cells, as in testicle, thymus, mammary glands, &c. 3rd, Those formed out of areolar tissue, as in cancroid, cholesteatoma, &c.

Cholesteatoma belongs, consequently, to the class of complete heterologous formations, because it arises in places which normally neither contain epidermis nor epidermis-like elements. Virchow suggests the name pearly tumour as being preferable.

In a thesis for the doctorate at Paris, J. N. Dupuy treats at length on 'Cancroid or Epithelial Cancer.' The subject is discussed in a general point of view; and the author, after giving in full detail four cases, draws the following conclusions:

1st. Cancroid is an affection which may affect all the parts of the organism, primarily or secondarily.

2nd. It may produce and propagate itself not only in the tissues or ganglia directly in communication with the anatomical region of the original seat, but it may also generalize itself, invade the whole economy, and bring on cachexy, infection, just like true cancer.

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* Archiv für Path. Anat. und Phys., p. 371. Oct. 1855.

3rd. Being an affection which only presents hæmœomorphous products, and having in all points a progress analogous to that of cancer, there is no need of specific heterologous capsules to constitute this malady.

On Multilocular Ulcerating Echinococcus Tumours of the Liver. By RUDOLPH VIRCHOW.*—After alluding to two cases of alveolar colloid, described by Buhl, one in the 'Munich Illustrated Gazette,' 1852, and another in the 'Zeitschrift für Rat. Med.' 1854, Virchow mentions a third described by Ernst Zeller, in an inaugural treatise at Tubingen, 1854; but in none of these was the history of their development capable of being made out further than that Buhl imagined their earliest development to be from solid or cystic grains, which might be compared to nuclei or elementary corpuscles, and the later growth to result from intussusception or exogenous surrounding deposition. In the case related by Sommer, there existed in the interior of the tumour, as in the other instances, hollow spaces, but also a great number of the colloid cysts, young manifest echinococci; and Zeller thought he could thereby establish the difference between the alveolar colloid and the special colloid cancer of the liver, and that the formation of the hollow spaces was due to the suppuration of the echinococcus cysts. Virchow then reviews numerous cases on record by Meyer, Dittrich, and Forster, which are possibly of the same nature, but limits himself to the three undoubted cases before mentioned, of Buhl and Zeller, and adds a fourth one himself. He substantiates the descriptions of the others, but entirely differs from them in their interpretation of their observations. Forster had already mentioned the resemblance between the echinococcus membrane and certain colloid formations. The subject of Virchow's case was a man, aged thirty-eight, who was treated for abdominal pain, diarrhœa, and jaundice. The patient died with voiding of blood by stool and collapse. On examination after death, the liver was found to be enlarged, having a thick cartilage-like plate on its surface, which, on section, proved to be from eight to ten millimètres thick, and covering a cavity the size of a fist, situated in a growth equal to a child's head in size, out of which a dirty yellow pus-like fluid escaped, which on standing was converted into greenish-yellow turbid serum, and a yellowish white sediment containing much granular material, partly fatty and partly consisting of cell forms, with here and there granular corpuscles. The inner surface of the large cavity was entirely covered by irregular projections, whilst in the neighbourhood of the upper surface mere secondary cavities existed, whose walls, soft and covered by a whitish, delicate, and somewhat flocculent investment, indicated their later formation. These small cavities were separated from the larger ones by the large partly dissolved and partly firm mass of the tumour. These last, on their under and inner part, showed a deeply yellow compound border, otherwise in every direction clear, globular, or cyst-like bodies, mostly of the size of hemp seeds, existed in a dirty greenish white matrix, in every direction. The wall itself of the tumour, which in various places possessed a very variable thickness, was for the most part of like composition. Externally came first a hard tendinous layer of connective tissue, and then more internally a number of gelatinous cysts, laying in cavities of the size of millet seeds, and smaller. Towards the under side, where were the neighbouring cavities, the layer of connective tissue was hardly a millimètre in thickness, and the presence of small gelatinous cysts, only to be recognised on the inner surface of the sac by the presence of many shallow pits. The mass of the tumour was composed of a fine alveolar tissue, whose stroma was white, very firm, and dense; whilst most of the alveoli appeared as small puncta. Here and there larger and more substantial cavities existed, corresponding to dilated gall-ducts, but containing no cystic or gelatinous masses. In the small alveoli, on the contrary, isolated yellow gelatinous lumps, lying quite loose, existed. On closer examination, a section through the tumour showed a thick fibrous stroma, exhibiting the ordinary properties of areolar tissue, beset with numerous spindle-shaped and reticulated cells. In many places—viz., towards the large cavities, this tissue was undergoing fatty change; in other places, masses of yellow and brown pigment existed. More to the outside, masses of parenchyma of liver cells infiltrated with gall pigment penetrated between the fibrous material. In the middle of the connective tissue, in sharply defined, partly rounded, partly elongated and bulging out, or contracted cavities, lay the gelatinous masses, corresponding in the majority of cases to the descriptions of Buhl and Zeller. The smaller of the cavities had a diameter of 0·03 to 0·16 millimètres; the largest, 0·3 to 0·4 millimètres in general; but towards the centre of the liver they were as large as three millimètres in breadth. The gelatinous masses in the smaller cavities consisted of a laminated glittering structureless wall, and a cavity filled mostly with somewhat granular and chiefly fat-holding materials. On the inner side the wall was very much folded, and the contents often disposed in masses; but oftentimes lateral outbulgings in the wall existed. Where the largest cavities existed, their distance from each other was much diminished—in some cases there was no intervening tissue. Out of the larger cavities large connected gelatinous masses could easily be withdrawn, which when placed in water quickly extended into large pellicles, out of which escaped cysts of the size of millet or hemp seeds, always very wrinkled. Here and there appeared to have occurred not only a clustering of more cysts in the same hollow space, but also an encasing of cysts one within the other. The cysts on their outer surface were mostly completely smooth, and only slightly sprinkled with amorphous, and here and there granular and coloured particles. Inside, on the contrary, an opaque material existed, which in most cases appeared simply granular, but often showed almost cell-like divisions. The larger pellicles exhibited the same changes as one generally finds in echinococcus membrane. In the place of the fine stripes, granular fat-like glittering particles existed in necklace-like rows, or in single and compound groups. But on the inner surface existed quite a new and peculiar arrangement. Here lay a star-shaped structure, anastomosing and net-like, which was thickened at the knotted joints. In places, this net-work tissue was larger, and its projections and threads of union broader and tube-like; and its larger parts more obvious, owing to granular deposit. Thus there arose the greatest similarity to lymphatic vessels in a state of development. Inside the pellicles existed an elongated or rounded sac or capsule, formed of a delicate membrane, containing large glittering bodies. The capsule differed from ordinary echinococcus cysts in being of simpler formation, the thickest not showing more than two layers. Yellow and yellowish brown pigment and hæmatoid crystals were accumulated also within them. Another kind of formation also existed, of a yellowish colour and cloudy appearance, elevated from the surface of the tunic, and looking like small clavate appendages of the same; many of them were almost homogeneous, and furnished at the free end with parallel curved lines, just as if in this place a growth in layers had occurred. Most of them contained under this laminated clavate extremity a small egg-shaped hollow, so that one was reminded of numerous entozoa ovaries. Around the tunics, and between them, lay very many concentric bodies, generally aggregated into larger groups, and consisting of calcareous salts and an organic base. They were discriminated from the well-known calcareous granules of echinococci by their size. In the fluid of the caverns also existed numerous needle-shaped truly fatty crystals, with a sheaf-like arrangement. The author discovered young echinococcus animals after some search, but only in the portal part of the tumour, where the great alveoli were, and which was probably most recently formed. They were invisible to the naked eye, and some possessed hooklets, whilst others were destitute of them. After this, he says we cannot doubt that the whole was composed of small echinococcus cysts, and should not be named alveolar colloid. Schröder van der Kolk, from his injections, imagines that the echinococci of the liver are situated in the gall-ducts. In none of the above instances was this the case; the ducts and vessels were all free, and it seemed as if the lymphatic vessels were the seat of the animalcules, as they followed the portal tissue mainly, and accumulated in places, as if placed in a tubular system. The author proposes the question, whether the hooklets disappear after a length of time, or whether it be that some animalcules in their early stage have no hooklets.

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* Verhandl. der Phys.-Med. Gesellsch. Würzburg, Band vi. Heft i. p. 84. 1855.

Collonema in the Brain.—An instance thereof is related by Wagner, of Leipzig.* No history is given of the case. The tumour was equal to a walnut in size, and was removed from the brain of a middle-aged person who died of puerperal fever. It was covered by a thin investment, excepting where its attachment had existed, and was so soft that when moved it trembled like a hydatid. It contained no vessels or any trabecular work, and its general colour was that of clear horn, whilst at its periphery it had places of the size of a pea, of a white or whitish-yellow hue. The investing membrane chiefly consisted of fine, partly streaked, partly irregularly wavy areolar fibres, closely matted together; but here and there intervened tolerably strong areolar bundles, terminating in fine fibrils with an undulatory course. Some fibres, called by Rokitansky "tubular fibres," also existed of a tolerable thickness, somewhat resembling capillaries, but on the addition of acetic acid showing a simple contour, clear walls, and contents at one time thickly, at another sparingly, granulated. In many places also between the first-mentioned fibrous network a very thick network of fine elastic fibres existed, such as is found in many serous membranes, and knot-like swellings existed where the fibres anastamosed. Some corpora amylacea were found along with brain débris on the outer surface of the investing membrane. The tumour itself showed, for the most part, a soft, slimy, amorphous, and very finely-granulated irregularly-striped or cloud-like mass, which only gradually mixed with water, and by addition of acetic acid assumed the form of thick threads and chords, or clumpy masses, becoming fluid on addition of potash water. Numerous structures, like areolar tissue corpuscles, were seen. There were at one place spindle-shaped and narrow corpuscles, passing gradually into fibres; at another place, globular, roundish-oval or irregularly angular. Their contents were variously granulated, exhibiting fine, glistening, darkly-contoured puncta. Some had a clear round nucleus, with or without nucleoli, but most were destitute of nuclei. The processes of the areolar tissue corpuscles were chiefly from two to four in number, and some had from one to two secondary processes from their extremities, and were of various breadths, diminishing towards their extremities. Some of the processes had club-shaped projections on them of various thicknesses. The processes chiefly had clear contents, with occasional dark molecular particles, and by the processes at times the corpuscles anastomosed. Bodies like areolar tissue corpuscles without any processes were also seen. Besides the above, bodies also were seen of $\frac{1}{400}$th to $\frac{1}{200}$th of a line in diameter, round, and with fine refracting contents, many of them containing bright spots, and having the same chemical re-agency as the areolar tissue corpuscles. Numerous sharply-contoured glittering colloid corpuscles, varying in size up to $\frac{1}{80}$th of a line, and very soft, were visible.

All the above microscopical elements were seen in the yellow as well as the white parts, but the two last-named elements were more abundant in the white parts. No stroma, no vessels, and no epithelium lining the surface of the investment, were to be discovered.

The author likens the tumour to two described by Müller, one by Schuh, and some by Rokitansky, at least to a certain extent. He considers the tumour as of non-malignant character.

A Case of Osteoma of the Left Femur is related by Virchow,† in which fibrous masses existed, consisting of round coarse fibres, resembling completely developed muscular fibres of the pregnant uterus, interlacing in the manner of uterine fibroids. Concentric lamination also of membranous material, round fibrous bundles, also existed. The tumour was not otherwise remarkable.

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* Virchow's Archiv, p. 532. Oct. 1855.

† Ibid., p. 524.

A Tumour of peculiar character, affecting the Cancellated Bony Structure of the Foot, is described by Dr. Ballingall.[*] Several similar cases had come under his notice at the Jamsetjee Jejeebhoy Hospital; and though the kind of tumour had been before described, its microscopical characters had never been detailed. The foot becomes enlarged to three or four times its natural size, and at first sight one is reminded of elephantiasis, but the affection does not proceed beyond the ankle. Tubercles exist on the surface like large granulations, many giving forth purulent fluid through openings which are connected with sinuses passing to the centre of the foot. The bony tissue is found almost entirely removed, and the articulation quite destroyed. The bones of the tarsus and metatarsus are replaced by a strong areolar tissue, having spaces in it varying in size up to that of a pea, and filled with granular materials. The pulpy substance is found by the microscope to consist of cells of large size, the walls of which in some cases seem to consist of several layers. They are for the most part circular and oval, and surrounding the walls are transparent fungus of long spicula. Numerous oil globules are also seen. The medullary canal of the tibia is also generally enlarged, and the bone reduced almost to a shell; but no abnormal microscopical appearances are to be detected of these parts. On treating the above-named cells with æther no effect is produced, but the spicula are dissolved by liquor potassæ: the cell remains intact. Dr. Ballingall thinks that the disease is of parasitic origin, and confined to the region of Guzerat. Very little pain attends the course of the disease, and the stumps of legs amputated in consequence of it, healed well. None of the patients operated on were known to return.

Of the following interesting papers we can only give the titles and references: On the New Formation of Striped Muscle in the Testicle.[†] The Developmental History and Surgical Importance of Cysts of the Testis. By Billroth.[‡] The Nature of Colloid-Cystoids. By Dr. Beck.[§] On Cavernous Tumours. By Maier, of Friburg.[||] Trichina Spiralis. By Henle.[¶] Microscopical Examination of the Atmosphere. By M. A. Baudremont.[**]

QUARTERLY REPORT ON PATHOLOGY AND MEDICINE.

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I. *Observations and Remarks on Diseases of the Brain.* By H. Bamberger. (Verhandlungen der Physicalisch-Medicinischen Gesellschaft in Würzburg. Band vi. Heft 2, p. 283.)

Although we are not informed with regard to the number of the cases upon which Dr. Bamberger's remarks are founded, it is manifest that his experience is extensive, and his opinions therefore carry considerable weight. The cases which he does record are of much interest, and embrace almost the whole field of cerebral pathology. The following are the prominent points of his investigations to which we would draw the reader's attention.

Apoplexia Nervosa.—Pathological anatomy has so much narrowed the limits within which it is possible to apply the term nervous apoplexy, that we now rarely meet with cases to which it may be fairly given—viz., those in which sudden death occurs with cerebral symptoms, and in which no palpable lesion is discoverable after death. It is probable that the microscope and pathological chemistry may reveal minute changes that have hitherto escaped detection, and that the term, in its present sense, may have to be entirely eliminated from nosology. Dr. Bamberger is of opinion that sudden death resulting from violent emotions, electricity, and concussion, must be classed in this category. He quotes one case that fell under his observation. A girl, aged twenty, previously in perfect health, was admitted into the Prague Hospital in January, 1850, having the evening before been seized with vomiting, followed by universal convulsions and unconsciousness, brought on by the information received in the morning of the same day that her lover had proved faithless. The temperature of the surface was elevated, the pupils unaltered, the eyes closed, the face pale, respiration stertorous, and the pulse intermittent. There was occasional spasm of the extensors of the upper and lower extremities, and also of the abdominal muscles. The extremities, when raised and allowed to fall, descended as if lifeless, though not actually paralytic. There was no return of consciousness, and she died twenty-eight hours after the seizure.

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[*] Transactions of the Medical and Physical Society of Bombay, p. 278. 1853–4.

[†] Virchow's Archiv. April, 1855.

[‡] Ibid., p. 268. July, 1855.

Illustrirte Medizinische Zeitung, Heft 5.

[||] Virchow's Archiv. April, 1855.

[¶] Henle's Zeitschrift, p. 247.

[**] L'Union Médicale. Oct. 20, 1855.

Necropsy. The brain was pale and anæmic, the walls of the left ventricle of the heart were slightly hypertrophied, the aorta very narrow and its coats thin, the heart and large vessels were full of loose coagula. All other organs were perfectly healthy. There was no suspicion nor any evidence of poisoning.

Apoplexia Serosa.—We are still on debateable ground; for although the occurrence of sudden death, with symptoms of apoplexy, and exhibiting serous effusion into the ventricles, the substance of the brain, or the meninges, is undoubted, the majority of observers (as Abercrombie, Dietl, Wunderlich, Leubuscher) are of opinion that these cases are rarely, if ever, idiopathic. Dr. Bamberger has frequently met with the varieties of acute serous effusion alluded to, but is of opinion that they are always the secondary result either of other cerebral diseases and abnormal states of the cerebral circulation, or of an altered state of the blood induced by some other acute or chronic disease, as granular kidney, typhus, acute exanthemata, tubercular, cardiac, and other maladies.

Meningitis.—Dr. Bamberger adverts briefly to a few points connected with this subject, one of which is the occurrence of inflammation limited to the ventricular lining membrane; he is of opinion that where the post-mortem appearances indicate such a condition, a previous inflammatory exudation on the surface has been reabsorbed, or overlooked as an unessential concomitant.

Cerebral Hæmorrhage.—The author refers all cases of hæmorrhage to increased pressure in the vascular system, or to an altered condition of the coats of the vessels. He denies that passive hæmorrhage accompanying dyscrasic states, results directly from the altered condition of the blood, but from the alterations previously induced in the coats of the vessels. He admits that the latter lesion has not yet been demonstrated. As but few authentic cases of passive hæmorrhage within the cranium are on record, he relates some that have fallen under his own observation in typhus (typhus petechialis), scurvy, and chlorosis. The rarity of the occurrence in typhus is shown by the fact that Dr. Bamberger has only met with it once in above a thousand cases of the disease. In that case, after death, which had ensued on the thirteenth day of the typhus, in a boy, aged fifteen, a cavity of the size of an egg, containing blood that was slightly coagulated, was found in the right corpus striatum. This was also the site of the apoplectic spot found in a girl, aged twenty-five, who died suddenly while under treatment for intense chlorosis. In scurvy, which the author has repeatedly found almost epidemic, he has also met with apoplexy in a girl, aged twenty three, in whom numerous small apoplectic spots were found closely aggregated in the right anterior cerebral lobe, besides another large extravasation on the convexity of the left posterior lobe.

We must pass over the author's observations on the uniform occurrence of the crucial paralysis shown with reference to the facial, fifth, oculomotor, optic, and acoustic nerves; on the rapid return of sensibility, compared with that of motility, in the paralysed half of the body; on hæmorrhage into the pons, the sac of the arachnoid, into the tissue of the pia mater, and the grey matter of the brain.

Red softening occurs in three forms; it may be latent and accompanied with such trifling symptoms as not to induce a suspicion of a cerebral affection; it may be accompanied by symptoms of apoplexy; or it may manifest a very chronic form, in which we meet with the most varied symptoms of cerebral irritation and compression. It is only in the last variety that a diagnosis is possible, though even here there are numerous sources of error. A very peculiar case is detailed, in which the author assumes the conversion of the ordinary products of normal inflammation into tubercle—a view which is certainly at variance with the prevailing opinions on tubercle and the tubercular diathesis. The case is briefly this. A female, aged thirty-five, was seized in the fifth month of her seventh pregnancy with pneumonia, which lasted three weeks; about three weeks later severe headache was followed by sudden rigidity of the left extremities, the fore-arm and leg being flexed; severe convulsive movements of the same extremities ensued, lasting a few minutes. There was no unconsciousness, though she was slightly giddy during the attacks. The rigidity and the temporary spasms continued for a week, when she was admitted into the hospital (November, 1851). She was able to answer questions, but her memory was somewhat impaired. There was occipital headache, paralysis of the left side of the face, violent contraction of the right trapezius, of the left arm and leg; attempts to overcome the flexion caused severe pain. Sensibility of the parts unimpaired, total loss of motility; some improvement took place in the paralytic condition, but in December an epileptic seizure supervened; delivery followed in the same month; further epileptic attacks ensued, with pleurisy in the right side, and advancing tubercular disease of the lungs. Death on the 27th January. The state of the brain was as follows:—On the inner and upper surface of the right hemisphere, a portion of the size of a desert plate exhibited intimate adhesion between the membranes to the brain by means of a greyish-red cellular tissue, and a yellow cheesy friable mass; the subjacent gyri were converted into a similar substance to an extent of 9 to 10 lines, not circumscribed as cerebral tubercle generally is; the cerebral tissue in the immediate vicinity was reddened and softened, the more distant portions almost pulpy. Old and recent tubercles were found in the apices of both lungs; the liver and spleen also showed tubercular deposit. Dr. Bamberger argues that the symptoms showed that the cerebral disease commenced with inflammation, and that therefore the deposit in the brain was the result of a conversion of plastic exudation into tubercle; but it necessarily suggests itself that the tubercular deposit may have been long dormant in the brain, and that the inflammation was a secondary affection. Until such cases are multiplied, it appears illogical to adopt a theory which is opposed to the common experience of pathologists. Two interesting cases are given of encephalitis, resulting from plugging of the arteries by fibrine carried from other portions of the circulating apparatus.

With regard to cerebral abscesses, Dr. Bamberger only confirms the known fact of their remarkable latency. The details of three cases are introduced in evidence.

Paralysis Agitans.—In one necropsy of a female, aged forty-five, who had been subject to constant tremors of both upper extremities and the head from her childhood, the meninges were found opaque, and infiltrated with serum, of which two ounces were found in the ventricles; the brain was otherwise normal. The characteristic feature was found in the spinal cord, which was white and moist, and exhibited throughout the white matter numerous grey, gelatinous spots; from the middle of the cervical to the middle of the dorsal portion there was a central canal, admitting of the passage of a probe. Dr. Bamberger regards the gelatinous spots as the residue of previous inflammation, and the formation of the canal as the result of atrophy of the cord.

Encephalic Tumours.—The diagnosis of encephalic tumours still remains, to a great extent, a matter of guesswork, the symptoms being mainly those of compression, which they share equally with other affections. Of 17 cases observed by Dr. Bamberger, 11 occurred in men, 6 in females—a ratio established by Lebert and Friedrich. They were distributed over the different periods of life as follows:—Under ten years, 1; ten to twenty, 3; twenty to thirty, 4; thirty to forty, 4; forty to fifty, 2; fifty to sixty, 2; sixty to seventy, 1. Six were large tubercular or tuberculoid masses; 2, cancerous; 2, fibrous tumours; 2, simple cysts (not apoplectic); 1, echinococcus; 1, extended hard masses, of an undefined character; 2, osseous tumours in the cerebral tissue; and 1, cholesteatoma. In 10 cases the cerebrum, in 5 the cerebellum, and in 2 both, were affected.

The most uniform symptom was cephalalgia: this was absent only in 2 cases; it was severe and paroxysmal in 6. Paralytic affections occurred next in order of frequency—viz., 10 times; in 5 gradually, in 5 suddenly. Convulsive attacks were met with 8 times: 7 in the form of epilepsy (6 of these with cerebral, 1 with cerebellar, tumours); 1 in the form of convulsive affections of one side of the face. Derangement of the intellectual functions occurred in 8 cases.

The details of 3 cases of encephalic tumours, for which, however, we cannot make room, conclude Dr. Bamberger's interesting communication.

II. *Remarks on the Disease termed Insolatio, or Heat Apoplexy; with Observations on its Pathology.* By Marcus G. Hill, Officiating Assistant Garrison Surgeon, Fort William. (The Indian Annals of Medical Science, Oct. 1855, p. 188.)

We can scarcely do justice to the elaborate paper of Mr. Hill in the brief space that we have at our disposal; to those who take an interest in the subject, we would recommend it as a comprehensive survey and an intelligent analysis of the facts at our disposal. While he eloquently describes the widely-prevailing effects of the intense heat of an Indian sun, he denies that the heat alone causes the disease in question. A *tertium quid* is superadded in the form of intemperance, or some previously debilitating cause. Opinions of numerous well-known writers are quoted—Dr. Mouat, Dr. Johnson, Mr. Martin, and others—in support of the view of the author, that there is so close an analogy between remittent fever and head apoplexy as to amount to an identity. The difference between the two he regards as consisting mainly in the greater concentration of the poison, and the fatality of the event; he describes insolatio as an exaggerated attack of remittent fever, which few individuals possess the vigour of constitution to struggle through. A table of cases collected by Mr. Hill shows at a glance the fearful mortality of insolatio. Of 504 seizures there were no less than 259 deaths; of the remaining number 8 were doubtful, so that the per-centage of deaths to seizures was as 51·38, and of recoveries to seizures, as 45·03. Formerly, the disease was regarded as a primary inflammation of the brain; the more careful study of the post-mortem conditions shows that we have not to deal with any inflammation at all, and that the brain is only secondarily involved. The most uniform lesion met with after death is intense congestion of the lungs, amounting at times to an apoplectic condition; the organs being, as Dr. Mortimer describes them in his cases, "almost black, and to all appearances completely obstructed;" whereas the utmost trace of disease found in the brain is congestion of the superficial vessels, with some serous effusion under the arachnoid.

"But not only," says Mr. Hill, "do the after-death appearances favour the idea of the lungs primarily suffering, but the premonitory symptoms, though often referred to the head, are nevertheless as frequently concomitant with disagreeable sensations about the chest; and however we may feel inclined to dispute the point, there exists an extraordinary analogy between these cases of heat apoplexy and poisoning by carbonic acid gas, whether we look to the mode of accession, the sensations of the patient, the symptoms, the phenomena of the disease, the manner of death, the remarkable retention of the heat long after death, or to the post-mortem appearances. Likewise, the treatment so successful in these attacks, is the same which is found necessary in cases of poisoning by carbonic acid."

This theory is still further developed, and the views of different authors—Macculloch, Holland, Alison—are quoted in support of the analogy of malarious poisoning, with a circulation through the whole system of an over-carbonized blood. Knowing how much often depends upon the *name* given to a disease, and how much doubt and theory influence the selection in a given case, we fully sympathize with the following observations of Mr. Hill, to be found in the concluding remarks of his important paper:

"The facts on my side would have been much stronger had a more accurate system of nosology been adopted in classing this disease, for there can be no doubt that many cases of heat apoplexy have been arranged either as remittent fever or as apoplexy. When death has been rapid, then perhaps the latter name has been selected; whilst on the other hand, when the first symptoms have abated, and death does not occur for some time, or at all, then the former designation would appear to be preferred."

III. *Communications on Diseases of the Cerebral Nerves.* By Dr. LUDWIG TURCK. (Zeitschrift der k. k. Gesellschaft der Aerzte zu Wien, 11th Jahrg., Monatheft ix. and x. p. 517.)

The first case given by Dr. Türck is one of compression of the right olfactory nerve against the bone, by means of cerebral cancer, producing, about four weeks before the death of the individual (a man of thirty-two years), entire anosmia. The nerve itself exhibited numerous glomeruli, but no diminution in the number of nerve-tubes.

A series of cases, in which the optic nerves were involved in diseased conditions, follow. Dr. Türck has met with eleven instances of compression of the chiasma, resulting from chronic enlargement of the brain, induced by cancerous or tubercular disease. In all these cases, a consecutive degeneration of the entire optic nerve had followed, to which amblyopia, or complete amaurosis, with sluggishness of the pupil of the corresponding side, were attributable. In two cases, the part immediately behind the chiasma was pressed downwards by morbid growth upon that section of the circle of Willis from which the posterior communicating branch arises from the internal carotid, so that the anterior part of the optic tract was constricted by the internal carotid and the posterior communicating artery.

Tubercular meningitis in three cases produced pathological changes in the nerves at the base of the brain. In one, the third pair were flattened against the posterior clinoid processes, and the arachnoid of the base was opaque and granular. The nerves, examined by a lens, were found to be uniformly reddened, and their bloodvessels much congested; under the microscope, they exhibited between the intact nerve-tubes a fine molecular mass. Similar changes were observed in the sixth pair, so far as they are in contact with the arachnoid. The right nerve was, in each instance, more affected than the left. During life there had, for six days before death, been complete paralysis of the third pair, with paralysis of left abducens, that of the right being doubtful. In the second case of tubercular meningitis, there had been intense paralysis of the left motor-oculi nerve for nine days before death, and the nerve was found distinctly reddened and injected to the distance of three or four lines beyond its exit from the arachnoid, all the other nerves being entirely pale. In the third case, where paralysis of the left oculo-motor had supervened during the last days of life, the nerve was found visibly injected, and presented in its interior a few small spots of capillary extravasation. In the last two cases, the nerve-tubules were unaltered; nor was any molecular deposit observed.

Several cases are detailed in which cerebral nerves were found degenerated. In a female, aged twenty-seven, who died of carcinoma of the brain and spinal cord, and who had exhibited divergent strabismus, with immovable pupils, the oculo-motores presented a lardaceous appearance, and almost cartilaginous hardness; the tubular structure had almost entirely disappeared. In a female, aged thirty-eight, who died hemiplegic, and had exhibited complete paralysis of the right oculo-motor nerve, the nerve was found reddish-grey, irregularly thickened, and after its entrance into the orbit, uniformly thickened and harder, opaque, and greyish-yellow.

Well-marked atrophy of the right abducens was noticed in a girl of sixteen, who, from her second year, had been affected with convergent strabismus, following an attack of meningitis. A similar condition was observed in a man who, after a temporary paralysis of the right rectus externus, died of hemiplegia. Atrophy of the two accessory and hypoglossal nerves was found in a female, aged forty, who, for a year before her death, had been affected with gradually increasing universal paralysis. The nerves were reduced in size, of a reddish hue, and presented fatty degeneration of some of their tubes.

Similar conditions were observed in other cases of disease of the osseous textures at the base of the brain, involving secondarily the optic, ophthalmic, abducens, facial, hypoglossal, and accessory nerves; and in which the derangement of function during life corresponded with the lesion of the respective nerves. The lesion consisted in compression, in inflammation of the nerve, or in infiltration of its tissue with cancerous products.

IV. *Obliteration of the Thoracic Aorta.* (Wochenblatt der Zeitschrift der k. k. Gesellschaft der Aerzte zu Wien, Nov. 5, 1855.)

At a meeting of the Medical Society of Vienna, held on the 19th October, 1855, Professor Skoda introduced a man affected with obliteration of the thoracic aorta. In illustration of the lesion, the Professor exhibited preparations of a five-months' fœtus and of a new-born child, in which he indicated the point at which alone this anomaly can take place or has hitherto been observed. It is the point at which the ductus botalli communicates with the aorta and the short space intervening between this point and the origin of the left subclavian artery. During fœtal life, this portion is commonly narrower than the remainder of the aorta, and only acquires the same calibre after birth.

The individual in question was a man, aged forty-seven; a jeweller; of normal complexion, and throughout well nourished. On the whole, he enjoys good health, and has only come under clinical observation owing to his having, for three years past, suffered from some dyspnœa in making violent exertion. This is due to an insufficiency of the tricuspid valve, which has only been established for three years.

The following are the grounds upon which Professor Skoda has diagnosed a co-existing obliteration of the aorta:—In addition to the blowing murmur coincident with the impulse, and which indicates the above-mentioned insufficiency, a "peculiar vibration or whirring (schwirren) is to be perceived over the greater part of the thorax, partly by palpation, partly, as in the course of the intercostal arteries, by auscultation; it follows the impulse, and for that reason has its seat in the arteries. The vibration of the arteries of the thorax is due to their dilatation, as may be shown by touching the superficial epigastrio arteries, which are much dilated and very tortuous. The beat of the crural arteries at the groin is very feeble, and no pulsation can be felt in the abdominal aorta."

These are the indications characteristic of obliteration of the thoracic aorta; the collateral circulation is carried on by the branches of the subclavian arteries, which must therefore be dilated. A large volume of blood passes from the anterior intercostals to the posterior intercostal, and by centripetal movement reaches the descending aorta, which is thus filled with blood sufficient to supply the arteries of the intestines, but not sufficient to produce distinct pulsations. The inferior extremities probably also receive a supply by the anastomosis of the superior and inferior epigastric arteries. No cyanosis is observed, because nowhere venous blood is introduced into the arterial system.

In connexion with this case, Professor Skoda made the following remarks:— 1. That in examining the heart, we occasionally perceive murmurs which give rise to the assumption of valvular disease, while the heart is afterwards found healthy; and that the murmur was produced in the coronary arteries or in other arteries, in the vicinity of the heart. Such errors can only be avoided by carefully attending, as in the case detailed, to the coincidence or non-coincidence of the murmur with the movements of the heart. 2. The circumstance that the nutrition of the individual was unimpaired, although the circulation in most of the organs must be, doubtless, slackened, proves that the deranged nutrition, so frequently coinciding with impediments in the circulation, does not depend solely upon the latter.

Professor Skoda was of opinion that the obliteration of the aorta was due either to a complete obliteration or absence of the corresponding portion of aorta in the fœtus, or to the contraction of the latter coincidently with the ductus botalli, owing to the exceptional extension of the tissue of this channel into the coats of the aorta. Professor Skoda maintained that the obliteration could not be set down to inflammation, as arteritis led, not to obliteration, but to aneurism. He referred to an analogous case which had occurred in his wards some years previously, where no disturbance of function was manifested until, accidentally, endocarditis supervened. Death occurred later from pneumonia; and the obliterated aorta has been preserved in the anatomical museum of Vienna.

V. *Statistics of Delirium Tremens.* By JOHN MACPHERSON, M.D.

(Indian Annals of Medical Science, October, 1855.)

Dr. Macpherson draws attention to the great discrepancy prevailing in the statistics of writers on delirium tremens, with regard both to its frequency in both sexes, and to the mortality of the disease. He attributes this chiefly to a want of due classification—ebrietas or drunkenness being returned as delirium tremens. Calmeil states the rate of mortality at 5 per cent., Bougard at 19 per cent. Colonel Tulloch, in his report for 1853, gives the following per-centages of mortality:

Great Britain, infantry	17·6
„ cavalry	13·8
Bermuda	15·
Canada	7·94
Gibraltar	13·6
Malta	8·8
Nova Scotia	9·1

With regard to Bengal, the author says: "I believe I should not be far wrong in stating, that an army of about 18,000 strong sends more than 600 cases of ebrietas into the hospital in the year (slight cases are not sent), and that although the number of cases of delirium tremens is reduced by more than one-half, yet even now it rarely falls short of 150; and that the proportion in Bombay and Madras is not very different."

A return of admissions and deaths from delirium tremens and ebrietas in the General Hospital in Calcutta, from 1848 to 1852, and another of admissions and deaths from the same causes in the Medical College Hospital, during 1851-52-53, is given. The following are some of the more important results offered by the analysis of these cases:

That delirium tremens occurs in women and men in the proportion of 1 to 25; but that this difference is due to the difference of habits rather than of sex.

That in regard to age, the ratio is as follows:

	Cases.	Deaths.	Per cent. of deaths.
Ages from 20 to 25	34	4	9·1
„ 25 to 30	66	16	24·2
„ 30 to 35	48	11	22·9
„ 35 to 40	76	7	9·2
„ 40 to 45	62	6	9·6
„ 45 to 50	23	4	17·3
„ 50 to 60	7	0	
„ 60 to 65	5	1	

The greatest mortality is between the ages of twenty-five to forty, which is confirmed by the analysis of another series of sixty-four fatal cases. The percentage shows that there is no uniformity in the proportion of deaths to the number of cases.

There is no evidence to show that the season of the year exerts a definite influence on the occurrence of the disease, whereas the mortality very palpably varies with the temperature—it being more than double in the eight hot, than in the four cold months.

The apparent cause of death was as follows:

No. of cases.	
33	by exhaustion (often with coma).
18	by coma.
11	by fits (sometimes apoplectic, called sometimes epileptic).
1	died on nightstool.
1	found dead in bed.

Convulsions occurred in at least twenty of the above cases. One distinct case of paroxysmal opisthotonos occurred in a musician, who, during the intervals, was able to sit up and whistle tunes.

The post-mortem appearances of forty-five cases are not given with that statistical accuracy which we should desire; but they afford a confirmation to the received opinions of pathologists:

"As to the general frequency of the morbid changes, it may be observed, that the most constant were the red patches in the stomach; next, the opalescent appearance of the arachnoid; next, serous effusion; next, change of liver; next, of heart; next, of spleen; and lastly, of kidneys; but the last organs do not appear to have been always examined."

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E., London.

I. *On the Pathological Changes produced in the Urinary Organs in Egypt by the Distomum Hæmatobium.* By Dr. Bilharz. (Wien Med. Woch., 1856, Nos. 4 and 5.)

Dr. Bilharz, a physician practising at Cairo, forwarded several observations to Professor Siebold upon an entozoon peculiar to the inhabitants of Egypt, and which were published in the 'Zeits. für Wiss. Zool.,' Band iv. pp. 53 and 454. In that paper he chiefly occupied himself with the zoological and anatomical characters of the parasite, while in the present one he publishes an account of the ravages it gives rise to. It is found in large numbers in the vena portæ, its roots and branches, as well as in the hæmorrhoidal and vesical plexus of the indigenous Egyptians, while myriads of ova are deposited in the mucous and submucous tissues of the bladder, ureters, and rectum. The entozoon belongs to the order trematoda, and the author denominates it *distomum hæmatobium.* Its length is about four lines, and the characters of the sexes are distinct. For these we must refer to the paper cited, our business being with the practical relation only. The ova are about $\frac{1}{18}$th of a line, and $\frac{1}{60}$th in breadth. The entozoon is nourished by the blood amidst which it lives, its intestinal canal always being found full of the corpuscles. It is of such very frequent occurrence among the indigenous Egyptians (the Fellahs and Kopts), that it would not be an exaggeration to state that one-half of the adults exhibit the worm or traces of its presence. It is not rare to meet with it among the Nubians also, but all of those who had come under the author's observation had lived long in Egypt. In regard to the Turks and Europeans residing in Egypt, the author has never met with a case among them presenting symptoms of suffering from the parasite, and in these races post-mortem examination is not allowed. Among numerous autopsies of negroes, the author only met with the worm and its ova once.

The frequent occurrence of diseases of the urinary apparatus, and especially *lithiasis*, in the indigenous Egyptians, has been noted both by old and modern writers; and, in fact, among many pathological peculiarities which dissection reveals, scarcely any are more striking than the great frequency of the anatomical changes in these organs, especially the bladder and ureters.

Catarrhal Inflammation of the Bladder and Ureters.—The acute stage of this affection is seldom seen alone, there being, at least at certain points, degeneration perceptible. At these parts the mucous membrane is somewhat swollen and loosened, of a bluish or brownish red, surrounded by varicose capillaries, and covered with a layer of tough, transparent mucus, which often contains scattered blood-corpuscles. This can be easily stripped off as a pellicle, and consists of inter-adherent epithelial cells. Minute bloody points occupy the mouths of small bloodvessels that open on the surface. In all cases very large numbers of the ova of the distomum are here found, while the parts which are still normal do not exhibit these. They are found in the sub-mucous tissue, imbedded in the mucous membrane, in the pellicle formed by the adherent epithelial cells, and in the small coagula which project from the capillaries. The ova are either scattered, or massed together by means of a transparent gelatinous substance. They contain the embryo, sometimes in the mature and sometimes the immature state, side by side with burst shells, which are either empty or contain fat or lime. As consequences of this condition of the mucous membrane, M. Bilharz specifies induration, polypous growths, and ulceration.

1. *Induration* is the most frequent and marked of these. The mucous membrane becomes thickened, of a greenish or greyish yellow, devoid of blood, and of a leathery toughness. On nearer examination a number of minute, shining, burst granules are observed, giving the appearance of fine-grained sandstone, and under the microscope proving to consist of innumerable ova imbedded in the tissue, containing no living beings, but some filled with fat, and the greater part with lime. More or less tenacious layers cover the diseased portions, which contain also many ova filled as above, a few of them also containing uric acid. Pruner has already shown that there are frequently calculous deposits in these rough leathery patches. These concretions, of the size of millet-seed, consist chiefly of uric acid, and are sometimes slightly, and at others closely, adherent to the mass. Within the smallest of these, ova are sometimes found, but they are usually homogeneous. Besides these concretions there are often minute microscopic molecules deposited, which resemble urate of ammonia. This leathery degeneration may affect any part of the bladder, and frequently occupies half of its surface. In the ureters, ring-like deposits take place, so narrowing their cavity that a small catheter can scarcely be passed, while in one case complete obturation occurred. As a consequence of this obstruction, the rest of the ureter, the pelvis, and calices of the kidney undergo dilatation. This degeneration may occur at various points, the commonest being the vesical orifice, giving rise to the dilatation of the ureter throughout its whole course. 2. *Polypous hypertrophy* is frequently observed under the form of a fungoid prolongation of the mucous membrane, of the size of a millet to that of a bean, sometimes pediculated, and at others with a broad base; sometimes superficially lobed, and at others rounded, deep red, vascular, and often covered with incrustations, which consist in part of ova, in part of urinary salts. On dividing these, the mucous membrane is found thickened; and the sub-mucous tissue hypertrophied; both being traversed by an abundant capillary network, the vessels being not unfrequently dilated into tolerably spacious intercommunicating cavities. These cavities often contained the distomum, while the ova were found in great numbers within the parenchyma of the excrescences. This form of degeneration is found co-existing with the leathery, though seldomer than it, and it is rarely met with in the ureters. 3. *Ulceration* has only been met with in one case in a portion of the bladder affected with acute inflammation; the

ulcer, which contained masses of ova, much resembling those observed in the intestines in dysentery.

The author expresses his conviction that the deposit of the ova of the distomum hæmatobium is the immediate cause of these pathological appearances; inasmuch as in all the cases in which they have been observed, large quantities of ova have been found, the appearances have been proportionate in amount and severity to the number and degree of development of the ova, and such appearances are only observed among individuals suffering from the presence of the distoma, which is only found in certain races of men.

The *symptoms* of this very chronic affection, which may last for years, are in part common to simple chronic catarrh of the bladder, and in part derived from the presence of the distoma. Among the former, constant sense of weight in the hypogastric region, exacerbated at times into severe and burning pain; sensibility to pressure, and very persistent though but slight hæmaturia. The catheter passing over the roughened spots may give rise to the suspicion of stone, but the dull rubbing feeling and the immovability of the part enable us to distinguish, while a finger passed per anum may often bring the portion of bladder between it and the catheter. The easiest means of diagnosis is furnished by the microscope, the ova being detected by it in the mucous sediment of the urine, and especially in the small coagula of blood that accompany this. In regard to *treatment*, the local pains are best relieved by opium, while the radical cure has several times been attempted by endeavouring to poison the parasite with long-continued doses of calomel. Thus far, the author has had no case long enough under his observation to determine the amount of success. *Lithiasis*, owing to the frequency with which uric-acid concretions are met with on the surface of the parts that had undergone the leathery degeneration, being mostly but lightly attached, they might be expected to be easily separated so as to form nuclei for calculi. The obstructions too in the ureters would favour deposition from the urine. Dr. Heinrich Meckel, examining Professor Reyer's collection of Egyptian urinary calculi, has found one containing a large quantity of the ova.

II. *Cases of Gun-shot Wound of the Orbit.* By Drs. Warren and Bethune. (Boston Medical and Surgical Journal, vol. liii. p. 226.)

At a meeting of the Boston Medical Society, Dr. J. M. Warren related the following case:—A man, aged thirty-five, received, in 1847, a severe wound of the head, from the breech-pin of his gun, which exploded. He states that the left eye-ball was blown out, the upper part of the socket destroyed, so as to expose the brain, and a communication formed between its back part and the nasal sinuses. His recovery was very slow, and he suffered much from pain in the head, dizziness, &c. The nose was entirely stopped up, so that he could not breathe through it. At the end of rather more than a year, a firmness was felt on the hard palate, and something seemed to obstruct the posterior fauces. A screw was found projecting through the roof of the mouth, and an incision having been made, the whole breech-pin, with the screw projecting from it at a right angle, was removed, after remaining there unsuspected during eighteen months. It was three inches and a quarter long, and almost three inches in diameter. Admitted into the hospital, December 1, 1854, the left eye-ball was found to be gone, the eyelids, apparently uninjured, remaining open. Free communication took place with the mouth and nose by means of an aperture at the back of the socket, the edge of this being irregular where the bone had been destroyed. He could only speak intelligibly when he closed the eyelids by pressing his fingers into the socket, preventing the passage of the air from the mouth; and even then, owing to the fissure of the palate, he was not easily understood. Swallowing was difficult, and required an upright position of the head.

In order to obstruct the passage of air through the socket, the tarsal cartilages were removed (the patient being etherized), the edges brought together with sutures, and collodion applied—the speech becoming at once much improved. The fissure of the palate was closed by operation, the next week, but without success, owing to the intractibility of the patient. A repetition was entirely successful. The eyelids united, with the exception of an aperture the size of a pin's head at the inner angle, through which no air passed, but which gave rise to a thin discharge like tears, apparently indicating the remains of a small portion of the lachrymal gland, although all the part of the orbit to which this is attached seemed to have been destroyed. The patient left, March, 1855, with his voice in a great measure restored.

Dr. Bethune was called to a young man who had tried to blow his brains out six days previously. The ball carried away the right eye-ball, and, going behind the upper part of the nose, passed out of the left orbit, carrying away about a third of the left eye. No severe general symptoms followed; the globes suppurated favourably; and at the period of the relation of the case to the Society, about a fortnight after the wound, there seemed every probability of recovery.

III. *On the Treatment of Fistula Lachrymalis.* By M. TAVIGNOT. (Moniteur des Hôpitaux, 1856, No. 16.)

M. Tavignot is of opinion that fistula lachrymalis is the result of an organic disaccord between the chemical properties of the tears and the physiological properties of the naso-lachrymal mucous membrance. This explains both the obstinacy of the disease and the relative efficacy of that treatment which most protects the mucous membrane from the contact of the tears. We find the tears will not flow through the canal, even when it has been dilated by surgical means; while the presence of a foreign body in the canal causes the cessation of the accidents; this being better tolerated than the tears, the access of which it prevents. These various modes of treatment only succeed after long preseverance has modified and transformed the characters of the mucous membrane.

In place of occupying so long a time in obtaining this alteration in the sac and the duct, the author recommends that the gland itself should engage our attention. Where the affection does not arise from scrofulous disease, when it is amenable to appropriate remedies, he is unaware of any means of restoring harmony to the parts, although in the early stages antiphlogistics and topical remedies do much to remove complications and procure temporary relief. The contact of the tears can only, by the various means usually employed, be temporarily prevented, while obliteration of the passages is difficult to obtain, and is attended with stillicidium. The lachrymal gland itself may, however, be removed without inconvenience. It is, in fact, the orbital portion that is alone to be removed; and the palpebral granules that remain, suffice, with the mucus of the membranes, to lubrify the surface of the eye. The operation is inoffensive. Very soon great amelioration ensues, after the immediate effects of the operation have passed away, and this may go on to a definitive cure. When this is delayed, owing to the still disordered state of the passages, iodine injections should be employed.

IV. *Glycerine as a Dressing for Wounds and Ulcers.* (Gaz. des Hôpitaux, 1856, Nos. 144 and 146.)

M. Denonvilliers has recently brought this under the notice of the Paris Société de Chirurgie, speaking highly of this substance as forming so very clean a dressing, and which, not adhering to the parts, is easily removed. To its extreme cleanliness he attributes much of its efficacy, contrasting it with the cerate dressings, under the use of which there takes place around the edge of the wound a mixed accumulation of pus, cerate, and epidermis, forming a thick crust, the removal of which creates irritation and retards recovery.

M. Demarquay stated that he had also employed it with good effect in hospital gangrene, in open buboes, syphilitic ulceration, ill-conditioned ulcers of the mouth, &c. All these sores healed rapidly, seeming to imply a certain amount of local action, which the astringent and stinging taste of the substance accounted for. It may also exert some influence on erysipelas, or at all events the occurrence of this during its use is rare. It undoubtedly modifies the amount of suppuration; and when the supply of glycerine has run short, the wounds next day have secreted more pus, and assumed a worse aspect.

M. Brocas, while advocating the use of glycerine as a clean substitute for cerate, deprecates the vaunting it as a panacea for the various forms of wounds. As to hospital gangrene, it is now met with in a far milder form than that described by the classical authors, and may become cured by hygiene alone, the glycerine merely allowing the irritated parts to be left at rest. Believing this disease may be propagated by miasmata, glycerine may also act by protecting the wound from the contact of the air; for excellent practical results have followed when this has been prevented, by covering the patient's wounds with gummed gold-beater's skin.

M. Dallas stated that he had employed glycerine at Odessa since 1851, where, as in other parts of Russia, it has become a highly-popular remedy, not only as a dressing for wounds, but in a variety of affections. He has found it also very useful in many cases of deafness, in various cutaneous diseases, especially when attended with itching; and in some forms of ophthalmia.

M. Robin observed, that, deceived by its mere appearance, glycerine has been compared with the oils, and been supposed to act as an inert substance, protecting wounds as neutral fatty bodies do. It is, however, a body analogous to alcohol; and so far from being inert in its action on the organic tissues, it intimately penetrates these more rapidly than water does, and exerts a special action on several of them.

V. *On a New Operation for Phymosis.* By M. BONNAFONT. (Gaz. des Hôpitaux, 1856, No. 2.)

The usual operation of circumcision for phymosis requires a separate division of the mucous membrane, which is attended with great pain. M. Bonnafont, surgeon to the military hospital of the Roule, where he meets with many of these cases, operates as follows. An assistant draws the prepuce forwards, endeavouring to enlarge the orifice as much as possible, through which the operator introduces, by means of a director, &c., either fine charpie or tow, until the whole cavity is filled. This done, the operator may circumcise without fear of injuring the glans. M. Bonnafont, in order to avoid the small artery of the frænum, which sometimes gives rise to an abundant hæmorrhage, makes his incision slightly ovular, from above downwards, and from behind forwards. If it be thought desirable, the skin may be first incised, so that the mucous membrane may be divided somewhat more backwards—a procedure to which some attach importance. For twenty-four hours cold applications are made, no sutures being employed.

VI. *On the Congelations observed at Constantinople in the Winter of* 1854—5. By M. LEGOUEST. (Rev. Médico-Chir., xviii. pp. 270 and 335.)

Congelations, in all their stages, were frequently observed among the troops sent home from the Crimea. The cold to which they had been exposed during December, January, and February, had not been very intense, and had been accompanied by much wet. Almost without fuel, and insufficiently sheltered, many had passed a fortnight without change of clothing, alternating long periods of immovability, their legs half buried in snow at the trenches, with the frozen slush of their bivouacs.

The first stage of ordinary childblain was rarely met with, and the slow-healing ulcers which sometimes resulted were advantageously treated by tar ointment. A more common result of the first degree of cold, and usually resulting from the long retention of wet clothing, was the production of a reddish-brown induration of the skin, which extended over a considerable portion of the external surface of the feet and legs. It was of a very chronic nature, the sensibility of the parts which was quite destroyed, not being restored sometimes for five or six months. Stimulating frictions and warm clothing formed the best treatment.

The second stage of congelation was of much more frequent occurrence, and was characterized by the production of phlyctænæ containing a purulent serosity, or, more frequently, blood. These last have only been met with on the feet, being usually found at the plantar surface, and where the epidermis is thickest. The entire heel, or anterior part of the foot and toes, may be implicated, the epidermis being stained black. The effusion feels hard, is bounded by no areola, and sounds like mummified tissue on percussion. On opening it, the blood does not issue. Viscous at first, it shortly concretes into a very black deposit resembling dried varnish, and scaling off. Its detachment takes a long period to effect, and when completed, a new dermis, with epidermis, may sometimes be found beneath; while at others, exquisitely sensible and exuberant blackish granulations are found springing up from an ulcerated surface. When not seen early, the parts, prior to the detachment of the epidermis, can with difficulty be distinguished from dry gangrene.

A third stage is marked by spots of a blackish-blue colour, the size of small pieces of money, and which are sometimes placed amidst healthy structures, and at others amidst the brown induration of chronic frost-bite. They are soft eschars, visible through the transparent epidermis; and, becoming detached at a remote period, are replaced by fungous, bleeding granulations. At other times they fall as if they had been punched out, leaving reddish, nearly dry, tissue beneath, having no tendency to cicatrization. These differences in the fall of the eschar seem to be due to the degree of depth to which the alteration of tissues has taken place, this being greatest in the first case. The patients complain of little pain. These eschars were very often multiple on the same foot, the projecting portions being especially liable to them, the bones and joints of the toes often suffering.

When the cold acts with still greater intensity, another form of gangrene results, which may be called sudden gangrene, and differs essentially from that which follows reaction after exposure to cold The parts are of a deep livid colour, somewhat tumified, and gorged with fluid, all sensibility having disappeared. Entire toes, the whole foot, or even part of the leg, may be attacked; but this gangrene possesses no invading tendency. After awhile, varying in individuals, but always long, the parts desiccate, shrivel, and mummify, acquiring all the hardness and resonance of wood. A slight inflammatory circle extends just beyond the line of demarcation, redness, and liability to gangrene on pressure; sometimes, however, extending as far as 15 to 20 centimètres. This form bears the greatest analogy to dry or senile gangrene. Some patients, however, pass through all the phases of soft gangrene, which ensues as a consequence of a certain amount of reaction due to the less amount of cold, or the more energetic and active condition of the subject. But the author has never observed these secondary gangrenes putting on the rapid invading characters met with in traumatic gangrene.

At the numerous autopsies performed, besides the usual appearances observed in the soft parts, the bones were found to have become friable, and to have undergone rarefaction of their substance, the areolæ imbibing a yellowish, glairy, sanguinolent, or purulent fluid. This change was best seen towards their extremities, where they could be easily divided by a scalpel. It was rare for a bone not to suffer during a mortification of the tissues, even when a certain thickness of these covered it; and when once attacked, its entire length usually suffered. The latter circumstance seriously compromised the existence of parts at first uninjured by the cold. Amidst the depth of the adipose tissue of the plantar region, in patients who had had only some toes frozen, or in others in whom the feet were intact, small effusions of coagulated blood were often found, varying in size from a millet seed to a barbary, which last they much resembled. Similar effusions, though smaller and less well-defined, have been met with in the vicinity of eschars and ulcers, and in chronic frost-bite. They were also very often met with in the cellular tissue surrounding the nerves and vessels, or in their very sheaths. M. Tholozen regards them as of scorbutic origin.

The account of congelations given by M. Legouest differs from that of some of his predecessors, and he believes that epidemic *scorbutus*, though not the cause of what he has related, has much modified appearances. Speaking of the general symptoms he witnessed, he states that it is often most difficult to distinguish between the remote effects of cold and scorbutics. Most patients exhibited much emaciation and a jaundiced colour, and complained of severe pains in the limbs. Great slowness of movement and torpidity, and a leaden sleep, were observed in most. Many were attacked with incoercible diarrhœa, and some with painless dysentery, and these cases were very fatal. Permanent improvement took place in most, while in others their condition became aggravated, and all the symptoms of scorbutics were present, except the affection of the gums, which was rare.

Passing over the treatment of the slight congelations, the author cautions surgeons not to mistake the sub-epidermic sanguineous effusions for the mummified gangrene they so well simulate, and remove parts still living. As to the gangrene itself, its treatment is as under other circumstances, the author believing he has derived some advantage from the use of solution of sulphate of iron, which solidifies the eschars and corrects their smell. He prefers, as a general rule, temporizing to amputation, coming to nature's aid, however, with regularizing operations, thus saving the patient much suffering, and diminishing the time he has to pass in the dangerous atmosphere of a crowded hospital.

VII. *On Continuous Local Tepid Baths in the Treatment of Wounds after Operations.* By Professor Langenbeck. (L'Union Médicale, 1856, Nos. 11 and 12, from the 'Deutsche Klinik.')

By various apparatus, constructed in zinc or vulcanized caoutchouc, Professor Langenbeck contrives to keep the wounded part in constant contact with tepid water. The apparatus must not be resorted to where secondary hæmorrhage is feared, and thus its application to stumps after amputation should be usually delayed for eighteen to twenty-four hours. In several cases it has, however, been resorted to, even before the patient has recovered from the anæsthesia, this saving him from the pains after the operation and from the dressing. The part must be removed from the bath if hæmorrhage occurs. When applied immediately, the water should be at a temperature of from 10° to 13° C.; and if the water is not renewed it acquires in from three to twelve hours a temperature of 15° to 31°. After the first day the latter is that which is most agreeable to the patient; and later, when the wound begins to clean and suppurate, a temperature of 34° to 35° is to be maintained. The patient's sensations usually form the best guide; and the temperature can be maintained pretty equable by covering the apparatus, or by adding warm or cold water from time to time. In summer, with a temperature of 20° to 25°, the water rises in twelve hours to from 34° to 37°; and in winter, at 17° in the room, the water falls to 31° or 30° in the twelve hours. As a general rule, the water requires renewing only night and morning; and if there is a large wound, with abundant suppuration, it should be well washed with a chlorined solution.

The advantages of the procedure are thus summed up:—1. Diminution of pain subsequent to the operation. As long as the parts are kept under water, whatever the size of the wound, no pain is complained of, although this at once becomes severe when they are exposed, general shivering then, too, coming on in a quarter of an hour. The author has never observed the shiverings, so frequently met with after large operations, when the water was at once applied. No dressings are required, the sutures are removed under water, and the greatest cleanliness is secured. 2. The traumatic and suppurative fever is much diminished in intensity. 3. The removal of the secreted fluids is favoured, and their decomposition prevented. If the wounds are deep or sinuous, injections must be used, and the free issue of the discharges must be secured by the usual means. 4. Cicatrization is more prompt. 5. Professor Langenbeck believes the means to be operative in preventing purulent infection.

VIII. *Tincture of Iodine in Bubo.* By M. Pirondy. (Bull. de Thérapeut., xlix. p. 276.)

M. Pirondy relates 16 cases in which he employed this means, which, he says, often leads to the resorption of pus. Having removed the skin by means of a blister, he dresses the part two or three times a day with charpie dipped in tincture of iodine, diluted with water according to the sensibility of the patient. In 11 of the 16 cases, resorption took place at the average period of twenty-three days. In 5 a spontaneous opening occurred, yet, owing to the detachment of the integuments being less, and their different layers being rendered firmer by this kind of tanning process, cicatrization took place more rapidly than usual, complete healing taking place in a medium period of forty days.

IX. *On Reduction of Dislocated Maxilla.* By Dr. Leo. (Schmidt's Jahrb., Band lxxxvii. 233.)

Dr. Leo strongly recommends the following procedure, on account of its simplicity and easy execution. The surgeon places himself behind the patient (who is sitting) on the right side, taking his head under the left arm and pressing it against the chest. He passes the thumb of the right hand into his mouth as far as the last teeth on the right side of the jaw, surrounding the external side of the jaw with his other fingers, and exerts moderate pressure downwards. As soon as the jaw becomes moveable he presses it backwards. In dislocations of the left side he fixes the head with his right arm, and replaces the jaw with the left hand; and when the dislocation is double, he reduces first one side and then the other.

X. *On the Orange-coloured Flocculi observed in Recent Wounds.* By Professor Zeis. (Gaz. Médicale, 1855, No. 50.)

Professor Zeis's attention was called to this appearance six or eight years since. It is never observed before the fourth day, and it persists from four to eight days. Filaments are found covered with matter of a brilliant orange colour, and of the consistence of good pus, the wound never being entirely covered with this mass. When we try to remove it, a portion always remains adherent at the bottom of the wound; next day it is found reproduced, even though not a single drop of blood has become mingled with the pus; but when it has once disappeared of itself, on the establishment of free suppuration and granulations, it never reappears. The author thinks he has oftenest seen it in lacerated wounds, and when aponeuroses have been exposed; but he does not regard it as a cause for unfavourable prognosis. Under the microscope, an amorphous substance is perceived with the pus globules, which are partly destroyed, as also fatty drops (margarine). Some contain brown or orange-coloured rhomboid crystals, which present all the characters of hæmatoidine. Sometimes crystals are not discoverable, an unorganized brownish mass being indeed the commonest appearance, amidst which crystals are rarely seen. In one of the preparations exhibited the crystals were large, and in enormous quantities. Dr. Zeis concludes that the composition of the mass is the same in both cases, conditions favourable to crystallization being absent in the one.

M. Robin believes the orange colour of the filaments to be due (1) to the presence of crystals of hæmatoidine, (2) to that of an amorphous colouring matter or liquid, which would appear to be either amorphous liquid hæmatoidine, or rather the colouring matter of the blood separated from the red globules destroyed during the eliminating process, or after minute capillary hæmorrhages. That is to say, it is the colouring matter (hæmatoine), naturally semi-liquid and coagulable, which has not yet undergone the special chemical modification which causes it, in certain pathological conditions, to pass into the state of hæmatoidine, a solid, slightly coagulable and crystallizable body.

XI. *Clysters of Acetate of Lead in Hernia.* By D. Ulmann. (Schmidt's Jahrb., Band lxxxvii. 335.)

Dr. Ulmann, while relating a case of strangulated hernia, which did well, although the operation was long delayed, and it became necessary to tap the intestine to discharge the air before it could be returned, takes the occasion to state the great benefit he has derived in his practice from the use of enemata of acetate of lead, which often soon rendered the taxis successful, though this had been already used in vain. It induces a contraction of the canal, which is propagated to the imprisoned part. The intestine should be first cleared out by a common enema, and not more than three or four ounces of the lead injection should be thrown up, as its utility entirely depends upon its being retained.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D. (Lond.)
Physician to the Metropolitan Free Hospital, late Physician-Accoucheur to the Western General Dispensary.

I. Menstruation.

1. *On the Period of Puberty in the Negro Race.* By Robert Clarke, Esq. (Journal of the Statistical Society, March, 1856.)
2. *Case of Early Menstruation.* By J. O. Bronson, M.D. (American Medical Monthly, September, 1855.)

1. Mr. Robert Clarke says, "With respect to the period of puberty in the Negroes, all my inquiries have tended to show that it commences about the age of ten or twelve years. Girls who have arrived at this age, and much beyond it, may be occasionally observed walking the streets (of Sierra Leone) naked, with the exception of a long strip of white calico, which hangs before and behind below the knee, from a circlet or zone of beads which surrounds the loins, and which scarcely covers the genital organs. These streamers are the signals of the girl's being marriageable. During the presence of the catamenia they are deemed unclean, when a coloured strip of calico is substituted, to intimate the presence of the secretion."

2. We cite the facts observed in the case of Dr. Bronson, reported as one of early menstruation. The subject is named Phœbe Anne Baker, born the 19th of January, 1851, in Sing Sing, Westchester County, N. Y. At the age of ten months her menses appeared, accompanied by the usual signs and developments,

and have continued with healthy regularity ever since. The girl is large for her age, with light brown hair and complexion, and blue eyes. Her form is mature. Her mammæ are prominent, the size of an orange; pelvis wide; and her pubis covered with hair. In fact, she is a woman in physical, and a child in her mental developments. She is quite unconscious of her condition. The catamenial discharge is healthy in colour, character, and quantity, and not accompanied with pain. Nothing concerning the parents, or otherwise, was elicited, tending to throw light upon the causes of this early establishment of the female function. This case, says Dr. Bronson, cannot be classed with those of disease simulating menstruation, but is a *bonâ fide* case of infantile puberty.

II. GESTATION AND LABOUR.

1. *On the Depth at which the Placenta is implanted in the Uterus; and on the Stage of Extension of the Placenta.* By V. RITGEN. (Monatsschr. für Geburtsk., October, 1855.)
2. *On Parturition in the Negro; and Obstetricy in Sierra Leone.* By ROBERT CLARKE, Esq. (Journal of the Statistical Society, March, 1856.)
3. *On the Use of Chloroform in Midwifery.* By Dr. KRIEGER. (Verhandl. d. Ges. für Geb., 1855.)
4. *On his Method of Protecting the Perineum (in Labour).* By V. RITGEN. (Monatsschr. für Geburtsk., November, 1855.)
5. *A Case of Spontaneous Version of the Child.* By Dr. BENDA. (Verhandl. d. Ges. für Geb., 1855.)
6. *A Case of Extra Uterine (Tubal) Gestation.* By SAMUEL L. KURTZ, M.D., of Phenixville, Pa. (Amer. Journ. of Med. Science, October, 1855.)
7. *A Case of Amniotic Dropsy terminating fatally.* By GEORGE AMERMAN, M.D. (American Medical Monthly, September, 1855.)
8. *A Case of previous Separation and Expulsion of the Placenta.* By Dr. E. V. SIEBOLD. (Monatsschr. für Geb., Oct. 1855.)
9. *The Statistics of Placenta Prævia.* By JAMES D. TRASK, M.D. (Transactions of the Amer. Med. Assoc., 1855.)
10. *A New Principle and Method of Treating Placenta Prævia.* By ROBERT BARNES, M.D. (Lancet, and Med. Times and Gaz., January, 1856.)
11. *My Method of Treating the Placenta.* By Dr. COHEN. (Monatssch r. Geburtsk., April and May, 1855.)
12. *On Dr. Cohen's Paper.* By Dr. CREDE and PROFESSOR HOHL. (Same Journal, 1855.)

1. The memoir of Von Ritgen is an elaborate and interesting illustration of the various seats of attachment of the placenta, other than to the neck of the womb. He refers to the method discovered in recent times, of determining after delivery the height at which the placenta was attached, by measuring the distance of the rent in the membranes made by the passage of the liquor amnii and fœtus from the margin of the placenta.

The bag burst at the edge of the placenta in 22 cases. It burst at one inch from the edge in 8 cases; between one and two inches in 12 cases; two inches in 7 cases; between two and three inches in 16 cases; three inches in 5 cases; between three and four inches in 4 cases; four inches in 6 cases; between four and five inches in 8 cases; five inches in 3 cases; six inches in 6 cases; and eight inches in 3 cases.

It follows, that since the distance of the edge of the placenta from the rent is absolutely decisive as to the distance of the edge of the placenta from the os uteri, that the edge of the placenta rested on the os uteri in 22 cases, and was within one inch in 32 cases, within two inches in 49 cases, and so on.

This proves that the placenta has commonly a much lower seat than has hitherto been believed.

It also appears that smallness of the ovum has a closer relation to lower seat of the placenta than is to be accounted for by the simple diminution of all the dimensions of the uterus.

The Period of Exclusion of the Placenta.—V. Ritgen says, that instructions were given in the hospital for many years, not to remove a detached placenta without the express permission of the director. The reason was, to ascertain whether the leaving behind the detached placenta would cause mischief to the mother by absorption of the dead matter. This rule was followed for a time, so far as to allow the placenta to remain several days, and until the foul smell became insupportable; but at a later period it was not carried to this extreme, after it was ascertained that *no absorption of decomposing constituents of placenta ever took place, except in cases of fleshy growth of the placenta to the uterus.*

Summarily expressed, the detached placenta remained fifty-two times, or in about one-half the cases, less than four hours in the uterus; and in the other half, between four and fourteen and a half hours.

The spontaneously completely detached placenta was removed artificially in 3 cases on account of hæmorrhage. In 1 case it was removed on account of spasmodic pains. In 2 cases after operations. In all the rest, the placenta was removed on account of severe after-pains, heavy pressure of the vagina, difficulty of micturition, disturbance of rest and sleep.

[We cannot but express the hope that the Professor is satisfied with these results, and that he will not consider it necessary to carry this experiment further. —Rep.]

2. Parturition in the negro has been generally represented as an easy process, and soon accomplished; but Mr. Clarke's observation is quite opposed to this opinion, for the negro woman suffers as much during child-birth as the female of civilized countries, and unfortunate cases have happened where the woman has died undelivered. Instrumental assistance is as often required; and some of the worst cases of laceration of the perineum, recto-vesical fistula, neglected prolapsus uteri, and even laceration of the soft parts to such an extent as to lay the vagina, rectum, and neck of the bladder into one common cloaca, were brought to hospital for medical treatment. Among the natives the practice of midwifery is confined to aged women. The patient is generally placed on a mat on the floor, close to the fire, with a woman behind to support her, in a semi-recumbent posture. The external parts being freely lubricated with oil, the midwife seats herself before the patient, and during a pain encourages her to bear down strongly, at the same time compressing the back by pulling together with all her force the ends of a shawl previously wrapped round the loins. She also from time to time rubs the abdomen with her hand, smeared over with "doneh-grease," or shea-butter; and if the delivery is slow and lingering, she causes the woman to get up and walk about, or bathes the belly with a foment of country leaves and herbs. They never think of supporting the perineum; but some of them have been known to snip it to facilitate delivery. To hasten the expulsion of the placenta, she is directed to retain her breath and to blow strongly into her hand. If this does not succeed, they bandage the abdomen tightly, make her stand upright, and shake her well; and sometimes they lave her abdomen with cold water, to constringe, as they believe, the womb, and to cause the placenta to separate. Mr. Clarke has known the woman placed upon her hands and knees, the cord being fastened by a string to one of the toes; and while in this position the nose was irritated with a feather, and the fauces tickled in no very gentle way with the handle of a spoon or fork, to excite vomiting, and thereby bring on uterine contraction.

3. Dr. Krieger's paper adds a number of valuable facts to our knowledge of the use of chloroform in labour. In some introductory remarks he declares himself an advocate for the induction of anæsthesia, not limiting himself to operative midwifery. He says that this practice has made but little way amongst the obstetric faculty of Berlin.

Since the 13th of December, 1847, when he first administered chloroform, he has conducted 235 labours; and it is important to remark that these did not occur in hospitals, where the mortality attending childbirth, especially in Berlin, is greatly raised by circumstances connected with this condition, but in the ordinary course of practice. He gave chloroform more particularly in 96 cases: in 23 of these the forceps were used, in 1 case of which after perforation of the head, in 1 for eclampsia, and in 3 after turning by the feet; in 10 cases turning was performed; in 5, prolapsus of the cord; in 2, adherent placenta. In 43 cases no manual or instrumental aid was called for. Of these latter the last cases were very tedious and the patients much exhausted; in 4 others the pains were spasmodic, painful, and without influence in promoting labour; 2 others were so restless as to impede labour; in the 17 remaining the chloroform was partly given to satisfy the demands of the patients.

The mode of use was directed by the end in view. In the case of an operation, about a drachm was poured at once upon the cloth, so as to produce full narcotism as quickly as possible; if it were only wanted to assuage pain, ten or fifteen drops were used at the onset of each pain, and the cloth was withdrawn on the cessation of each pain. In the last manner, Dr. Krieger says he has gone on for three or more hours without any bad consequence for mother or child. We had prepared a short abstract of Dr. Krieger's cases, which would constitute a valuable record, but are compelled to refer the reader to the original.

Out of the 96 cases of anæsthesia by chloroform, death of the mother happened five times within the puerperal period: once from rupture of the uterus, once from epistaxis, twice from peritonitis, once from lung-paralysis, probably caused by metro-phlebitis.

[The frequency of metro-peritonitis in these cases deserves to be borne in mind. Also the occurrence of hæmorrhage.—Rep.]

Dr. Krieger concludes with a somewhat *naïve* remark upon laceration of the perineum. It has, he says, been urged as one of the advantages of chloroform, that it serves to secure the perineum from injury. Dr. Krieger believes in this, and thinks he has saved several perineums by its means. But he cannot but wonder that, in revising his notes, he finds the great number of *sixteen injuries to the perineum* out of 96 cases of chloroform-labours: 2 of these happened in cases without artificial aid, and 14 forceps-labours. [The Reporter thinks it worth calling to mind, that out of 27 cases in which chloroform was used by Dr. Sachs, in the Berlin Lying-in Hospital, no less than four cases of rupture of the perineum also occurred.]*

4. Dr. von Ritgen refers, in a memoir of great length and detail, to a method proposed by him in 1836 for the security of the perineum during labour. He states that the proceeding he recommended has been misunderstood by some, and is generally but little known; and he also adduces his subsequent experience to prove its utility. He defends his method against those who have represented it as consisting in the making incisions in the margin of the ostium vaginæ, so as to widen the orifice. He again explains in what his method does consist. It never entered into his mind to split the labia pudendorum. He leaves the perineum quite untouched, and *makes small scarifications in different spots*, from the labia majora to the upper edge of the constrictor vaginæ. His object is to effect the dilatation of the ostium vaginæ by *several superficial* incisions. The best instrument for these scarifications is a bistoury with a test-blade of four inches long, slightly curved, forming on both sides a somewhat rounded ridge. The free end of the blade is blunt for half an inch, without a knob, but the point rounded off. Then comes the cutting edge, an inch long, on the concave side of the blade. The rest of the blade has no cutting edge.

The scarifications are performed most safely and easily when the patient lies on her left side, with a round pillow between her knees. They should be made during the pains. The following is the seat and nature of the scarifications, and the mode of performing them:—The head must be at the orifice of the vagina; the head is to be held back by the left hand; the knife is then passed flat between the head and the outer border of the orifice of the vagina, during an interval between pains; the cutting edge is turned outwards during a pain, so as to make a small transverse nick in the border of the orifice of the vagina. Before the knife is withdrawn, as many more nicks are made as are necessary on one side. The range of the scarifications is between one inch from the transverse ligaments behind, and one inch from the clitoris in front. Dr. von Ritgen has found that each scarification, being only a line deep, yields, by the dilatation of the orifice, a stretching to the extent of from two to four lines; so that fourteen such scarifications would be an aggregate addition to the circumference of the orifice of about two inches.

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* Lancet, 1850: Anæsthesia in Natural Parturition, with an Analysis of twenty-seven Cases where Chloroform was administered by Dr. Sachs. By Robert Barnes, M.D., &c.

As to the results of experience.—Dr. von Ritgen cites the journals of Dr. Theodor Faustmann, incorporated in the eight 'Inaugural Theses' of that gentleman, published in Giessen in 1851. From his tables it appears that, in the lying-in institution under Dr. von Ritgen's direction, up to the end of 1850, laceration of the perineum had occurred 190 times in 4875 labours. The length of the rent was 53 times a quarter of an inch, 41 times half an inch, 15 times three quarters of an inch, 53 times an inch and a half, and once the perineum was torn to the wall of the rectum, without dividing it; the rent, however, spread on either side of the rectum. Thus, out of 4875 labours, 54 serious lacerations took place. Of these 54 cases, 45 happened before the introduction of the scarification of the vagina in 1828, and 9 since. Since this time these 9 cases happened in 3464 labours, and scarifications were employed in 266. If from these 9 cases we subtract 3 in which the rent began in the middle of the perineum and then spread towards the anus and transverse ligaments, there remain 6 severe cases of the ordinary kind out of 3464 labours.

Since 1851 to the present time, 757 labours have occurred, and the scarifications have been made 83 times. *In this time not a single laceration of the perineum, even of the slightest kind, has happened.*

5. Dr. Benda's case of spontaneous version is interesting. A woman was found with an arm-presentation, the waters having escaped. The right arm, as far as the half of the humerus, was outside the vagina, little swollen. Dr. Benda diagnosed on careful examination the second shoulder-presentation. In spite of attempts by himself and his colleague, Dr. Lehfeldt, it was impossible to pass the hand into the uterus to seize the foot. While waiting for chloroform, the following process, which took place very rapidly, was minutely observed. The hitherto relaxed perineum was suddenly distended, and the presenting right arm was drawn back into the genital organs; at the same time that the pelvic end of the child rose, the right side of the abdomen came first against the perineum, then the pubic end, and during a half-revolution upon the long axis the back was directed against the symphysis, the left hip was evolved over the perineum, whereupon quickly and in one pain, the legs folded upon the abdomen, and the head bent upon the breast followed. Thus, out of the second shoulder-presentation, and by strong uterine contractions alone, working in a capacious pelvis, the first breech-presentation had been developed; a half-turn upon the transverse axis taking place, as well as a half-turn upon the long axis. The child, at first asphyxiated, recovered perfectly.

[This case is a proof that the account given of spontaneous turning by Denman is in some cases correct, and that the mode described by Douglas is not that universally followed.—Rep.]

6. The following is an abstract of Dr. Kurtz's case. Dr. Kurtz was summoned in July last to see a young woman who was suffering violent pain, and was thought

to have cramp of the stomach and bowels. She was married last New Year's-day. More than three months since menstruation ceased, appearing again on the 2nd of July. The discharge, on this occasion profuse, lasted but twenty-four hours. About two months since she began to have irregular pains in the right iliac and pelvic regions, augmented by exertion or coughing. On the 12th of July she was suddenly seized with pain in the side. When seen by Dr. Kurtz she was much prostrated, deathly pale, skin cold, a clammy sweat, pulse at times imperceptible, weak and fluttering, 120 to 130 in the minute; breathing hurried; nausea, had vomited once. The pain, which extended from the epigastric to the right iliac region, at first was intermittent, and like labour-pains, but was now continuous, and much increased by pressure. Abdomen full, somewhat tympanitic. Uterus in proper place; os, size of a shilling; no discharge. Opiates and enemata given. She died at four A.M. next day.

Autopsy.—Abdomen filled with blood and bloody serum; clots filling the interstices of the viscera; about three pints were removed. The fœtus, with membranes perfect, seen floating immediately above the uterus. This pushed aside, a large tumour presented itself in the right pelvic region, which proved to be the enlarged Fallopian tube, which had contained the fœtus up to the time of its bursting. The walls of the tumour were very vascular, on one side thick and strong, on the other thin. The ovarium of the right side enlarged; the tubes apparently elongated. The obstruction occurred in the external half of the tube. The fœtus was well-formed, eight inches long when extended. The period of gestation was estimated at from three to four months. The womb, which was larger than the non-gravid womb, was apparently healthy, and 1¼ in. in thickness. The canal of the cervix was filled with a ropy fluid, and by pressure on the womb it discharged a claret-coloured mucus.

7. Dr. Amerman's case of amniotic dropsy is a rare example of death from this complication of gestation. Jane Lewis, aged twenty, admitted into Bellevue Hospital, under Dr. Barber, on the 29th June, 1855, in her first pregnancy. She could not tell how near she was to her confinement, nor the date of her last menstruation. Her legs were considerably swollen. About the middle of July, she complained of very poor appetite, and felt very weak. On examination the legs were found very œdematous, pulse small and weak, respiration much interfered with. Bowels costive, urine scanty. Liver, kidneys, lungs, and heart, apparently healthy. On the 26th July, an attack of syncope came on. Abdomen, now enormously distended, hard, tense, and painful. Occasional vomiting. She had not felt child for four weeks. On the 6th August, she could not keep the horizontal posture. Dyspnœa very great; countenance very anxious; restless. Dr. Barber having determined to puncture the membranes, perforations were made. In her endeavour to walk from the bed to the chair, the patient was seized with so great dyspnœa, that she became almost delirious. Soon after she fainted, and died, evidently suffocated.

Autopsy.—Thorax: all organs healthy. Aorta unusually small; not being over an inch in diameter. The uterus occupied the *entire cavity* of abdomen, pushing up the diaphragm. The intestines were pushed far up to either side, filling the smallest possible space. All the organs healthy. The uterus and its entire contents weighed 22¼ lbs.: the fœtus weighed 8¼ lbs.; the uterus and placenta 4¼ lbs.; the amniotic fluid 9¾ lbs., or very nearly *five quarts.* Slight dropsy of the cord. No evidences of inflammation in any of the parts.

[It is to be regretted that the condition of the placenta was not minutely examined.—REP.]

8. Dr. Von Siebold relates a case of spontaneous expulsion of the placenta before the child. A woman, aged twenty-eight, six months pregnant, felt pains on the 6th of July, 1853. Hæmorrhage occurred with labour-pains on the 8th. When seen by Dr. Varenhorst, at seven P.M., the pains were strong, but the bleed-

ing was less. The os uteri was the size of a dollar; the placenta was presenting. Forty-five minutes after this, the placenta was expelled; and immediately afterwards the child appeared, in foot presentation, and was delivered. The child gave no sign of life. There is no mention of hæmorrhage after the expulsion of the placenta.

9. Dr. Trask has collected all the published cases of placenta prævia which he could find in the leading medical journals, and in the pages of standard authors; adding some cases not published. This collection is both more numerous and better classified than any other yet published. He especially avoids the error of Dr. Simpson, pointed out by the Reporter, of fusing the cases of spontaneous and of artificial detachment of the placenta. The cases are arranged under three heads. Table I. consists of cases subjected to the various ordinary modes of treatment, embracing recoveries and deaths, and a few cases that died undelivered. Table II. embraces cases of spontaneous expulsion of the placenta prior to the birth of the child. Table III. includes cases in which the placenta was artificially detached before the birth of the child.

[The Reporter would observe, that the analysis of the individual cases given in the tables is so full and well-arranged, as to exhibit the leading particulars of a vast number of cases in a form very compact and easy for reference and study.]

Table I. embraces 251 cases. Of these, 200 were cases of turning; 141 recovered; 59 died, or 1 in $3\frac{4}{10}$ths. There were 50 cases of spontaneous delivery; 43 recovered; 7 died, or 1 in $7\frac{1}{7}$th.

Among the *recoveries* after *spontaneous expulsion* of the child, there are 20 cases of *partial* presentation of the placenta, and 10 cases of *complete* presentation. Of the *fatal* cases after *artificial delivery*, there were 12 cases *partial*, 45 *complete*.

Among the *recoveries of the mother*, in which the fate of the child is noted, in 46 cases the child was *living*, and in 61 it was *dead*. Among the *deaths* of the mother, in 10 the child was *living*, and in 23 *dead*.

Table II. contains 36 cases. Among these cases of spontaneous expulsion of the placenta, in 29 in which the result is mentioned, there are but 2 *deaths*, one eight days, one twelve days after delivery, both from diarrhœa. We are struck at once by the fact that in these cases the womb acted with much more vigour than in cases of this accident in general. In 9 the pains are spoken of as *strong*; in 5 others the pains are expressly spoken of; and in most others it is evident that active labour existed.

Of the 36 cases, 16 were delivered by spontaneous expulsion, 1 apparently in the same manner, 3 assisted by traction on foot, 9 mode not stated, 7 by turning. Of these last 7, 3 were *arm*-presentations. After the separation and expulsion of the placenta, hæmorrhage for the most part ceased. Of 22 cases in which the degree of subsequent bleeding is noted, it ceased in 14, in 4 it continued very slight.

Table III. contains 66 cases, all that have been published, in which the placenta was separated by the hand. This table gives 47 *recoveries* and 13 *deaths*, or 1 death in $4\frac{6}{10}$ths as the gross mortality in artificial separation; whilst in spontaneous separation it is only about 1 in 14, a comparison that clearly demonstrates how unsafe it is to deduce the rule of total artificial detachment from the results of spontaneous detachment. Dr. Trask says, the gross mortality after artificial separation is therefore somewhat less than the general mortality under ordinary modes of treatment, and especially less than after turning; but it is very much greater than after spontaneous expulsion of the placenta.

Of the cases in this table, it is noted that in 35 cases the presentation was complete. This gives a considerably larger proportion of complete presentations among those in which artificial separation was resorted to, than in those included in Table I. The child was delivered in 22 by natural powers, in 3 by craniotomy, in 1 by forceps, in 2 extracted, in 33 by turning, in 1 by vectis, 1 undelivered, in 3 not stated: that is, 1 in 3 was delivered by spontaneous expulsion of the child. It is a remarkable fact, not adverted to by the author, that 33 cases, or exactly one-half, required *turning* in addition to the artificial detachment of the placenta, although the avoidance of this operation is a main argument advanced in favour of the practice.

As to hæmorrhage after detachment of the placenta.—In 35 it ceased *immediately* and *entirely;* in 1 no further hæmorrhage is spoken of; in 1 none for several hours, then slight; in 2 ceased "almost instantly;" in 4 ceased "entirely;" in 1 case not a teacupful lost afterwards; in 1 not over two ounces lost; in 3 it continued slight; in 1 continued at intervals; in 1 it was "not increased;" in 1 "no further danger;" in 1 it abated, but ceased only after cold water; in 1 it continued a good deal; in 7 immediate delivery followed; in 4 cases, not stated.

Conditions for which detachment of the placenta was resorted to.—In 31 cases there was *extreme* exhaustion. Of these, 23 recovered, 8 died. Of the 8 fatal cases, 1 died in half an hour, 1 in a short time, 1 in a few hours, 1 in twenty-six hours, 1 on the eighth day, 1 in one week from fever, in 2 the period of death is not stated. In 11 cases there was *rigidity of the os uteri,* in 9 the patient recovered.

Disposition of the placenta.—In 36 cases the placenta was simply detached, in 30 it was withdrawn at once. Of the 35 cases in which hæmorrhage ceased at once and entirely, it was separated only in 20 cases, and separated and withdrawn in 15 cases. Hence it would appear that the mere separation of the placenta is sufficient to arrest the hæmorrhage.

Mortality of children after artificial detachment.—Fifteen children are reported saved, 32 as lost, in 16 the result is not stated, in 2 it was not viable, 1 was undelivered. We should probably not err in adding to the number lost the 16 in which the result is not stated. This would give 48 deaths out of 63, or 15 saved out of 63, a mortality of about 75 per cent. Now, Dr. Trask states the mortality of children after ordinary modes of delivery to be nearly 25 per cent, or 75 per cent. saved, against 25 per cent. lost. And if we subtract from the number said to be saved after artificial detachment, the 2 cases to be referred to presently, in which it is more than doubtful whether the entire placenta was detached, we have only 13 saved, instead of 15, out of 63, as calculated by Dr. Trask, giving a mortality of 80 per cent. of the children when the placenta is wholly detached before the delivery of the child. This success is, however, greater than was anticipated, and one which instigates an inquiry into the conditions under which children may be born alive after the detachment of the placenta. It is found that among the children saved, delivery took place in 6 immediately, in 3 apparently immediately, in 1 immediately in part, in 1 after dilating the os and turning, in 1 in less than 10 minutes, in 1 in half an hour, in 1 in 5 hours, in 1 not stated. That a child should live five hours after the total separation of the placenta seems improbable, and it turns out that this case, taken from Perfect, is one in which it is not proved that the separation was complete.

The case in which the child was born alive after a lapse of half an hour rests on the authority of Dr. Bland, and is recorded in 'The Missouri Medical Journal,' 1847. On analysis, however, it appears that in this case also the placenta was *not withdrawn,* and that in all probability the separation was not complete. It results, that we have no case recorded in which the child was saved unless it was delivered immediately, or in less than ten minutes, after the detachment of the placenta.

10, 11, 12. The subject discussed in the several memoirs on placenta prævia relates to a point in the physiology and pathology of placenta prævia not until recently noticed by obstetric authors. As the papers in which the abstract of Dr. Barnes appeared are accessible to our readers, we must refer them to those journals. Dr. Barnes' views and pathological applications have recently been the subject of warm controversy in Germany.

Dr. Crede's and Professor Hohl's papers are merely controversial and critical.

III. Puerperal State and Lactation.

1. *On the Contagiousness of Puerperal Fever.* By Dr. Credé. (Verhandl. der Ges. für Geb., 1855.)
2. *Secretion of Milk excited by Nursing in a Grandmother.* By J. Boring, M.D. (Atlanta Medical Journal, Oct. 1855.)

1. The report of Dr. Credé on Puerperal Fever is confirmatory of the conclusions arrived at in Vienna, as to the contagiousness of that disease. He relates that for nearly two years puerperal fever had raged with but little intermission in the Charité Hospital in Berlin. He refers to a statistical account by Dr. Quincke, to show that of about 650 women delivered there in the last year, 139 had been removed for illness to the inner station; all of these, with the exception of 15, were affected by puerperal fever, and 68 died. All the apartments used for the labour patients were twice changed, and once every utensil and all the attendants were changed. All had little or no influence. In the new rooms, as in the old, puerperal fever continued. Upon this the physicians of the outer station made the observation that the contagion of hospital-gangrene and of pyæmia, which also had not ceased within that time, was in close relationship with the puerperal fever contagion. It was therefore weighed by the committee whether it would not be desirable to remove the lying-in institution altogether from the Charité. Dr. Credé added, that it appeared manifest that wherever hospitals were connected with lying-in wards, puerperal fever contagion assumed far greater development and intensity, as in Vienna, Prague, Stuttgard.

2. Dr. Boring's case is of physiological and medico-legal interest. Mrs. J., aged about forty-seven, was married at twenty-three; had borne three children, and suffered one abortion. She nursed her last child thirteen years since, and until recently has not secreted a vestige of milk. In April last, her married daughter died a few days after childbirth, leaving the child to her mother (Mrs. J.), who to soothe it at night applied it to the breast. An abundant secretion of milk was the consequence.

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APPENDIX.

REPORT ON THE FIRST EIGHTEEN MONTHS OF THE FOURTH YELLOW FEVER EPIDEMIC OF BRITISH GUIANA.

BY DANIEL BLAIR, M.D.,

SURGEON-GENERAL OF BRITISH GUIANA.*

CHAPTER I.

In the third edition of the 'Account of the last Yellow Fever Epidemic of British Guiana,' reference is made to the outbreak of a new epidemic which threatened to resemble that of 1837 in direction and severity. The prediction has been verified; and the pestilence continues to afflict the susceptible portion of the community till the time of the present writing. Until the new epidemic has passed away, and the cycle of ascertainable facts has been completed, it would be premature to apply the *numerical method* to the investigation of its phenomena; but as there are certain advantages to be derived from a detail of individual cases, the elements from which the grand averages are obtained, the writer has felt that his time (during a few months' leave of absence from his professional and official duties) would not be misapplied in selecting from the hospital records a moderate number of cases of those who had been admitted for treatment during the existing epidemic, and arranging them for publication, as a sequel of the monograph above referred to.

The reports of cases in the public hospital of Demerara and Essequibo are made out by the resident surgeons at the bedside of the patients, and under the immediate superintendence of the writer. He also keeps private memoranda where the hospital reports seem to him not sufficiently ample, or the observations recorded admit of doubt as to their accuracy; and also of important facts observed by him in private practice. These memoranda will be used in the form of preparatory notes, where applicable, at the commencement of the cases, and in the construction of a general report on the epidemic disease now under consideration. The resident surgeons who were engaged in reporting the cases were, Drs. Driessan, Levin, Butt, and Goring; and a few very interesting cases are reported by Dr. Fowler, formerly of this hospital.

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* When this report was commenced, it was intended as an introduction to a volume of cases illustrative of the fourth epidemic of the colony, and selected from the records of the public hospitals. Two hundred and sixty-nine cases were copied out; but the plan of publication having been altered, the cases are omitted for the present, and the report somewhat abridged, that it may not exceed the limits suitable for the pages of a periodical.

The *microscopes* used were, one of Pritchard's for ordinary work, and one of Ross's for minute observation. The *test paper* was Griffin's neutral tint, of the *old stock.* Each patient was furnished with three vessels, and the ejections were kept scrupulously apart till after examination. The double test for albumen was always applied, and sources of fallacy sedulously attended to.

The arrangement of the cases will be into *aborted, recovered,* and *fatal,* and cases occurring among the *dark races:* each division including incidentally relapse cases, and cases of second or third attack. The number of cases given in each division will bear no relation to the actual number of such cases which are to be found in the hospital-books—that would be a quantitative analysis, which must be reserved till the close of the epidemic. All the complete fatal cases will be given—that is, all those in which there has been a post-mortem examination. Almost all the cases in which recovery has followed black vomit, will also be given. Cases of special interest, no matter in which division, also will appear. The *aborted* cases, being short, might admit, with convenience, of a lengthened list; but there is so much uniformity in the march of such cases, that a long array of them would be quite unnecessary for the instruction of the student. Sometimes, however, in this class, the implied definition of them will be found to shift. Thus, cases will occasionally be found that are at once arrested by the larger dose of calomel and quinine, and a purgative; sometimes, auxiliary treatment, and slight *after*-treatment, are required in addition. Very seldom do the symptoms in this class run as far as *albuminous urine,* and never to the stage of *acid elimination,* which would exclude any such case from the category of aborted cases.

Although the hospital cases are appealed to as illustrations of the statements and opinions of the Report, it is to be recollected that the hospital reports were not made by me, but by independent observers; and that though, therefore, they have their peculiar value as such, that perfect appositeness is not to be expected, nor the development of salient points, nor the sufficiency and effectiveness of individual cases, as if the doctrine and testimony came from the same source; still I believe that the evidence will be found apmle and satisfactory.

The Georgetown Hospital, or, as it is known by ordinance, the Public Hospital of Demerara and Essequibo, besides other special branches, is divided into seamen's and colonial departments, and the wards of each are in separate buildings. The patients of the latter department consist chiefly of the immigrant labouring population, of which a large proportion are Portuguese or Madeirans, who are highly susceptible of yellow fever. Most of the cases are admitted in a late stage of the disease, owing, in part, to the insidious nature of the malady, to its being mistaken for harmless intermittent fever, as well as from apathy and indifference of the patients and their friends. The seamen have a superstitious dread of hospitals, which is, of course, intensified during the ravages of an epidemic. But, by a salutary law of the colony, the responsibility of obtaining speedy medical relief for sick seamen is thrown on the masters of vessels, respectively. Hence, although the law is often disregarded, the cases from the shipping are generally admitted in better time than those of the colonial department, their histories are better ascertained, their treatment has a better chance of success; and hence, such cases, chiefly, have been chosen as representative cases in the following collection.

CHAPTER II.

Since the beginning of 1845, the health of the seamen of this port was all that could be desired, and that of the colony, generally, was good. In the months of June, July, August, and September, 1850, mumps became epidemic and epizootic, and was very fatal to cattle. From the 21st of July, 1851, till about the 24th of August following, a malignant influenza swept over the country, and was very fatal to the feeble and dissipated of the Coolie and Portuguese immigrant population. The influenza was, however, almost unfelt by the seamen, and till the end of that year, when yellow fever reappeared in the manner described in the last edition of the 'Account,' the harbour of Georgetown might have ranked among the healthiest in the world; and no disease existed in the colony of which the newly-arrived European or North American need have had the slightest apprehension. The year of the advent of the new epidemic, and the following year (1851 and 1852), were remarkable for the extraordinary average yield of the sugar plantations, being nearly double the average crop. The increase was due almost entirely to the favourableness of the season. From 1849, a great change took place in the distribution of rain over the colony. That year was the acme of the rainy years. Before it, and up till 1851, the rain and dry weather appear gathered up in the meteorological charts in large masses. Since then there had been a less quantity and greater dispersion. During 1851, the rain was so equally distributed over all the months, that no great washing or drying of the country took place. The meteorological characteristic of the weather preceding and accompanying the advent of the new epidemic, therefore, was *the absence of any decided dry or any decided rainy season.* It was favourable to vegetation and agreeable to the feelings; and the minimum temperature of six years occurred in the month of January, 1852 (13th), when the thermometer fell as low as 67·7°. The coincidence of the invasion with the most cool and agreeable time of the year, corresponded in this respect with the epidemic that preceded ours along the windward coast of South America; that of Cayenne having commenced about the end of November, 1850, and that of Surinam about the end of January, 1851.

Although our former epidemic had every appearance of local origin only, that from which the colony now suffers would seem to be the result of some general exciting cause acting consecutively along the south-eastern seaboard of America, which, beginning at the Brazils, passed on to French, then Dutch, then British Guiana—thence to the West India Islands, New Orleans, and, finally, Bermuda. Had the winter not interfered, probably Philadelphia and New York would have been reached. Although, if its diffusion was due to the agency of the trade-winds solely, whose course it followed, the latitude of Bermuda should have been its terminus. The hypothesis of a great epidemic wave, rising in the east, and flowing on westerly, only apparently suffers from a minute inquiry into its course; for, although Demerara was invaded at the end of 1851, while Berbice, which is easterly, or to windward, did not suffer seriously till the end of July, 1852; still, in New Amsterdam, the port and capital of the latter country, two fatal cases occurred as early as February, 1852, and one in May following; and it is to be considered that this town (unlike Georgetown) is situated several miles above the mouth of its river, and may have been caught, so to speak, in the eddy of that great epidemic wave, which so peculiarly affects the sea margins.

Although the present epidemic has been apparently more intense and diffusive than its predecessor, and its origin seems referable to a foreign source, still it affects special localities as before; and the tenements which suffered most on a former occasion, have been again those of its severest visitations. The *focus of intensity*, as indicated by the number of cases and the short period of incubation, is, as was before, the mouth of the river and its east bank. From thence it extended, in unequal radii, to the west bank, the islands in the mouth of the Essequibo river, and the Essequibo coast to leeward; and up the east coast of Demerara to windward, more slowly, and for a shorter distance. About the middle of June, it had reached Plantation Plaisance, about ten miles east of Georgetown; and by August, Mahaica, which seems to have been its eastern boundary, only twenty miles from Georgetown. In the intervening district of Mahaicony, which separates the county of Demerara from the county of Berbice, no case occurred having its origin there. On the 9th of April, a case was admitted to the Colonial Hospital, which had its origin in Camounie Creek, about twenty miles *inland* from Georgetown, and in the air-line of the trade-winds. In November of the same year (1852), the epidemic influence, in an extensive but diluted form, and of only a month's duration, extended to the penal settlement at the junction of the Cayenie and Mayaroonie rivers, about sixty miles inland, and still in the direction of the trades. Prior to this date, a single fatal sporadic case occurred at the penal settlement, of which special notice will be taken hereafter.

The march of the epidemic, its dates and lines of diffusion, would indicate the influence of atmospheric currents on its progress. Outside the boundaries of epidemic influence just defined, there was safety. The danger seemed in some measure proportioned to the nearness of approach to the centre of infection; and several striking instances have occurred of parties descending on a visit from the uplands of the interior, and the uninfected regions of the coast, falling victims to the infection of the town. Within its circumscribed range, the epidemic manifested local predilections; and though some places seemed permanently infected, the lines of infection occasionally shifted, as in the former epidemic; and infected and uninfected localities were temporarily in juxtaposition. Thus the *Marion* of the Clyde sailed from the port in August, after having all hands sick, of whom six died. The *Unicorn*, also from the Clyde, arrived and took her berth, and after lying six weeks, also sailed, without a man sickening. Thus, also, while on Plantation La Penitence and Plantation Houston the mortality of the Portuguese was excessive in January and February, 1852, Plantation Ruimveld, which lies between these two estates, and had as many susceptible subjects on it, scarcely experienced the disease.

Lulls and exacerbations in the general violence and intensity of the epidemic were frequently observed in its course. The first of these lulls occurred in the last half of the month of March, and the first exacerbation in June. By the end of August, another lull, but of short duration. In February and March, 1853, the epidemic power was intense. It moderated again till June, when it was renewed with great virulence. These lulls in the epidemic were as illusive as the lull of symptoms in the fatal progress of the disease; and it was often my painful duty to discourage the hopes that were so eagerly entertained by the authorities and the public, of the entire and speedy disappearance of the epidemic, and to resist, with apparent pertinacity, the repeated proposals for the return of the white troops to the military service of the colony. During the periods of exacerbation, " threatenings" in the hospital became numerous. These were cases which occurred in the persons of patients admitted for other ailments, and who, during these periods, seemed to be in an *explosive* condition. Such cases presented all the appearances of an invasion of the disease. They were almost all aborted by prompt treatment; but there was seldom any record kept of them, unless they resisted one or two doses.

Although the epidemic sprung up at a delightful season of the year, when the general health was excellent, and, perhaps, irrespective of weather, yet in its course it seemed materially influenced by meteorological conditions; and sometimes even diurnal variations were observable in the condition of the whole of the patients in the hospital, which could only be referable to atmospheric causes. A cool, dry, brisk air seemed to have a mitigating effect; while a hot, sultry, close, moist air increased the number of admissions, and aggravated the type of the disease, particularly on its immediately following the other meteorological state.

Although, when the epidemic influence was strong, intermittent fever and its sequelæ disappeared, it sometimes seemed to blend itself with that disease. It impressed itself sometimes on other diseases, and was itself impressible. Although the influenza came suddenly and disappeared suddenly, about the month of August, 1851, it still left some traces of its influence in the manifestations of disease, and might be detected modifying the yellow fever. In Surinam, the two epidemics were contemporaneous; and we find, by the medical report of the chief medical officer of that colony to the governor, that the two formed a completely mixed disease.

A " phlogistic constitution" of the atmosphere was often observed with us, indicated, through the population non-susceptible of yellow fever, by the prevalence of pleuritis, hepatitis, and dysentery. This condition was found to impress itself on the epidemic, and to influence local congestions and determinations in the progress of the disease. During the exacerbation of the epidemic in June, 1853, small-pox became very prevalent, and was suspected in some cases of spontaneous origin. Mixed cases of the two diseases occasionally happened, the small-pox always predominating. Sometimes the yellow fever would engraft itself on the secondary form of small-pox, after the stage of desquamation commenced, and then it had its own sway unmolested. The co-existence of pneumonia and pleuritis with yellow fever, sometimes the one being primary, sometimes the other, was of frequent occurrence, particularly among the

Portuguese immigrants. In the course of the epidemic, several long-standing cases of chronic disease, to the consternation and surprise of the bystanders, terminated suddenly and fatally by black vomit without any precursory fever. But if the epidemic, as a whole, was subject to modifications and fluctuations, the *early* individual symptoms of the disease were similarly affected. Sometimes the full complement of standard symptoms were present, sometimes they were imperfect or deficient, and sometimes displaced. At one time the diagnostic symptom was the supra-orbital headache. This, in the epidemics of Cayenne and Surinam, seems to have been the constant characteristic, accompanied generally by lumbar pain. At other times, the tongue symptoms alone were diagnostic. Sometimes their equivalent was observed in the fauces and uvula. In the Surinam and Cayenne epidemics our tongue symptoms do not seem to have been at all recognised. These variations and shiftings of the symptoms were not irregular or promiscuous, but *periodical;* and they continued steadily for several weeks together. Towards the end of April, 1852, the tongue symptoms were unpronounced. Towards the end of May, they were well marked. In the middle of September, the early diagnostic was the frontal headache. On the 4th of October, the tongue symptoms were again well marked. On the 15th of December, they were absent, and the head symptoms developed. On the 21st of December, the absent symptoms were unusually numerous. On the 8th of March, 1853, the tongue, eye, and lip symptoms were intensely developed. Intense surface heat, early albumen in urine, and early black vomit, were the character of the later symptoms; and smoky, pale urine, with perfect blood-corpuscles, took the place of the straw-coloured or bilious urine, with its sediment of tube-casts and epithelial matter. Notwithstanding this variation of symptoms, they were never so defective as to prevent the formation of a correct estimate of the nature of the disease with which the practitioner had to deal. The variation of symptoms had sometimes a relation to the mode of accession of the disease. In the diarrhœal or choleroid cases, the tongue and head symptoms were seldom so early or developed. Having thus sketched the circumstances and general habits of this new epidemic, I shall proceed to consider its phenomena as an individual disease.

CHAPTER III.

In general, the testimony of the patient is, that he was quite well up to the date of attack, and he can tell with accuracy when it began. He was probably awoke in the night by a severe pain in his forehead and back, with sickness of stomach and vomiting, and a sensation of heat and thirst. Or he may have had a decided chill with the frontal headache, and a sweaty skin. Or he may have had violent vomiting and purging, with cramps of the gastrocnemii muscles. In almost all cases, however, supra-orbital pain and fever are associated. Occasionally, the headache may have been experienced several days before the invasion; and a fatal case occurred, in which the characteristic headache and a feeling of *mal-aise* and oppression of the præcordia existed thirteen days before the overt invasion. Dr. Levin, the resident surgeon, who died on the third day of his illness, complained of sore throat and dysphagia (which continued longer than the sore throat), and muscular pains of the neck and chest, for fourteen days before his seizure. These precursory symptoms were at the time ascribed to cold or influenza, which he was supposed to have contracted by exposure to a thorough draught after having been heated by violent exercise. In the fatal case of Mr. R., at Messrs. Gray, Cunningham and Co., *vomiting* alone was the first symptom complained of. It commenced at night, but he got up next day and attended to his duties in the store, and ate a hearty dinner afterwards. The following night the vomiting returned about the same hour, with fever and intense frontal headache.

The supra-orbital headache is a truly valuable diagnostic symptom in old residents, in the dark races, and those of low susceptibility, and in those cases of diseased complications which would tend to suppress the development of capillary irritation. In the case of W. Munro (Seaman's Hospital, November, 1852), it *was the only obvious symptom* of his attack, the correct diagnosis from which was subsequently demonstrated. As has already been said, it is a most valuable premonitory symptom, giving notice often several days before the actual seizure or overt manifestation of the disease. This headache, and the punctuated tongue, were the signs by which epidemic taint was detected in some cases of intermittents, and which would not yield to ordinary treatment of such cases. This headache is unlike the temporal and general headaches of intermittents, but like it in being often associated with lumbar pain. It is generally relieved speedily by the first or second dose of medicine. But it seems normally to belong only to the formative and febrile stages of the disease, and subsides spontaneously in the middle and late stages. Its etiology is obscure, and the causes may be compound. The pain during the fever is sometimes too intense to be referable alone to any imaginable degree of capillary vascularity of the lining membrane of the frontal sinuses. It is probably aggravated by the hydraulic pressure of the blood in the brain during the fever stage. The pain is sometimes described as in the orbits, more rarely in the upper part of the forehead, and occasionally as extending to the occiput. I have seen cases in which it was instantly relieved by the vomiting of bile. An increase of temperature over the forehead generally accompanies this characteristic headache.

The practitioner having noted the symptoms derivable from the testimony of the yellow fever patient, will observe a *specific capillary irritation* showing itself in the flush of the face, as characteristic as the hectic of phthisis or the fuliginous complexion of typhus. This suffusion generally occupies a zone over the eyes, and about an inch above and below them. The eyes are injected, like those of a person *just awake*, but generally without any lachrymation or photophobia, although the injection may be as intense as in ophthalmia. Sometimes the irritation extends to the palpebra, to one or both, and sometimes only one eye is affected, but that so violently, as if the patient had been *stung* or received a blow on the eye before admission, as in the case of Cash (S. H., 17th May, 1852). The nares also may be found injected, with a coarse vascularity. The lips may be crimson or vermilion coloured ; the tongue scarlet at tip and edges. If the fauces be examined, the roof of the hard palate will sometimes be found covered with a coarse network of capillaries, and this reticular vascularity extends to the uvula. If the practitioner has already been satisfied as to the nature of the case, this examination of the fauces should be omitted, on account of the *vomituritis* generally induced by pressing down the tongue preliminary to the observation. The following entries in my memoranda-book are the first notices, I believe, of this sign: "To-day I was called to see a Portuguese who was ill with yellow fever. The frontal headache and vascularity of eyes were sufficiently marked, but the tongue triflingly so. He was facing the light of the evening sun, when I asked him to show his tongue, and he opened his mouth so wide that I saw down the posterior fauces. The top of the pharyngeal pouch was unusually red and vascular. *Mem.*—This appearance to be looked for in Seaman's Hospital, 6th February, 1852."

"The *throat sign* confirmed by observation at Seaman's Hospital, 9th February, 1852."

"*New sign of yellow fever.*—The examination of the throat to observe vascularity and redness, causes nausea and retching when the tongue is pressed down by the finger or spatula. This unusual sensitiveness of the stomach enhances the value of the sign, 18th February, 1852."

If a careful examination be extended to the chest, a sub-cutaneous rash may sometimes be observed, which occasionally extends to the arms and abdomen. This efflorescence of the skin is the rarest manifestation of capillary irritation in yellow fever.

Such are the *external symptoms* which declare themselves to the eye. They may be all present, or the majority may be absent. Those generally present are the injected eye, and red-edged and tipped tongue. But though I have never required to investigate further for this class of symptoms, on the first or second day of disease, in any case of yellow fever, I could conceive the possibility of an instance of genuine yellow fever presenting none of these appearances; and, in that case, I would examine the scrotum and anus and rectum with a speculum, for a similar sign: and then I would expect no vomiting, and would look to the cæcum in the post-mortem examination for those appearances usually seen in the stomach. For all practical purposes, the tongue-sign is by far the most important, and is the seat of the most interesting daily observations during the whole course of the disease. All these external symptoms may be more or less intense, and their number and degree constitute an important element of prognosis. They are much more distinctly manifested in the European or North American than in the Madeiran, and are at the *minimum* in anæmics from intermittent fever, or where pericarditis is co-existent. The redness of the tongue varies in tint and in arrangement. In some cases it is confined to the tip; in others, the tip and edges are simultaneously affected. I once observed it in the under surface, near the frænum. In other cases, again, the redness on the tip and edges, or on the surface, is in dots or punctuations: sometimes they are few in number and elevated, and occupy the fungiform papillæ. The punctuated redness is a mild indication, and is the form generally in diseases merely tainted with the prevailing epidemic, or in the formative stage, when the frontal headache exists without any overt seizure having yet happened. In such cases, if the disease proceeds, on the second or third day the punctuations will have *fused*, and a continuous crimson appearance will be formed. On the fourth or fifth day, if the disease has been aborted, or subsided after acid elimination, the dottings will be restored, and then speedily disappear. These little speckles are sometimes faint red, and sometimes fiery; but in all cases are distinctly discernible through any fur that may be on the tongue. At the tip and edges, the usual site of morbid redness, the tongue is always clean. When the dots project above the surface, these sparse papillæ are always crimson or scarlet. The general appearance of the tongue, however, is *uniform* redness of the tip, or tip and edges together; and the general shape is inclined to be compressed, or wedge-shaped. If we might describe by differences—the contrast with a yellow fever tongue would be a broad, flat, flaccid, pale tongue, round at the tip, and broad and indented, or thin and diaphanous at the edges—a tongue, as if slightly œdematous, and circulating little red blood in its capillaries. Such a tongue, in a yellow fever patient, would be "clean gain." The next change which appears in the yellow fever tongue refers to the upper flat surface. The fur, which at first is smooth and uniform, seems as if it had been *curdled*, and lies on the surface in innumerable little greyish-white wavy flakes. This appearance is most observable when the tongue has been broad and sordid. In the case books of the hospital it is called *curdled*—it might be called pilose, or villous. But its first name is derived from its close resemblance to milk acted on by acids, and from the idea, once entertained, that it was perhaps induced by the vomiting of the first acid-matters of the stomach. But although it is generally associated with that stage of the disease, it is not in the relation of cause and effect, for this condition of the tongue has frequently been observed previous to the stage of acid elimination. A modification of the curdled tongue is the *tessellated* tongue. In this case the villi are not tufty and flaky, but appear like separate pavement-shaped forms on the surface of the tongue. This is a more advanced stage of the morbid tongue, but the altered appearance is probably due to a greater shortness of the villi from *wear*. The tongue (where there is no inflammatory complication or head affection) is always sufficiently moist during its changes. The next step in the tongue symptom is the peeling of the epithelium. This generally begins at the point, and proceeds to the edges and down the raphé, and may continue till the whole surface is denuded and the papillæ obliterated, and the tongue becomes smooth and dryish, and the colour and appearance of raw beef. This desquamation of the tongue, as in the fatal case of Juan de Nobriga, may extend into the larynx and bronchi, causing complete aphonia and dry sonorous rhonchi under the stethoscope. It is seldom that the basement membrane is eroded or ulcerated; but this sometimes happens. In the late stages the tongue is liable to be incrusted by the buccal hæmorrhage. In one case, ulceration of the tonsils was observed. One death occurred from gangrene of the larynx, and suffocation therefrom. In the "smouldering" form of yellow fever, and when the case is passing into convalescence, and where there has been little peeling of the epithelium, the fur at the base and centre frequently appears as if stained with tobacco juice. This appearance can seldom be traced to any lesion of the tongue itself, but is found to be a blood-stain derived from the gums at their line of junction with the teeth, and manifests the hæmorrhagic tendency in its lowest degree. In some protracted cases of recovery, an aphthous condition of the tongue has been observed. But, in general, the raw denuded tongue appears in convalescence as if brushed over with a thin coat of milk and water, and the epithelium and papillæ are speedily restored.

Though the tongue symptoms were so striking and characteristic in our epidemic, they seem either to have been undeveloped or unnoticed in the epidemics of Cayenne and Surinam. The following is all that the medical report of the Council of Health of Cayenne to the Minister of Naval and Colonial Affairs says on the subject:

> "LANGUE. La langue était blanche, muqueuse elle devenait ràpeuse quand les hemorrhagies buccal se declaraient. Quand l'enduit blanc-jàunâtre qui la recouvrait devenait visqueux collant, c'était de fort mauvais augure; il en était de même lorsqu'elle offrait des colorations diverses et sineuses lui donnant l'aspect irregulier d'une carte de géographie."

In the report of the health-officer of the first class to the Governor of Surinam, the following is a translation of what is said of the tongue in the epidemic of Dutch Guiana:—"The tongue was generally broad, covered with a dark slime: only in a few cases were the edges more than usually red. In general, the tongue was moist and sticky." In this short statement of the symptoms, it will be perceived that the red appearance of the edges of the tongue had been an object of observation, and its rarity was positive, and not due to inattention of the observer. An explanation of this may perhaps be gathered from a subsequent part of the health-officer's report. He says—" A troublesome sequela of those cases that speedily recovered was the abrasion of the skin around the anus, in consequence of severe purgation. One hundred and sixty-three cases were treated for this." The cause of this excoriation round the anus, assigned by the health officer, does not seem satisfactory. It appears to me that the seat of the capillary irritation in the Surinam cases was displaced, and that the rectal end of the alimentary canal would, if examined, have presented some of the appearances which we expect in the tongue, and that the cæcum and colon took the place of the stomach and duodenum, forming the intestinal variety of yellow fever, a few cases of which were seen by us in the present epidemic. Excoriations of the anus and scrotum were rather rare symptoms with us, and seldom observed except in the last stages, or in convalescence. The tongue symptom is one of the highest value. Yellow fever by it has been detected under its strongest disguises. In the case of a Portuguese, who presented himself at the admission-room, and complaining of his side and cough, the co-existence of pleuro-pneumonia with the epidemic disease was instantly diagnosed by his red and partially peeled tongue.

Herpes labialis was rare, but looked on as a favourable sign when it appeared. Like the other individual symptoms, when it appeared at all, many instances of it came together. The vesicles were not so perfect as in those of intermittent fever, or from solar exposure. They contained less fluid, and finally became bloody crusts, but never had the appearance of rupia. In some cases the scarlatinoid rash caused turgescence as well as efflorescence of the skin, as in the fatal case of Antonio Fernandez, which did not subside till after the establishment of black vomit, when it subsided. When patients have recently arrived from a cold climate, and have a fine, delicate, sensitive skin, their legs, arms, chest, and all exposed parts, are frequently covered with rose-coloured spots of a somewhat circular shape, varying from the size of a flea-bite to what might be covered by the point of the finger. Some are flat, some a little elevated, and some have vesications. These are mosquito wounds, and become hæmorrhagic at the end of the disease, if it terminates fatally. There is a different kind of exanthem from those already described, which I saw best marked in two young men *ex*-"Livonia" in private lodgings. It consisted of inflamed patches, also chiefly over legs and arms, but there were many over the body also. There were no vesications. The lodgings were very much infested with mosquitos, but after extensive experience in all the variety of mosquito wounds, I would hesitate to ascribe those appearances to that cause. Both cases were of the highest grade of yellow fever, but they recovered. The *stomach kept quiet*, and the recovery was, perhaps, in a great measure due to the diversion of the congestive tendency from the centre to the circumference, set up by this exanthem.

There is another *external* or *surface symptom* connected with the skin. The face, chest, arms, and legs have sometimes a slight purplish appearance after the second or third day, and sometimes the colour of a boiled lobster. This appearance varies much in degree, but may be detected by pressing the hand flat on the chest, when the fingers will for a short time be delineated in white with purple outline. This symptom occurs chiefly in the "smouldering" form of the disease, and is often so deep as to conceal the jaundiced appearance of the skin. It is quite different in its character and nature from the cuticular efflorescence before alluded to. The one is active, the other passive—the one is inflammatory (specific and peculiar), the other congestive; the one apparently from the direct action of the irritant poison, which induces the disease, yellow fever—the other a secondary or tertiary effect. This *languid capillary circulation*, as it is called in the case-books, is generally seen, as has already been remarked, in the "smouldering" form of the disease, and is looked on with favour. It would seem to indicate that the congestions were selecting the periphery of the body for their pressure; or that the vital internal organs were relieved of a part of their load by the hyperæmia of the skin. In these cases the skin is generally cool and moist, and sudamina occasionally appear. Desquamation of the cuticle of the front of chest, and of the hands and arms, is sometimes observed in convalescence. But this can scarcely be considered as having any relation to the scarlatinoid rash. It occurs in sailors while in hospital, no matter what has been their ailment, and is confined to the sun-burned parts of the body. In the advanced stages of yellow fever, the capillaries of the conjunctiva, where the eye has been markedly affected, become coarse and enlarged, and the red injection has become orange, and there is a gumminess of the eyes and lids. A little splash or spot of ecchymosis is also common below the tunic at either angle of the eye. If the stomach has remained quiet, and the secretion of urine has ceased, the pupil it is likely is contracted, the palpebral apertures are narrowed, the brows are a little corrugated, and the light is unpleasant, and there is something of a titanic physiognomy.

This condition was very marked in the case of the captain of the *Hinda* (private lodgings), and also in that of Mr. G. and the Rev. Mr. L. It is generally associated with nervous symptoms, or restlessness, or irritability, or joviality, and is one of the manifestations of uræmic intoxication in yellow fever.

There is no mystery in the yellow suffusion of the skin and eye in this disease. It has over and over again been demonstrated to be occasioned by the presence of bile. The tint is seldom deep, except when jaundice supervenes on convalescence as a sequela. It appears in the primary disease associated with an active condition of the liver, and a full supply of bile in the alvine evacuations. It is one of the earliest signs of those internal irritations and congestions in which most of the viscera begin to become involved after the disease has been of one, two, or three days' duration, and the blending of the yellow and the red in the capillaries communicates obviously the orange tint to the sclerotica. The yellowness of the eye is soonest observed at the angle formed between the eyelid and eyeball, and the lid should be turned down while the patient is directed to look up, in seeking for its early detection. It is valuable as a signal of the attack on the liver in the procession of morbid actions, and as a criterion of considerable accuracy of the degree of lesion or disturbance of that organ. The observation of this symptom, however, is subject to fallacy. There are some sclerotics naturally tinted. This is particularly so with Coolies and Negroes, and the mixed races, in whom a little of the black pigment is frequently found in the sclerotica, giving it a smoky appearance, and which has been mistaken for the bile tint. In such cases the report of yellowness of eye would be premature. An error of an opposite kind consists in overlooking sometimes this symptom when it is actually present, and until the degree of it is so deep as to be noticeable on the exposed parts of the eye and skin. By examining, however, the line of junction between the lids and eyeball, both sources of fallacy will be avoided, for there the earliest trace is to be found, and it is seldom the site of accidental discoloration. The difference of appearance between the eye in advanced stages of yellow fever and the eye of jaundice, consists in the absence in the latter of vascular injection, and the presence of a flat gamboge colour only. If we had a case of mild ophthalmia occurring in a jaundiced patient, then, no doubt, the resemblance would be complete. When there has been little or no irritation of the eye in the first stage of yellow fever, the yellow suffusion is simply jaundice. Cases have occurred in which a notable quantity of bile was detected in the urine before discoloration could be discovered in the eye. And fatal cases have occurred, though rare, in which, till the very day of death, no yellowness of the white tissues nor biliousness of urine existed—because, as revealed by post mortem examination, the liver had not suffered in those instances.

Among the surface symptoms may be placed *Epistaxis.* But this will be noticed when the blood comes to be considered. It sometimes happens early in the disease, and it is then an active hæmorrhage, caused, probably, by the dynamic power of the circulation during febrile excitement. In the late stages of the disease, however, death has supervened from incontrollable epistaxis, and then, probably, it originates in the same pathological condition as the next surface symptom to be described. Generally, bloody furuncles appear late in the procession of symptoms. Their most common site is on the wrist, over the metacarpal joints of the fingers, along the front of the legs, below the scapula, and over the hip, and in the parotid, and on the forehead and lip. They are generally in close proximity to the smaller arterial branches—viz., the ulnar and radial, anterior tibial, glutæal, intercostal and facial arteries. In the majority of cases, these must rather be considered *sequelæ* than phenomena of the disease proper. But so close are they on the primary affection (as in the fatal cases of Miss N. of the *Belairs*, and Mr. L. M., in whom they were contemporaneous with black vomit, and in the latter case on the third day of illness), and as they are sometimes even the cause of death in the progress of yellow fever, from hæmorrhage and disorganizing infiltrations of blood, to separate them from the train of morbid processes which proceed direct from yellow fever poisoning, would do violence to truth for the sake of system. Sometimes these furuncles are very tender, are acuminated, and inflamed; sometimes they form large abscesses of purulent matter, with a pale or an inflamed surface, and this chiefly when below the scapula, or over the hip. Generally on the legs they are flat, present no inflamed appearance, but show a flat, purplish vesication, about the size of a split pea or a sixpence. If you open one of these vesications, a little watery, curdy sanies will be discharged; and you will believe that that is all, and of no consequence. But if you clip away this vesicle, and wipe the bare cutis, you will perceive in the centre of it a circular perforation, into which a probe easily passes, and which goes down through the true skin and cellular tissue to the surface of the deep fascia or the muscle. And if you now squeeze on each side of the vesication, one or two little dark clots or pellets will start up, and be accompanied or followed by a little purulent matter. There is no base or hardness; there seems to be no cyst of any consequence; and the whole affair will close up and heal, and require no further treatment than the emptying it. Now this is the simplest form of that morbid manifestation. But when it occurs over a joint, or below a strong confined fascia, abscess, with diffuse phlegmonous inflammation—or in a vascular tissue, as the parotid gland, death, from destructive infiltration of blood, gangrene, and hæmorrhage, may follow. The formation of these bloody furuncles is, it is likely, not confined to the external parts of the body. In the case of Ballobitch (Seaman's Hospital), the post-mortem examination disclosed a condition of the kidney which was probably due to this cause. The following instance may be given as an illustration of a case with bloody furuncles, although they appeared in *convalescence* from a graver attack. It is cited, because by it I became enlightened as to what, I believe, is the true nature of these (which, for want of a better name, we call) bloody furuncles.

Peter Daley, of the ship *Alenker*, was attended by me in private lodgings, in January, 1853, and recovered. The following is an extract from my notes:

"*Bloody Furuncles: considerable loss of Blood on* 11*th day of illness of Yellow Fever.*—Jan. 27th. Peter Daley, referred to at pages 29 and 31, has lost about eight ounces of blood from a bloody furuncle on wrist of left hand, and another on the metacarpal joint of little finger of right hand, to-day. I have had to apply compresses to each. That on the wrist began about five days ago like a 'blind boil,' and was tender, for he winced on several occasions when feeling his pulse. The swelling afterwards became distinctly acuminated, and below the cuticle there was lividity, as if some bloody ichor was extravasated there, but not amounting to vesication. At present, having burst two or three days ago, the cuticle seems undermined and separated for about the diameter of three-quarters of an inch, and there is a decided loss of substance, a sinking below it. Out of this occasionally sprouts a small mass, about half the size of a filbert, but elongated, of blood, like the 'bullock's liver' of scurvy; or rather like the clotted blood which escapes when, with a lancet, you divide a recent painful external hæmorrhoid. But when this is removed, florid blood trickles out rapidly, as if an arterial twig had been opened. There seems to be the separation of a small slough this morning, after the poultice was removed (for it and the other had been poulticed till to-day). The furuncle over the finger is also very painful, and the joint swollen. Clots of blood can be squeezed out from below the fascia for the distance of an inch and a half. The edges of the orifice are livid and unhealthy. His urine is still very copious. There is still albumen in it, though not much. He had a tolerably good night last night, though the previous night's rest was bad. The night before that again was good, for I had given him some drops of solution of acetate of morphine (quarter of a grain). If we look on the disease as having passed on the 7th day, these furuncles may be considered as *sequelæ;* but they are sometimes seen in the advanced stages of bad cases. He has many small ones on different parts of his body; but those mentioned only are such as require medical treatment. I prescribed five grains of gallic acid every three hours=four doses. Do these half-active, half-passive hæmorrhages arise from the dissolution of the solids or fluids? I incline to the former.

"Jan. 31st. Peter Daley (page 37) has to-day a large phlegmonous swelling over entire surface of left hip, threatening extensive suppuration. In taking off the bandages used in restraining the bleeding, to-day, the following were the appearances—i.e., over the metacarpal joint of the little finger of right hand, and extending about two inches in long diameter,—the cuticle is elevated and separated from the true skin. When it is removed, an ulcer over the joint, about the size of a sixpence, is discovered, of considerable depth, and showing the plugged mouth of an arterial twig, from which the hæmorrhage must have proceeded. The edges of the ulcer are well-defined and clean, but the base is foul. On the left wrist, over the projection of the ulna, there is a similar ulcer, but cleaner, and with an evident tendency to heal round the edge. This ulcer, like the other, is excavated clean down through the skin and integument. It has no surrounding separation of cuticle, and no plugged vessel that I can see. Indeed, from the furuncle which preceded this ulcer the hæmorrhage was comparatively trifling.

"Feb. 1st. Peter Daley (page 39) was seen again by me about two hours ago. The ulcers over little finger and wrist have almost *completely* filled with healthy granulations since yesterday (twenty-four hours). There was little redness over left glutæi muscles to-day; but feeling distinct fluctuation, I punctured the tumour, and about half a pint of thick altered blood escaped, with a few clots of purulent matter. This sequel of yellow fever is now clear to me. In this case, as, no doubt, in others, the cause of these tumours and furuncles is the *rupture of an arterial twig;* and the presence of the extravasated and decomposed blood sets up a certain amount of irritation or inflammation, which extends to the skin, giving it the flush, and causing the presence of more or less pus in the cavity. These swellings are generally painless, and are discovered, as it were, by accident. I have no doubt that in Peter Daley's hip, the vessel had ruptured two or three days before the ailment was discovered; and if I had made a more careful examination yesterday (when my attention was directed to the hip, from observing the difficulty with which he got up from his bed, and not from any complaint that was made), I should have detected the fluid, and let it escape. When these abscesses (?) are emptied, they heal immediately; they have no inflammatory cysts to be dispersed.

"Feb. 2nd. Still very copious rusty discharge from hip. Vesications on legs drying, but containing blood. Has had to take morphine for restlessness for the last two nights, and, consequently, magnesian mixture to-day.

"Feb. 3rd. To-day, a prominent bloody vesicle has appeared over upper end of right fibula, less than the size of a sixpence. I clipped off the cuticle, and wiped away the altered blood with which it was filled. It appeared, before being cut, to be quite on the surface—cutaneous—with a slight areola; but on examining the part carefully, I observed a circular perforation, which penetrated into and through the cellular tissue, and leading evidently to a cavity. On squeezing this, blood and pus exuded. It seems to me that here, also, an arterial twig has given way, and a little false aneurism had caused the ulceration, suppuration, and vesication.

"Feb. 4th. Peter Daley's abscess (?) over hip has ceased to discharge to-day. Of the little cavity, ulcer, and vesicle over fibula, nothing remains but a blood-stain on the skin. Ulcers of wrist and fingers cicatrizing. He goes on board to-morrow."

Several similar cases, such as those of Major and Anderson, were in the Seaman's Hospital at the same time; and by further observation of them and others subsequently, the conclusions which I had arrived at from Peter Daley's case were confirmed.

But there is another species of abscess which occurs as a sequel of this disease, and which I have never seen in the primary stages; and Peter Daley himself affords an example. In a note of the 30th of January, I have entered thus: "To-day, I detected a slight, livid, painless swelling over his left eyebrow, *which had come since yesterday*, and on opening it, about two drachms of apparently healthy pure pus escaped." Whence came this? was it absorbed from the bandaged ulcer—could it be?

High temperature of the body seems to have persisted longer through the stages of the disease in this epidemic than the past. There is great irregularity in the temperature of the surface. Sometimes the forehead is the hottest part of the body, and occasionally the chest. The uncovered portions of the body in the late stages are easily reduced in temperature; and thus, while the exposed chest and extremities may feel cool to the touch, the axilla may raise the thermometer to 102° or 103°. The highest temperature I have observed in the axilla during the course of the disease was 107°.

We have now considered the chief symptoms ascertainable from the testimony of the patient, and by the observation of the surface of his body. Those depending on an examination of the *secretions, excretions, blood*, and *breath*, will next be considered. And here it may be remarked, that the test-tube and microscope are as necessary for the correct diagnosis and prognosis of yellow fever, as the stethoscope and pleximeter for diseases of the chest.

CHAPTER IV.

At page 93, in the 'Account of the last Yellow Fever Epidemic of British Guiana,' there is a note by Dr. Davy, in which is mentioned the discovery, by Dr. Collings, of albuminosity of urine as a characteristic of yellow fever. This important discovery was duly appreciated in the investigation of the phenomena of the present epidemic. The following remarks will embody the results of observations made on the urine of yellow fever generally, since the 6th of February, 1852. The urine is always *acid* in the first stage, and continues so generally till convalescence, when it becomes alkaline, or until it becomes heavily charged with bile. In the case of Macey (Seaman's Hospital), the urine in the advanced stages was neutral on being passed, and immediately became intensely alkaline. This happened also in the case of Ellwood (Seaman's Hospital). These instances of alkalinity in fresh urine seemed due entirely to the presence of ammonia. Numerous experiments on the specific gravity of the urine were made without any striking general result being elicited. During the early stage of the fever the urine is normal in colour, clearness, and quantity. As the disease proceeds, about the third day, the colour alters, and becomes that of sulphur, or primrose, or straw, or light gamboge, and is perhaps slightly turbid, with a little floating sediment. The colour, during the progress of the disease, deepens, till it becomes yellow or orange; and if the case end in convalescence, the urine is very copious, and may appear, *en masse*, black. As the colour deepens, the sediment becomes more decided, both in quantity and gravity. It is, however, seldom very considerable in quantity, and might escape careless casual observation. But in one case, that of Theodore Ternaban (Seaman's Hospital), it contained a sediment which occupied one-half the urinal. If the case is going to terminate with *suppression*, it generally does so in an abrupt manner. At other times, when the event is to be the same, the urine is expelled of an amber colour and of an oily consistency, and in quantities of a drachm to an ounce, as if with some tenesmus of the bladder. In one case only was diuresis noticed (Barnet, Seaman's Hospital, March, 1853) during the active course of the disease. This was after the use of a tobacco clyster, and was at last followed by fatal suppression. About the month of June, 1853, the physical appearance of the urine was for a few weeks considerably altered. It then had a pale watery or smoky appearance, with a layer of blood corpuscles as a sediment; and in some cases the urine was *very bloody*. In uncomplicated yellow fever the urine is never buff nor red (unless from blood), and a glance of it at the bed-side of the patient has been sufficient to correct at once erroneous impressions as to the nature of the case. Fevers of rheumatic and inflammatory origin have thus been discriminated from the epidemic. A pinkish sediment was observed in a few intermittent fever complications. Malingering is very rare during an epidemic; indeed, among the seamen, the chief difficulty lay in inducing them to enter the hospital early enough. There was one exception, however, and he was detected at once by the urine. Quillan, on the 9th of February, 1853, returned to the hospital after having been discharged cured of an attack of yellow fever. He complained much of nausea, and showed copious vomit in his basin. Pulse was very quick, and he looked ill and prostrated. On looking at his pot, full of pale, non-coagulable urine, his case was seen through forthwith. He was partly malingering and partly suffering from hysterical excitement. On further inquiry, I found that he came from the *Copia*, the master of which we had had fined for neglect of his sick men, and who had the very worst reputation among the seamen for his severity or brutality. The *Copia* was about to sail, and Quillan's plan for leaving the vessel was to sham sickness. The crisis of his fate rendered him nervous, and his urine betrayed him. The copiousness and wateriness of hysterical urine contrasts well with that of yellow fever. During the progress of the disease *retention* sometimes occurs from apathy. The patient does not pass it, either because he thinks he cannot, or he feels no impulse. But in such cases, if he is told to do so authoritatively, it will be done. Retention requiring catheterism occurred in six individuals in both hospitals. In one of these (Swede Anderson, Seaman's Hospital) the retention occurred in convalescence, and required three operations. When *suppression* occurs in the course of the disease, it may be regarded as the most fatal sign. In one case, however (Barkway, Seaman's Hospital), the secretion was restored, and the patient recovered. About the period when the urine changes its colour, and particularly if there be turbidity, if in quantity more than three or four ounces, it will, when recently passed, appear *frothy.* It then contains albumen—for ascertaining which the double test of heat and nitric acid should always be used. It is also well to be apprized that urine was observed about the month of June, 1853, in which albumen, though present, did not answer to the double test till the specimen was put aside, and suffered to *cool.* The cases wherein this happened were chiefly dissipated subjects, but not in all instances. Albumen appears on the second or third day generally; but in a few days it has been found as early as the first day of illness; and in a few cases it did not appear till the day of death, and after black vomit had set in. In several instances on the fourth day, when the tongue was completely denuded, the urine was not yet coagulable. Albumen was seldom seen in aborted cases. In a few of these it appeared during their convalescence—for instances, the following may be cited: Peter Kayle, 19th of January, 1853; Henry Russel, 27th of November, 1852; M'Grigor, 19th of October, 1852; John Smith, 29th of October, 1852; Poole, 7th of December, 1852,—all of Seaman's Hospital. During that period of the epidemic when torpidity of the bowels was observed, and croton oil was occasionally required as an early purge, the urine was later in becoming albuminous. In three cases the albuminosity was *intermittent* for one or two days. These were Asthrup (Seaman's Hospital, April, 1852), Profine Martinez (Colonial Hospital, June, 1852), and John Ferguson (Seaman's Hospital, October, 1852). Albumen appeared in every fatal case of normal duration. It sometimes ceased in convalescence suddenly, always before the yellow suffusion of skin and eye, or bile in the urine, disappeared, except in the single case of Manuel de Nobriga, in whom Bright's disease seemed to be a sequel of yellow fever, and who, after remaining in hospital upwards of two months, left with his urine still albuminous. Between the eleventh and twentieth day of gravior cases, it generally disappeared, and its disappearance formed the criterion for the discharge of the patient from the hospital. The colour of the precipitated albumen was never *white*, as it is in our cases of Bright's disease. It is doubtful if the primrose or sulphur colour is due to bile. In several such specimens the nitric acid failed to bring out the bile tint, although the urine was coagulable (as in the cases of Jones and Collard, Seaman's Hospital, December, 1852). But the gamboge yellow and orange colour were clearly referable to that source, as daily experience with Heffer's test demonstrated. As before mentioned, bile was thus occasionally detected in the urine before the eye or skin was appreciably discoloured. In one case (that of Bevan, Seaman's Hospital, 1853) the urine remained abilious till the period of his death.

The *turbidity* of the urine was not necessarily connected with its albuminosity. The urine may be deeply tinted with bile, and highly albuminous, and yet clear. The turbidity of the urine was caused by the presence of mucous epithelial matter, coagulated albumen, and casts of the urinary tubuli. It is probable that the free acid of the urine has a coagulating power, and sometimes communicates turbidity. The presence of mucus will have a similar effect, but the turbidity then is not general, but occupies a lower stratum of the fluid, and is light and floating there, while the supernatant liquid is clear. There is nothing distinctive or of importance in this mucous condition when the urinary secretion is copious. Perfect epithelial scales are rarely found in the sediment, but broken-up epithelial matter is abundant. In the case of Ternaban, before referred to, the appearance of the sediment to the naked eye was that of pus. The microscope showed this enormous mass to be broken-up epithelial matter. It was all soluble in *liquor potassæ*.

There is a variety of urinary sediment which appears of a yellowish-brown colour, a little darker than the fluid in which it is contained, and exists in small curdy-looking masses. It is only partly soluble in liquor potassæ or nitric acid. Under the microscope it has a fibrillated appearance, and it entangles numerous tube-casts and large organic globules and epithelial scales. This variety is rarely seen, but when it is, it bears the most fatal import. There is another variety of the "curdy sediment," in which this particular material is deficient; but it also entangles tube-casts, and it appears to be composed of amorphous epithelial matter, and when treated with acetic acid, shows large and small organic globules—the latter about the size of mucous corpuscles; and these bodies seem to constitute the mass of this curdy sediment. In all cases, except the two now mentioned, the tube-casts roll separately and detached in the urinary sediment. Probably one of these may be the matter which was seen by Dr. Collings, and was considered by him to be of the nature of *casein*. I was impressed with the opinion that fibrine entered into the composition of the curdy sediment, and also that the same material constituted the basement membrane of the tube-casts.

Although albuminosity is almost always the antecedent to the presence of tube-casts, a case (that of William Narro, November, 1852, Seaman's Hospital) occurred in which they were found in non-albuminous urine. The tube-casts are generally short, thick, and club-shaped, and nearly opaque. Along with them, also, there are frequently amorphous bodies, apparently of the same material. The casts consist of a basement membrane, and are covered with minute pavement epithelial scales, between the interstices of which there is some translucency. At one end they have generally a broken-off appearance, but some are round at both ends. Few are equal in diameter throughout their whole length; some are sacculated, some fusiform. They are sometimes slightly stained with hæmatosine or bile. At the broken end they are frequently destitute of epithelial covering. A long tortuous cast is occasionally seen, but being nearly transparent and without epithelium, may escape observation. I measured three specimens of tube-casts from Chugg and Holmes (Seaman's

Hospital, 4th of December, 1852), and Feliciana de Jesus (in Colonial Hospital, 8th of December, 1852). The measurements were made with Ross's micrometer eye-piece, and show the greatest length and breadth of each cast observed, in parts of an inch. Chugg's were $\frac{1}{210} \times \frac{1}{950}$, $\frac{1}{270} \times \frac{1}{950}$, $\frac{1}{633} \times \frac{1}{950}$, $\frac{1}{270} \times \frac{1}{380}$, $\frac{1}{210} \times \frac{1}{633}$, $\frac{1}{158} \times \frac{1}{475}$, $\frac{1}{126} \times \frac{1}{475}$, $\frac{1}{210} \times \frac{1}{661}$, $\frac{1}{190} \times \frac{1}{1266}$, $\frac{1}{633} \times \frac{1}{950}$, $\frac{1}{633} \times \frac{1}{950}$, $\frac{1}{190} \times \frac{1}{661}$. Holmes's were $\frac{1}{146} \times \frac{1}{950}$, $\frac{1}{950} \times \frac{1}{1266}$, $\frac{1}{380} \times \frac{1}{1266}$, $\frac{1}{316} \times \frac{1}{950}$, $\frac{1}{146} \times \frac{1}{1266}$. Feliciana de Jesus' were $\frac{1}{190} \times \frac{1}{475}$, $\frac{1}{380} \times \frac{1}{475}$, $\frac{1}{158} \times \frac{1}{422}$, $\frac{1}{211} \times \frac{1}{633}$. As tube-casts are so soluble, not only in liquor potassæ, but also in ammonia (though more slowly), it is necessary to look for them before decomposition of the urine takes place. The scanty, acid, amber-coloured urine, of oily consistency, in the last stages of yellow fever, is always highly coagulable, but contains no tube casts, and is loaded with mucous corpuscles.

It is very common, before the urine becomes albuminous, in using the nitric acid test, to perceive considerable effervescence, although the urine be acid, and no carbonate of ammonia can be suspected as being present. It is likely due to the decomposition of uric acid, or urea, by nitric acid, while the urine is heated. Is the effervescence in such cases due to an excess of one or other of these; or is it a normal condition, and are the instances of non-effervescence due to the deficiency of either? Whenever urine has been set up and examined for uric acid, it has always been found. But, except in the case of Profine Martinez, before referred to, I recollect of no instance in which it has been found as a sediment. In Martinez' case, it was on the tenth day of his illness on which it was observed, and the deposit was copious. In the case of Morgan (Seaman's Hospital, September, 1852), his post-mortem urine was examined both for urea and uric acid. The hydrochloric acid test discovered the latter, but no urea could be detected, although, in the single opportunity for experiment which occurred during life, it was found to be copious. The specific gravity of the urine during life was 1·023 at 85°. The sediment was loaded with tube-casts. When heated, the coagulum occupied more than a quarter of the space in the tube. When the albumen was separated by filtration, the urine yielded nitrate of urea so copiously, that it became solid. Yet the urine found in his bladder after death did not yield a trace. On the same day that Morgan's urine was examined for urea, Gilmey's, also, was tested for the same. The coagulum of his urine occupied upwards of a third of the space in the tube, and about one half of the specimen tried for nitrate of urea became solid: it appearing, from these two experiments, as if the quantity of urea present was in inverse ratio to the albumen. In two or three experiments for phosphoric acid, it was found abundant in yellow-fever urine.

The observer of this description of urine cannot but be struck by the rarity of the presence of crystalline bodies in it. After a time he ceases to expect them. There were only six cases in which, during the active course of the disease, triple phosphates were found. There was one case in which a copious sediment of urate of ammonia was present, with the urine still acid. This was in one of the two cases of the "Livonia" before referred to, in which the red patches appeared on the skin. After convalescence, the albumen was generally replaced by the earthy salts and triple phosphates and urate of ammonia, and the tribasic triple phosphates were frequently seen. On the 4th of December, 1852, however, while examining the urine of Chugg, before referred to, I discovered distinct and well defined minute octahedra of oxalate of lime in the sediment, which also contained numerous casts of tubes, epithelium, and apparently coagulated albumen. Associated with these were vibriones and moving monads. About the same time, in W. Bertie's urine, I discovered oxalate of lime crystals and vibriones. The urine on both occasions had stood for twenty-four hours. As yellow-fever urine is such as, *à priori*, oxalate of lime might be expected in, I surmised that it had previously escaped our observation by our neglect of the rules given for detaching it, and by observing the urine too soon after emission. Some of Robert Forsyth's urine was, consequently, put by the same day for examination for this salt of lime. After remaining twenty-four hours, the following were the results of observation: "Coagulable; numerous casts of tubes, most of them perfect, of average size, a little yellowish in colour. With the one-eighth inch object-glass I can detect no animalcules nor crystals. It is still slightly acid." Twenty-four hours afterwards, the phial having, in the mean time, been kept firmly corked, "sediment loaded with well-defined triple phosphates; two quartz-like uric acid crystals under the glass, slightly claret-tinted. The casts have nearly disappeared. The two in view are thin and wasted, but still retaining the yellow tint. The urine is now strongly alkaline." Subsequently to this experiment, I set aside seven different samples of yellow-fever urine for the purpose of examining it for oxalate of lime, under the guidance of Dr. Golding Bird's directions. The first four numbered specimens remained well corked and undisturbed fifty hours, for the deposition of crystals. The last three remained twenty-eight hours. Each specimen was first examined by a half-inch, and subsequently, by a $\frac{1}{8}$th inch object-glass of Ross. 1st. "Alexander Muschard.—Urine still acid; pellicle of fungus on surface; a few tube-casts observed, and much comminuted matter, probably of the same material; numerous sporules; no crystals; no vibriones. 2nd. George Thompson.—Urine still acid; several organic globules; much epithelial matter; a very few casts of tubes, tinged yellow; sediment copious but flocculent, and floating; no crystals, no vibriones. 3rd. Michael Flynn.—Sediment heavy; urine fetid; slightly alkaline; four crystals of triple phosphates in drop under observation; a few small casts of tubes; numerous amorphous pieces, probably of the same material; numerous luminous little spheres which, under a $\frac{1}{8}$th inch glass appear to be oval sporules; some vibriones. 4th. Antonio Ballabatch.—Urine still acid; sediment dense; very numerous tube-casts, faintly yellow; three of them in drop under observation deeply blood-tinted; some blue pieces, possibly extraneous; a few organic globules; minute luminous points, which under high power prove to be vibriones; no crystals. 5th. Michael Flynn (same patient as gave number 3).—Urine fetid; sediment heavy; still acid; numerous casts; amorphous pieces; two large casts, deeply-coloured orange; one amorphous piece, the same; several straight transparent casts without epithelial covering; many comminuted pieces of yellow tint; no crystals; vibriones seen by high power. 6th. Robert Forsyth.—Urine alkaline; copious buff sediment, part of which adheres to sides of phial; no casts; sediment consists of triple phosphates and urate of ammonia; no oxalates. 7th. Adam Smith.—Urine neutral or slightly alkaline, of vinous-urinous odour; sediment half dense; triple phosphates very numerous; casts of tubes becoming thin; no oxalates." Vibriones were seen on several occasions in fresh urine; but all in the most violent cases. For instance, in the case of Bruce (Seaman's Hospital, October, 1852), three hours after the emission of the urine; and in the recovered case of the master of the *Margaret Poynter* (private lodgings), after the occurrence of black vomit, on the fifth day of his illness, in a peculiarly dark sediment of fresh urine.

On the 17th of May, 1852, the first sporadic case of *bloody urine* was noticed. A year afterwards, it became a symptom of frequent occurrence, grouping, and giving a character to the cases, and then disappearing, as has been already noticed. In a few cases, such as that of Johnston (Seaman's Hospital, October, 1852), it assumed the form of active hæmorrhage. It appeared on the first day of fever in the case of Farish (Seaman's Hospital, June, 1853). In many of these cases of bloody urine there were no casts of tubes or of epithelium; and in a few cases there was "smoky" urine, with a thin layer of blood corpuscles as a sediment when the supernatant fluid showed only a mere trace of albumen (as in the boy Alger, Seaman's Hospital, 5th of January, 1853). In some few cases the blood intermitted, as in the cases of King and M'Call (Seaman's Hospital, January, 1853). In the latter case, the bloody urine of the morning was succeeded by pale urine, with cloudy sediment, which consisted of mucous corpuscles and organic globules, in the evening. The bloody urine, in many cases, seemed a favourable sign; and the interpretation of it probably was, that the hæmorrhage, proceeding from the calyces or pelvis of the kidneys, tended to relieve the congestion of the secreting apparatus—such as in the interesting case of King, above referred to, wherein the urine was bloody, highly bilious, and copious.

These observations on the urine of yellow fever, refer chiefly to that of males. With that of females, the difficulty of obtaining pure specimens was almost insurmountable. This arose, not only from the action of the bowels, but at that stage when the urine should become a study of great value and interest, the catamenia were sure to appear, whether due or not, and thus effectually embarrass the examination of the urine. My impression however is, from the few imperfect observations that could be obtained, that the urine is found less frequently, and less highly, albuminous than in males; that it is more quickly and abruptly terminated in convalescence; and that the flow of urine is fuller throughout. If the numerical method of induction applied to this subject should confirm this opinion, what is the rationale of the fact? Is the female urinary apparatus better endowed than that of the male? Is there any relation between the tendency to suppression and the calibre of the tubuli uriniferi? Or does the vaginal or catamenial hæmorrhage tend to relieve the renal congestion?

While our experiments and observations on the urinary symptoms were going on, they were extended to such cases of intermittent fever as presented themselves. In about twenty cases of this disease, contemporaneous with the epidemic, the urine was examined for albumen, and in no instance was it found present. One of these cases was that of Josefa de Susa, who had been previously in the hospital with yellow fever. She had had copious black vomit; a bloody furuncle then appeared on the cheek, and terminated by an abscess of the left parotid gland. A singular circumstance occurred in this case. The operation of opening the abscess caused nausea and vomiting, and renewed, for a day during convalescence, the stage of acid elimination. This woman returned to the hospital about two months afterwards, suffering with intermittent fever, and her urine was examined for albumen without detecting a trace.

On the 3rd of May, a mixed case of yellow fever and small-pox occurred in a Portuguese boy, named Manuel Gomes. He had been treated in town by an experienced practitioner for yellow fever, and sent to the hospital, in which the treatment was continued. He had the red-tipped and red-edged tongue on admission, irritability of stomach, with greenish, acid ejections, flushed face, and *albuminous urine.* He had been ill three days before admission, and the day after his admission the first rash of small-pox eruption came out; and subsequently he was removed to the small-pox branch, where he recovered in due time. This case brought attention to the necessity of ascertaining the state of the urine in small-pox, in order to estimate the value of albuminosity of urine in differential diagnosis. Several experiments were then made on the urine of small-pox patients, but no albumen was found. No record was kept of these extemporaneous experiments. But on the 10th of May, all the patients in the small-pox wards were subjected to examination with the following results:

Name.	Number of days ill.	Condition of urine.	Specific Gravity.
— Cuffy	42	Noncoagulable	1·014
Henry Cumings	14	do.	1·012
Tom. Manning	22	do.	1·016
Manuel Gomes (above)	8	do. (effervesces)	too scanty for urinometer.
George Warren	8	do.	1·008
Zacharias	8	do.	too scanty.
Maria Lewis	14	do.	1·017
Elizabeth	22	do.	1·008
Diana Sam	10	do.	1·008
Eliza Grant	8	do.	1·022
Juliana di Silva	12	do.	1·023
Susan Ward, no urine at the time of ob.			
Stephen (negro, 2 years old) eruption just out on third day		do.	
Mary M'Crae, admitted yesterday, eruption just appearing		do.	

At the end of April, 1853, a Portuguese child was admitted with purpura. She recovered, without medication, in about two weeks. She had no fever, nor irritation of mucous membranes, nor hæmorrhage; but her skin was thickly studded with petechiæ. This is a very rare disease in Demerara. I have no recollection of more than three cases within my experience, and this is the only one which had occurred during several years. The urine was albuminous in this case.

While on the subject of the urinary symptoms, it may be remarked that the report of the members of the Council of Health of Cayenne to the Minister of Marine, on the subject of the epidemic of French Guiana, announces that they carefully examined the urine, and discovered in it and the kidneys a notable quantity of *pus*. The following is an extract from the report:

> "Reins.—Les désordres des organes urinaires ont été constant. Les reins avaient perdu leur consistance; on distinguait cependant les deux substances, et la corticale semblait plus molle; toutes deux étaient décolorées; ces organes étaient souvent gorgés d'un sang diffluent et contenaient un fluide lactescent ressemblant a du pus, et même du pus chez un grand nombre de sujets. Un fait digne de remarque, c'est la promptitude avec laquelle le pus disparaissait quelques secondes apres l'incision de cet organe: il faut observer très attentivement pour constater sa presence, au moment de l'incision, car il se confond immédiatement avec les autres liquides." . . "Les urines avaient le plus souvent un teint jaune verdâtre qui leur était communiqué par la bile, de la presence de laquelle nous nous sommes assurés au moyen du réactif signalé par M. Dutrouleau. Elles contenaient aussi du pus qu'il fallait de grandes précautions pour apercevoir, car il se mêlait a l'urine avec la plus grande facilité. Venait-on le filtrer, il restait sur le papier, et alors, au moyen de l'acide nitrique et de l'ammoniaque, on en constatait facilement la nature."

Notwithstanding the great precautions taken, this statement as to the existence of pus in the kidneys and urine, is clearly founded on fallacious observation; and the error is due, no doubt, to the want of microscopic aid in the investigation. If a section of a yellow-fever kidney be made, and a papilla be uncovered from its calyx, and, either with the back of your knife, or finger and thumb, you compress this papilla, a purulent looking drop will be expelled, which under the microscope will show a multitude of epithelial granules, and some more or less perfect tube-casts. Now, undoubtedly, this was the fluid which the Cayenne observers mistook for pus. The extract given was from the chapter on the "*kidneys*." The following is the entire chapter devoted to the subject of the urine, and it will be seen from it, that they were not aware of the albuminosity of the urine in yellow fever nor the nature of its sediment, and how easily. therefore, they fell into the mistake:—"Urines.—La suppression complète des urines ne s'est montrée que rarement et seulement au début de l'épidémie. Quand il y avait suspension on pouvait le plus souvent, a l'aide du cathétérisme, en évacuer une notable quantité. Elles étaient peu colorées ou légèrement teintes de jaune-verdâtre par la bile, quelque fois sedimenteuses. Chez les convalescents, elles devenaient parfois abondantes et colorées."

It is almost superfluous to remark on the important indications that arise out of the study of the urine symptoms in the medical management of yellow fever. Placed in a ward of such patients, and led to the expectation of seeing no other kind of case presented to you for treatment, yet a glance at the urine at the bed-side of the patient may enable you to decide at once that the case in hand is *not yellow fever*. Such instances have happened with the fevers of peritonitis, rheumatism, and pneumonia. Not only are these symptoms extensively useful in clinical diagnosis, but they will probably be found the characteristic of the specific differences of tropical fevers, and may transfer yellow fever to an entirely new place in the classification of disease. The importance of these symptoms, as an auxiliary to defective *surface* symptoms, in identifying the true nature of the ailment, is at once apparent. The albuminosity, also, furnishes one of the most obvious manifestations of the disease entering its second stage, and its extension to the great solid viscera. Cases have died, as will be explained, even when the urine was full and free; but (as in the case of the mate of the *Sobraon*) life is prolonged thereby; and no guarantee of safety in one epidemic was so sure as an unobstructed action of the kidneys; and no sign, not even black vomit, so dooming as a suppression of urine. Hope then was gone. No matter how desperate the condition otherwise, if there was copious transparent urine, though ever so coagulable, and black as ink from bile, the struggle was hopefully maintained. For it was felt that the system was still competent to the elimination or decomposition of the yellow fever poison. But *suppression* after the abundant appearance, or curdy appearance of albumen and tube-casts, rendered despair reasonable. The scanty oily-looking urine was generally present in cases that might be abandoned. The tube-casts had disappeared—for the capability *of washing out these plugs of the urinary tubules* no longer existed: they were irrecoverably choked; and the bulk of the scanty secretion seemed to be derived, not from the kidney, but the bladder itself. It may be safely affirmed, that for a correct knowledge of the progress, diagnosis, and prognosis of yellow fever, the close observation of the condition of the urine is indispensable; and that after the attempt to abort the disease has failed, the prime object of solicitude and of treatment is the function of the kidney.

CHAPTER V.

When the attack commences, as it frequently does (not sporadically, but in clusters, as is the mode of appearance, disappearance, and re-appearance of the several prominent symptoms) by diarrhœa or cholera, it is seldom that any opportunity offers of examining the alvine evacuations; for this affection is of short duration. When the disease has not so commenced, the first stools observed are generally those resulting from the administration of medicine. In the hospital reports of our early cases, the following phrase frequently occurs:—" Stools characteristic of the powder." These were the evacuations which followed the resolvent or aborting dose and castor oil. They were generally copious, feculent, pultaceous, with old fæces, and sometimes horribly fetid. They had generally a mottled heterogeneous appearance, made up of old fæces and pulpy carbonaceous looking matter, and a copious intermixture of yellow bile with a white *materies*, the latter having the appearance of chopped half-boiled eggs. This was the stool characteristic of the powder. It had brought away old accumulations of constipated matter: it had acted powerfully on the liver and mucous glands, and it had removed the black material which we often see in the early stage of the yellow fever, and which in some cases constituted the entire mass of fæces, and which we named the *melanotic stool.* The cases of Mether, Farry, and Goodnight, in the Seaman's Hospital, about the 4th of April, 1852, furnished perfect specimens of this description of alvine evacuation. The melanotic stool, or patches of it, seems the first tangible morbid product of the disease, and is highly diagnostic in the first stage. Its tint varies. It is black (as after the ingestion of preparations of iron) or blackish-brown, or blackish-grey. It is always in considerable quantity and pultaceous. Neither in appearance, or in the stage in which it is found, need it be confounded with another dark stool —the scanty *black-vomit-stool,* which appears at the close of the disease. Sometimes, however, scybalæ of the melanotic stool unexpectedly appear in the evacuation after it might have been supposed that the bowels had been completely emptied. Very dark green bilious stool, *en masse,* has been hurriedly mistaken for this; but the former is thin, while the latter is consistent, and tilting up the former against the white sides of the pot readily detects its true colour. It is probable that this melanotic stool derives its appearance from the extravasations of blood in small quantity into the intestinal canal from the cæcum or colon, and this acted on and blackened by some of the intestinal gases, or acids, mixes with the fæces and communicates the characteristic tint. As an indication, its importance may be ranked with that of epistaxis, and below that of the slight florid streak of blood which is occasionally seen with the mucus in the early vomitings, particularly when the quantity vomited is scanty, and there is much retching. The condition of the intestines in which the melanotic stool appears may be the diminutive of that in which a hæmorrhagic dysentery ushers in the fever, as in the rare cases of Lynch (Seaman's Hospital, 24th of December, 1852); or Carmichael (Seaman's Hospital, 13th of February, 1853); or Morris (Seaman's Hospital, 3d of June, 1852), in the fatal cases of which the cæcum presented the appearances generally observed in the stomach.

The appearance of the bilious element of the stools is at first yellow, and subsequently (contemporaneously with the stage of acid elimination) green. Up to the period when it ceases altogether it is generally copious. About the stage when the suppression of urine occurs, the stool becomes abilious, though sometimes a tint of it is observed to the last. After the melanotic stool has passed away, another appears, which is also very characteristic. It is named in the hospital records the "*caddy stool,*" from its resemblance to the fine dark sandy mud, so common in our alluvial deposits, and known by that local designation. This alvine evacuation is of a dirty grey colour, abilious, and liquid, with a sediment (caddy like). It cannot be mistaken for the melanotic stool. It sometimes disappears, or is replaced by the restored secretion of bile, as in the case of George Brasset (Seaman's Hospital, 15th of July, 1852), and its sedimentary character as well as its colour is then lost; and when it appears, as in the same case, it is deficient in its crystalline constituents. A small quantity of bile may be occasionally present without destruction of its identity, as in the case of Thomas Young (Seaman's Hospital, 1st of September, 1852). The composition, as well as the appearance of the stool, is peculiar. If a small portion of the sediment be taken up with a pipette, and submitted to the microscope, well defined crystals of triple phosphates and uric acid will be found, sometimes singly, and sometimes together in the same specimen, as in the following cases:—George Brasset and Thomas Young, before-mentioned, J. Doherty (Seaman's Hospital, 20th of July, 1852), Morgan (Seaman's Hospital, 17th of September, 1852), W. Munro

(Seaman's Hospital, 29th of November, 1852), W. Burns (Seaman's Hospital, 26th of December, 1852), and Abraham Limisson (Seaman's Hospital, 25th July, 1852). In addition to the above crystalline bodies, it contains numerous little amorphous masses of black opaque matter, which seems to be its *constant* ingredient. In Brasset's case the uric acid crystals were very numerous in pale cubes; there were also coherent rhomboids, and many small masses like yellow quartz. They entirely disappeared when the bile re-appeared. In Thomas Young's case, when some bile was present, triple phosphates alone were found. In Morgan's case the triple phosphates were absent, and the uric acid was chiefly in long rhomboidal prisms in coherent parcels, and single hourglass-shaped crystals. In the case of Wallace (Seaman's Hospital, 22nd of October, 1852), when a little bile tinged the caddy stool, triple phosphates alone were present. In the case of James Carson (Seaman's Hospital, 24th of December, 1852), some crystalline bodies were observed, the nature of which we could not determine. They were not unlike broken quinine crystals, but they wanted the fibrous character of that substance, and they were much more regular in their outline, sides, and angles, than is ever seen in the sulphate of quinine. They also dissolved in dilute hydrochloric acid, which sulphate of quinine does not, and for the same reason they were not uric acid. They were insoluble in liquor potassæ and ammonia, and as several of them showed some imperfect prismatic forms, I was inclined to believe them to be triple phosphates, although this opinion is discountenanced by the fact that triple phosphates are always so decisive in their forms, and are soluble in the acids mentioned. Among the crystals and black amorphous bodies of the caddy stool sediment evident under the microscope, are also found bright yellow oblong bodies (cholesterine?) somewhat darkened, but not much altered by hydrochloric acid. Their bright yellow colour, unseen by the naked eye, contrasts strongly with the grey and crystalline heterogeneous matter which surrounds them. If the patient had been taking soup, vegetable and other débris (such as the cellular tissue of onions), will be found also in the sediment. This caddy stool may exist without the presence of the urinary crystals. This variety of it was particularly noticed in January, 1853, when blood corpuscles began to appear in the urine, and when it was paler and less albuminous, and the tube-casts were thinner and fewer, and the urinary organs seemed altogether less embarrassed in their functions than usual. At this time also a tendency to torpor was observed in the early stage of the disease, and croton oil had to be substituted for the usual mild purgative. There is a spurious caddy stool which I have observed in the case of Judge (Seaman's Hospital, 24th of August, 1852). In this case the sediment consisted of undigested starch globules, stained by dark green bile, and also bile globules or cells (?) of a striated appearance and bright yellow colour. The caddy stool was observed as well among the Portuguese immigrants as the seamen, and its composition was similar.

As the disease still further advanced, and towards its fatal termination, the alvine evacuations again changed their character, and became scanty and mucous. The mucosity varied much in consistency—from gelatinous, as in the case of H. Collins (Seaman's Hospital, 4th of December, 1852) to that of rice water, as in the case of H. Britton (Seaman's

Hospital, 1st of January, 1853). Its ordinary consistence and colour was that of thick mucilage, and it was more or less in quantity as it was more or less thin. The mucous stool in the case of Laman (Seaman's Hospital, 20th of September, 1852) was half an ounce in quantity; that of Dryburgh (Seaman's Hospital, 25th of July, 1852), about one ounce; that of H. Russel (semi-gelatinous), about one drachm. The mucous stool was also variously tinted. It was sometimes grass, or olive, or spinage green, sometimes fawn-coloured, sometimes primrose, sometimes rusty, and sometimes *brown*, or *black*, or *streaked*. The last described colours were denominated "*B. V.* (black vomit) *stool*." These mucous stools almost always appeared *after* black vomit, and were contemporaneous with the scanty urine before described, when it appears. The alvine evacuations in yellow fever, from the beginning to the end of the attack, are always alkaline, except in one instance—that of the black vomit stool: in that it is always acid. Its chemical quality is evidently due to the admixture of a portion of the black vomit, which has descended (if not found in the intestines) by peristaltic motion into the intestines, and mixed with the scanty mucous stool, and in such quantity as not only to neutralize it, but be in excess. The scanty thick mucous stool—almost a jelly—has generally a little thin serum around it in the bottom of the pot. The bulk of all these varieties of the scanty mucous stool consists of mucus, broken-up epithelial matter, and myriads of epithelial granules. Sometimes little wavy flakes, like morsels of cuticle, are also to be found. They also frequently contain the crystalline bodies of the caddy stool, particularly when they are rather thin and serous. By appearance, they would be taken for rectal stools and the results of tenesmus; but such is not the case. A burning sensation is often complained of, but seldom any tenesmus, and no doubt these stools consist of that mucous matter which we find after death lining the intestinal canal generally. In a few cases, where there has been total suppression of urine, these stools have become diarrhœal, as in the case of the master of the *Hinda* (private lodgings). In him the procession of the symptoms was as follows: "Caddy stool, then a urinous-looking stool without bile, then a reddish mucous fluid, as if altered blood had stained it, then a black molasses-like stool, evidently the admixture of imperfectly formed black vomit,—all within twenty-four hours, during which time there has been no secretion of urine, and he has been very frequently on the chair for diarrhœa. He is becoming very restless; tremor of arms; speech faltering; intelligence dull." He died on the following day. The evacuation of these stools seemed to be a well-meant effort of nature, and compensatory of the suppression of urine. As will be seen by the foregoing extract from my notes, there are several modifications of the *mucous stool*. But all the elementary forms, I believe, are included in the descriptions already given. Sometimes fatal cases terminate with hæmorrhage from the bowels. In such instances, of course, the alvine evacuations just described will not be apparent.

In observing these evacuations, a minute portion placed on the glass slip should always be diluted with a drop of pure water. Great care was taken to provent fallacy of observation, and to assign appearances to their proper causes. At first we suspected that the mucous stool might have originated from the irritation of the resolvent dose on the mucous crypts of the intestine, and the more particularly as in some of these stools we detected a few spiculæ of quinine. But we found that cases which had been neglected and untreated, and brought in to us in the last stage of disease, presented the same symptoms. It was also irrespective of the number of doses given, and it was separated by intervening phenomena from the supposed exciting cause. We were hence compelled to infer that this mucous stool at the close of yellow fever forms a portion of the normal morbid phenomena. We also suspected, at one time, that the crystalline matter of the caddy stool might have been due to the actual presence or the chemical agency of the medicinal substances employed; and the magnesian mixture first came under suspicion. But we found the same triple phosphates where this medicine had not been employed. We experimented next on the nitrate of potash, carbonate of soda, and aq. acet. ammon., without success in explaining the presence of urinary salts in the stool. The urine which had been passed, in a different vessel simultaneously with the stool, was also in all cases explored, and care was taken that not a drop of the two liquids had mixed; and in no case were these crystalline bodies found simultaneously in the stool and urine. The minute particles of undissolved quinine certainly resemble uric acid crystals, and when not broken, might, to a prurient imagination, assume the form of triple phosphates. But when large, their *fibrous* structure is apparent, and when broken, the irregularity of their sides and angles cannot simulate regular crystals; and they dissolve in hot alcohol, and while dissolving, show the long striæ of their structure. Feeling, however, our great liability to error in these observations, I transmitted to England a specimen of the *caddy stool* for Dr. Davy's examination. It unfortunately decomposed before its arrival, and all trace of crystalline material had disappeared. It is with diffidence, therefore, that these observations on the composition of the alvine evacuations in yellow fever are offered to the profession. They require the confirmation of future observers. But in the meantime, it would seem as if the intestines could, to some extent, assume a vicarious function with the kidney in yellow fever. *Live* lumbricoides were occasionally vomited and passed by stool during the course of the disease, and found in the intestines post-mortem. In the fatal case of Cornice, of the *Una* (private lodgings), a *dead* tapeworm was passed after the first dose of medicine. In convalescence, after the alvine evacuations have obtained bulk and consistency, they are for some time of a lead colour and abilious, while the urine is copious and charged with bile, and the skin jaundiced. This condition of the stool often changes suddenly. The convalescent vomits up, or passes off by the bowels, a quantity of yellow bile, and the jaundice symptoms begin from that moment to disappear. This secondary jaundice, a true sequel of yellow fever, I am inclined to think is due to ecchymosis around the orifice of the *ductus communis choledochus* mechanically obstructing the vent into the intestines; and its sudden removal arises from the absorption of that ecchymosis and the pressure of the engorged gall-ducts from any sudden muscular exertion.

CHAPTER VI.

The first ejections from the stomach of a yellow fever patient are seldom seen by the physician, but are described to him as food, &c., in a more or less digested state. After these have been discharged, mucus and bile next appear, occasionally with a streak or speck of blood, and with violent retching. The ejections of the stomach are at this time alkaline. The master of the *Valiant* vomited as well as purged yellow bile by pints, up to the fourth day of his illness. This bilious vomiting was succeeded by the ejection of bloody mucus from the stomach, and simultaneously the urine became albuminous. But still he recovered without the case proceeding even to acid elimination. This, however, is a rare case, and generally after the first vomiting the stomach becomes tolerably settled until the second stage sets in, on the second, third, fourth, or as late as the fifth day of the disease. Then, without warning or nausea, the stomach, on any trifling provocation, may eject a quantity of clear, pale, almost limpid or slightly opalescent *acid* fluid, as in the cases of Peter Brodie (Seaman's Hospital, August, 1852) and Harrington (Seaman's Hospital, 7th of December, 1852). Here the disease may terminate or go on to a protracted period, and still make no further progress, the symptom, as it were, becoming a chronic affection, as in the case of Tolloway (Seaman's Hospital); or, as usually happens, this symptom is merely the precursor of a higher and more complex elaboration of the stomach. The symptom now under consideration is the *white vomit*, and indicates the beginning of the stage of acid elimination, and is generally contemporaneous with the first peeling of the tongue. In a few cases, *hoarseness* has immediately followed the ejection of this fluid, as in the case of Mrs. W. But this can scarcely be ascribed to any corrosiveness of the fluid, but rather that in these unusual cases the peeling of the epithelium had been earlier, and extended further into the fauces than usual. In some cases, as those of Mr. W. and Master J. B., this vomit seemed equivalent to the perspiration of intermittent fever, and the whole ailment instantly vanished. In others, as Mrs. H.'s servant, the whole *febrile heat* and excitement ceased, but the disease passed on to a fatal termination notwithstanding, and occasionally the advent of this symptom seemed void of all modifying influence on the progress of the disease. The name *white vomit* may be objectionable as a term to indicate what is meant by it, for this ejection is often bile tinted, but the bile is evidently an extraneous and accidental ingredient in its composition. There is a *spurious white vomit*, which seems to have no critical effect, and is innocuous. It is plentiful, thick, ropy, and alkaline, and consists almost altogether of mucus. Occasionally in true white vomit an admixture with this ropy fluid takes place, and in such abundance as to neutralize the acidity of the former (as in the case of Miller, Seaman's Hospital, 4th of March, 1852), and the presence of the acid is ascertained only by its action on the bile which may be present, and to which it communicates a grass or verdigris green colour. True white vomit is serum, more or less acid, which, after repeated trials, remained clear on the application of heat and nitric acid. Sometimes the stage of acid elimination is first manifested by the alvine evacuations, as in Mrs. H.'s servant, before referred to, and is indicated by the changed colour of the bile. In some rapid cases, such as that of Mr. Dods (of the *Grafton*, private lodgings), in which the urine was albuminous on the first day, there was no intermediate white vomit or green-tinted vomit. He ejected yellow bile copiously up to the second day, when black vomit came on abruptly. The transition of symptoms, however, is generally gradual, and the white vomit, stained or unstained, is formed, and presents the "snuff-like" specks, the "suspicious sediment," the "*black vomit incipiens*," before it merges into well-defined black vomit. Although there is much reason to believe that the acids of this fluid are those only which are natural to the gastric juices, yet in several cases we have found in this stage the saline acid, as in Ringham (Seaman's Hospital, 2nd of March, 1852) and the same in A. Morison (Seaman's Hospital, 1st of March, 1852) and in O'Donoghue. In the last-named, also, rusty black vomit-looking matter, and highly acid, was expectorated from the lungs and bronchi, as if the acid were a *materies morbi*, or as if other surfaces might assume, under certain circumstances, vicariously, the functions of the great acid secreting organ, the stomach. In the case of Robert Smith, also (Seaman's Hospital, 10th of June, 1853), who suffered from pleuro-pneumonia as a complication of yellow fever, after the *rusty* expectoration ceased, the sputa were of a grass-green colour, apparently from the action of acid on the bilious constituent of the expectoration, while the skin was yellow. The *stage of acid elimination* continues to the close of the disease, and is most intensely manifested during the production of black vomit. Several attempts were made by us to determine the chemical characters of this acid, but without conclusive or satisfactory results. As in the investigation of the alvine evacuations, a difficulty is met with at the threshold, in discriminating what is precisely normal to the subject of examination from what is extraneous and accidental. On one occasion we distilled some of the filtered serosity of black vomit; an acid came over, and we continued the distillation till the vapour ceased to affect the test-paper. The residue in the retort remained acid. Here, then, one point seemed determined—that in black vomit there existed at least two acids, one volatile, the other fixed. We neutralized the distilled fluid with ammonia, and afterwards evaporated almost to dryness, then treating this nearly dry residue with a drop of concentrated sulphuric acid, we detected the *acetic acid* odour. Here appeared another discovery—the volatile acid was certainly acetic. But almost all the patients drink wine. Here was probably the source of this acid. On the 25th of June, 1852, some of the black vomit of Leonard (Seaman's Hospital) was filtered through paper, and put away for future experiment. It was highly acid. After remaining four days it was again examined, and still acid. On removing the cork a slight explosion and effervescence ensued. Microscopic plants and sporules were observed in it. It had a slightly vinous odour. On applying Trommer's and Moore's tests, I discovered the presence of sugar to a large extent. What! is this a gastric diabetes? can the stomach secrete sugar like the kidney? and is this sugar converted into poisonous oxalic acid? Here was a pretty hypothesis. But in order to test it, I stopped the allowance of sugar in the yellow fever diet and drink, and since then there has been no more effervescence, nor torulæ, nor sugar, in the black vomit. The black vomit serum always responds to the tests for hydrochloric acid; but as marine salt may always be expected in such a fluid, the results are subject to fallacy in the hands of untrained operators. I therefore entirely distrust any of our chemical researches on this branch of the subject, except those of the most simple and obvious kind, and will not even recount the many experiments that were undertaken with a view to determine the nature of the acid or acids contained in white and black vomit. I, however, forwarded to Dr. Davy a bottle of the fluid, and received from him the following result of his examinations, as contained in a letter from him, dated the 27th of December, 1852, of which the following is an extract:

> "The black vomit was not, I think, much changed. When the cork was withdrawn no air escaped; on rest, and many days were necessary, it separated into a pretty clear brownish fluid, and a blackish sediment. The fluid I found of specific gravity 1·049. It became slightly turbid when heated, denoting the presence of a minute quantity of albumen, which was re-dissolved on rest, and probably in consequence of the acid present. The nature of the acid I endeavoured to ascertain. I think I may say it was principally the muriatic, with a trace of sulphuric. I could not satisfy myself of the presence of either the acetic or lactic. Quinine I detected as well as starch, with the former of which no doubt the sulphuric acid had been introduced. Muriate of ammonia I also found in the solution, and in a notable quantity. I could discover no traces of urea, or lithic acid, or of oxalic acid. The sediment—the black matter—was small in quantity. When dried, it weighed only two grains. Under the microscope it exhibited no well-marked or distinctive character. Incinerated, it left a comparatively bulky ash, the greater part of which was not soluble in an acid, and seemed to be chiefly siliceous, no doubt derived from food. The weak acid solution contained a little iron and phosphate of lime, such as the colouring matter of the blood yields when similarly treated."

In corroboration of some of the results of this analysis, I may mention that I have repeatedly examined the serum of black vomit for urea and uric acid, and have uniformly failed in detecting either by the usual chemical modes of procedure. However, as in the case of Brown (Seaman's Hospital, 23rd of October, 1852), on evaporating a drop of the serum in the sunshine on a plate of glass to dryness, not only were crystals of muriate of ammonia noticeable, but also dagger-shaped and crosslet crystals of muriate of soda were found, the form which that salt is said to assume in the presence of urea. In the case of Sullivan (Seaman's Hospital, 3rd of October, 1852), a distinct urinous odour was perceived by all present at the distillation of his black vomit. I had been, for a considerable time, watching for manifestation of the effects of renal obstruction on the blood and excretions. It was on the 1st of October, 1852, that, notwithstanding our repeated failures to obtain urea or uric acid from the black vomit, and the acid character of that fluid, that we thought of questioning it for ammonia. The following is the entry in my notes of the result: "To-day, at the Seaman's Hospital, to the filtered liquid of black vomit (which was highly acid) I added in a test-tube an excess of liquor potassæ. On carefully introducing a test-paper into the vacant space it became blue, and a glass rod with muriatic acid showed white fumes. This was the black vomit in Wood's case. On application of heat to the tube the ammonia was evolved abundantly."

The presence of ammonia in black vomit is universal, that is, it has always been found when looked for, and may be considered one of the tests of black vomit. *White vomit* also contains it in a notable quantity. The specific gravity of black vomit was frequently ascertained, and found to vary from 1·004 to 1·006 at the usual temperature of the air at noon, 86°. Its variations in density were no doubt chiefly occasioned by the accidental admixtures of fluids drunk. Occasionally it was slightly bloody, and then it was coagulable. But in general, the acid of the black vomit seemed adequate to the precipitation of all the albumen. And thus we have had the paradoxical condition of an animal fluid containing hæmatosine without albumen. The sediment of black vomit seemed to consist of coagulated albumen and the débris of blood-cells. In no case in which the black vomit was normal to the eye, was a single perfect corpuscle observed. When pressed through a paper filter the colour is rendered considerably paler. The sediment of black vomit seems more highly acid than the supernatant liquid—it makes a stronger impression on the test-paper. The sediment acts as a ferment on liquids containing sugar. In samples which have been filtered and neutralized to excess with aqua calcis, and put aside, its condition of alkalinity will persist. But in the unfiltered portions, in a few days, the acid re-action will be fully restored. The sediment behaves, under chemical re-agents, similarly to the albumen of the urine. It is dissolved by liquor potassæ, and restored by nitric acid. Several instances have occurred in which black fluids have been ejected from the stomach, and mistaken for black vomit. On the 29th of February, 1852, while passing through the wards of the colonial branch of the hospital, wherein there were several cases of yellow fever, an intelligent nurse brought a basin nearly full of what he considered black vomit, which had been vomited by a Portuguese boy who lay huddled up in bed, seemingly very ill. He had been admitted the previous day for anæmia, and had taken for it the compound steel pill of the hospital. Now although the matter vomited was *blue* black instead of *brown* black, and had not that division into sediment and liquid usually observed, and although the patient's tongue was stained inky, yet a careless and inexperienced observer might possibly have mistaken the ferruginous dose for genuine black vomit. Other instances less palpably fallacious might be adduced of error of observation in this particular, and it is evident that *tests*, independent of mere superficial appearances, are desirable to ascertain the presence of this peculiar and significant product of the stomach—the more so, as traces of it may be present wherein there is no discoloration of the vomited matter, as when the blood corpuscles or hæmatosine has become enveloped in mucus, and thereby kept apart from the action of the acid—cases of which I have seen. The first test should be for its acidity; and, if found, the test is, *pro tanto*, corroborative. But instances, not many, to be sure, but several, will be mentioned in which the fluid ejected from the stomach, and pathologically identical with black vomit, was strongly alkaline. The second test is the solution of the sediment by *liquor potassæ*, which gives the fluid a port wine colour, and brings it out in fluid otherwise pale; when grey mucous flocculi entangling the blood have hitherto suppressed the true black vomit colour, (as in the case of R. Stopsford, Seaman's Hospital, 13th of February, 1853.) The restoration of the sediment by nitric acid is further corroborative of this test. The third test is the disengagement of ammonia from the fluid by the addition of an excess of liquor potassæ when the black vomit is acid, and by heat alone in the exceptional cases wherein the black vomit is alkaline. This third test may be considered pathognomonic. There is sometimes, in the early stage of acid elimination, a vomit which might be mistaken by the inexperienced eye for black vomit. It has a dark half-floating sediment. This, on examination, will be found to consist of epithelium and mucus, the former stained with bile, the true colour of which comes out under the microscope. In the same stage the vomit also is sometimes a *glairy* acid fluid, with greyish-black tenacious sediment (as with Robinson, Seaman's Hospital, 3rd of January, 1852), which is neither dissolved by liquor potassæ nor shows the port wine tint. The microscope reveals its nature. Like the former, it also consists of epithelium, tinted with bile and closely invested with mucus. The light flocculent matter frequently found floating in genuine black vomit is always mucus, entangling various substances. In one case which I examined, milk globules were found. The patient had drank tea a short time previously. *Normal black vomit* may be described as having a laminar or granular sediment, of a deeper or paler shade of brown, sometimes verging on jet-black, with a clearly-defined supernatant serum of low specific gravity, and without mucosity, partaking of the colour of the sediment, but sometimes nearly limpid when the sediment is black (as if all the colouring matter had subsided). Many deviations from this standard occur from causes already alluded to, such as the presence of ingesta, hæmorrhage, and excessive secretion of mucus. In one case (that of L. Valdon, Seaman's Hospital, 27th of August, 1852), both serum and sediment were bile-tinted. There are, however, two singular varieties worthy of particular remark, though appearing rarely: they may be called the "*caddy black vomit*," although they rarely contain sediment like the alvine evacuations of that name. The first two cases which happened were probably those of Smith and Myhal (Seaman's Hospital, 7th of February, 1852). But at that time the peculiarities were not duly appreciated. The next case was in private practice, in the person of a Mr. Dods, the mate, and subsequently, before his death, the master of the *Grafton* (29th of June, 1852). The next were in Theodore Ternaben (Seaman's Hospital, 4th of September, 1852), and Josea Joachim (Colonial Hospital, 7th of December, 1852), and the last in the steward of the *Livonia*, (in private lodgings, 25th of December, 1852). This vomit does not persist. It appears but once or twice in the individual, and is succeeded by or alternates with normal black vomit. It is of a dirty grey-brown colour, rather homogeneous in appearance, about as thick as mucilage, rather opaque, contains *vibriones*, and is generally strongly alkaline; but may be acid, as in the case of Ternaben. It would seem as if the ammonia in such cases was formed and poured out in excess of the acid, or that the acid was deficient in normal quantity. Josea Joachim's breath was tested, and found highly alkaline. The application of heat to his vomit, without any addition, caused the evolution of copious ammoniacal fumes. In Ternaben's case liquor potassæ rendered the vomit transparent, produced a claret colour, and the specimen gave off ammonia, as usual. The microscopic examinations were made within three hours after the vomit had been ejected, quite as early as was the practice in other cases, and yet no vibriones have been observed in the other varieties of black vomit, not even in specimens which have been put aside for several days. In Ternaben's case it is perhaps not correct to call the animalcules *vibriones*. Some of them were globular, about a quarter the size of a blood corpuscle, and some linear, but the latter seemed to be formed by the attachment of four or five of the monads. The movements were very brisk when the light was strong. Were these animalcules the cause of the change in the appearance of the black vomit, or had the excess of ammonia that effect? I incline to the former opinion. For the acid black vomit of Ternaben had the same aspect, and although carbonate of ammonia has been frequently administered internally, no such condition of the black vomit ever resulted therefrom. Moreover, twenty-two hours after the death of Josea Joachim, I had his stomach opened and a sample of the contents removed. The same description of fluid was then seen, but it was *acid*. It refused to yield ammonia till after the addition of *liquor potassæ*. He had taken no acid food or drink of any description before death. The specimen removed had a slight sediment on standing for an hour. It had a strong, unpleasant, somewhat fetid *garlic odour*. The sediment contained only a few shreds of broken epithelium and cell-walls, but *the whole liquid* swarmed with *vibriones*, and their number in this instance undoubtedly communicated to the fluid its dirty greyish-brown colour. The stomach was found coated with the usual tenacious black vomit lining. This condition of the vomit had its counterpart in the urine, as already noticed.

During the former epidemic it was noticed in cases of black vomit, that when it *preceded* the yellow suffusion the prospects of life were improved. The relations of this fact were not then understood. Black vomit is significant of imminent danger, from the circumstance that it is the *dernier ressort* of nature to relieve that contamination of the circulation which has been produced chiefly by impairment of the function of the kidney, and the retention thereby, within the system, of the worn-out nitrogenous elements of the body and their poisonous metamorphoses. Now, if black vomit appear early in the disease, before its march has extended to the great internal viscera, before the bile function has been disturbed or the urine rendered albuminous, it ceases to be the significant symptom which has obtained so much ill-omened celebrity. It is then the sign of a local, instead of a constitutional affection. I have now before me notes of four cases in private practice of what might be termed *benign* black vomit—those of Miss G., Miss S., a Portuguese woman, and a German baker. All these cases terminated in recovery. As yellow fever cases, they were nearly all anomalous. Miss G. had no fever, but strong supra-orbital pains and albuminous urine. Miss S. had one day fever like a paroxysm of intermittent, and the mouthful of black vomit the same day. The Portuguese woman complained only of *malaise*, and on the second day brought up black vomit. The German baker (was three months in the colony: his comrade, who arrived at the same time, was already dead, from the epidemic,) had fever, but the symptoms were mild, and on the 3rd day he vomited black vomit, *without having had albuminous* urine previously. In such cases the quantity is generally small, and is rarely vomited a second time. Four anomalous cases also occurred in the hospitals during the time included in this report (eighteen months), in which black vomit *preceded* albuminous urine. One was in an anæmic Portuguese, fatal, and presented an extraordinary instance of *truly discoloured* blood after death, —for scarce a trace of even a cell-wall could be found in the port wine-looking sediment. In another (Colin Knoley, Seaman's Hospital, 14th of January, 1853) the black vomit was *succeeded by white vomit.* One of the other two (Reid and Murphy, Seaman's Hospital, 24th of February, 1853) had early black vomit without albuminous urine, seemed to convalesce, but subsequently got albuminous urine and black vomit, of which he died. A post-mortem examination revealed that the first black vomit was probably occasioned by a hæmorrhagic extravasation at the juncture of the œsophagus with the stomach. Among the hundreds of cases of black vomit which I have seen since my attention was directed to the urine-symptoms in yellow fever, those cases just enumerated are all in which a palpable affection of the kidneys was not antecedent to the vomit. But exceptional and anomalous though they be, as Reid's vomit answered to all the tests of genuine black vomit, there is still some mystery about this subject, and perhaps grounds are furnished for the belief that the yellow fever poison acts not only secondarily, in obstructing the liberation of the effete materials of the body, but also directly, in augmenting their quantity. During the first eighteen months of this epidemic there were three cases of chronic disease in which life terminated, to the surprise of all around, with black vomit: these were Dr. B., Mr. B., and Mrs. H. There had been no antecedent fever in either case, nor a single sign of yellow fever that had been recognised. In one of these cases I did not know the condition of the kidneys, and cannot now ascertain, but in the two others I am aware that suppression of urine had occurred for several days before the appearance of the vomit.

CHAPTER VII.

There were many opportunities for becoming acquainted with the condition of the blood during this epidemic. Cupping, the use of the artificial and natural leech, arteriotomy, in a few cases venesection, epistaxis, and other hæmorrhages gave ample opportunity during life for examining its physical, chemical, and microscopic qualities. In no instance could we discover any really abnormal condition of colour, corpuscles, serum, and crassamentum during the first stage, except sometimes the presence of bile. In Mr. Dod's case before mentioned, in consequence of the intense congestion of his face, I opened the temporal artery on the second day of his illness. The blood (three ounces) was florid and coagulated well. The serum alkaline. A little nitric acid dropped into a small portion of it caused instant coagulation, at first opaque white, but after a few minutes a bright yellow, with a *ring* of *purple.* Bile had passed off freely, and until the fifth day, that of his death, decided yellow suffusion could not be observed. The artery burst open several times before his death, and could be with difficulty restrained, but the blood itself showed no abnormal appearance, except that the bile-test became more decisive up to the time of his death. In every instance in the first stage, the blood retained its normal alkalinity. The changes in the blood therefore were found only in the last stages, and in the post-mortem blood. And yet cases, terminating fatally after normal black vomit and hæmorrhages, as in Flynn (Seaman's Hospital, 14th of December, 1852), G. Ball (Seaman's Hospital, 13th of November, 1852), John Knowles (Seaman's Hospital, 13th of November, 1852), H. Stewart (Seaman's Hospital, 16th of February, 1853), are numerous, in which no unhealthy appearance of blood after death could be noticed, except as to the bile-tinge. The following are samples of the entries in my note-book on this subject:—

"*Blood in Yellow Fever.*—Francis Mitchell, to-day, in Seaman's Hospital, had free epistaxis. The blood was florid and formed a good clot. Under Ross' 1-8th inch object glass, found the discs normal. This was his second day of admission to hospital, 5th of December, 1852. . . . *Blood on second day of Yellow Fever.*—To-day I examined the blood of Mitchel, of the *Lucy* (Seaman's Hospital) The appearance of the blood was healthy, so also under the microscope. The *rouleaux* stood almost perpendicular. The blood was obtained by cupping the neck; 7th of November, 1852. . . . . *Blood on the fourth day of Yellow Fever.*—(*After black vomit.*)—To-day I had a few drops of blood drawn by an artificial leech from the nates of Charles Mitchel. The blood was rich and florid to the eye, and under the microscope was filled with apparently healthy corpuscles. So numerous were they that they much impeded the transmission of light, and for the better observing of them, the thinnest portion of the blood-film on the glass had to be selected. They stood up in erect rouleaus; 9th of November, 1852. . . . . *Blood in Yellow Fever after Death.*—In the case of Charles Mitchel, before referred to, there were two considerably sized fibrinous coagula in the heart. On examining the blood taken from the heart, by the microscope, the corpuscles seemed perfectly healthy and numerous. They arranged themselves here and there in rouleaus. The blood was neutral to the test paper."

The appearance of yellow fibrineus coagula in the heart was frequent after the worst cases, and in fact what may be termed the texture of the blood, often remained good. The following is my note on the case of Michael Flynn:—

"*Blood in Yellow Fever.*—To-day, three hours after death of Michael Flynn, I had an ounce-and-half phial four-fifths filled with blood from his heart. Two hours afterwards I examined the phial, and found the crassamentum so firm as to be unmoved by inverting the phial. On its surface was a bright crimson pellicle, concave, with the limits of the concavity extending about 1-5th of an inch up the side of the phial and covered with serum to the same depth. The serum was slightly alkaline. On forcing a pipette down through the crassamentum, I obtained a particle of blood for the microscope. The corpuscles were found flat and dark in their centre to half their radii, as if from collapse of the centre. Although one or two seemed ruptured, the rest of the discs were perfect in outline. On examining the phial three hours afterwards, at 8 P.M., I found that the clot and serum had still further separated, each occupying about a half of the space of the phial. The serum was a little turbid, and the crassamentum apparently softer. On examining microscopically now by the light of an Argand lamp, transmitted through a 'bull's-eye,' scarcely any of the discs had central darkness—neither were any ruptured cells visible. But about half the number of corpuscles had lost their perfection of outline, and were jagged by nucleoli on their edges, and the number of these seemed to increase during the act of observation. At the side of the thin glass, at which a little salted water had been applied, the corpuscles arranged themselves in rouleaus; in another specimen, however, they were numerous, without the use of salted water. In this specimen some imperfect or burst cells were seen. The serum when heated coagulated firmly and gave off no ammonia, with or without liquor potassæ. I saw nothing in this blood decidedly abnormal either in colour or physical qualities or minute organism: at any rate, nothing to countenance the idea that the blood was 'dissolved' or even seriously injured by the progress of the disease; and in this aspect of the case—the result of these observations,—I have been considerably disappointed; for the passive (?) hæmorrhages before death led me to expect a serious change in the circulating fluid; 14th December, 1852. . . . . *Death in Yellow Fever.—Blood normal* in colour and consistency, in the case of John Knowles, Seaman's Hospital. I examined his blood to-day, immediately after death. The heart was gorged. The clot was firm, and in due proportion in the test tube when it cooled. The corpuscles were not to be distinguished from those in perfect health; 13th November, 1852. . . . . *Blood of George Ball.*—Two hours after death still warm. A clot of yellow fibrine, about half-an-ounce in weight, found in heart. The serum of a deep yellow bile-colour. The blood clotted firmly. There was an abundance of albumen. The corpuscles normal, except a little bossed or convex in centre, as if the cells were distended; blood slightly alkaline—did not yield ammonia by heat alone, and not much on the addition of liquor potassæ; although comatose before death, his breath was strongly alkaline." Gibney (Seaman's Hospital, 21st of September, 1852) may be cited as another instance of healthy fibrine in fatal yellow fever.

There is, therefore, no doubt of the fact that the blood in the first stage of yellow fever has no appearance of being unhealthy, except as to its occasional intermixture with bile; and also that in many cases which terminate fatally, and with previous black-vomit, the blood is found normal in all its appreciable qualities, except as to the before-mentioned intermixture. But in order to arrive at this conclusion, the blood specimen must be procured direct from the proper containing vessels. If instead of examining the blood of epistaxis in yellow fever we take as our specimen that which has been discharged from the rectum, or by hæmatemesis, when the acid elimination has been scant and the blood little acted on, a very different condition will be found, as was observed in the cases of Jackson (Seaman's Hospital, 21st of April, 1852), Macnamara (Seaman's Hospital, 4th of November, 1852), and Racy (Seaman's Hospital, 7th November, 1852). The following memoranda will serve to describe the condition of the blood in all these cases:—

"*Examination of Blood of Hæmatemesis of Yellow Fever.*—To-day Macnamara, in Seaman's Hospital, vomited about four ounces of mere blood. It was a thin dark fluid, without fibrine—was strongly alkaline—in appearance like port wine. He had had creosote and soda prescribed for irritability of stomach, but none had been given before the blood was ejected. He had passed urine moderately (three ounces) about the same time that he vomited. It was acid, coagulable, with a few tube casts. His breath was alkaline. The blood was not brightened by treating it with nitrate of potash and muriate of soda. On gently heating a specimen in a glass tube, without any addition, it gave out ammonia freely. The heated blood showed scarcely as large a proportion of albumen as the urine had done. Three hours after the blood was vomited, I examined it microscopically. There was not a single perfect corpuscle found, and very little débris. The fluid was nearly colourless under the microscope. There were numerous vibriones of various sizes; some 1-3rd the diameter of a blood corpuscle in breadth, and twice as long as the diameter of a blood corpuscle; others not half this size. They were equally broad throughout their entire length, and their motions were vermicular. There were also other little moving translucent bodies, circular or globular, about a quarter the size of a blood corpuscle. For the sake of comparison, I at the same time examined some blood of epistaxis which I had brought home from the Seaman's Hospital, and put aside in a phial a week before. When the cork was removed, a slight explosion ensued, and the smell was offensive. It was so thick, however, as to paint the sides of the phial when revolved, and of a deep bright red. When a drop of this was examined under the microscope, no perfect corpuscle was seen, but a large quantity evidently of cell-walls, and the hæmatosine was of a deep tint. There were no animalcules in it. 4th of November, 1852."

Blood passed off by stool, though unmixed with, and of good crassitude and colour to the naked eye, is always found under the microscope with all its corpuscles ruptured. In this intestinal blood, I have never detected vibriones. It seems clear that the alteration observable in the blood from the stomach and intestines, is due in great measure to chemical changes which occur after its extravasation. But though the blood within its proper vessels is often found healthy, through the whole course of the disease up to the last moment of life, it is not always so; and in the last stages is frequently found injured in its obvious constituents of fibrine and cells. As illustrative cases of the deterioration of the fibrine element, reference may be made to those of Morgan and Laman (Seaman's Hospital, 21st of September, 1852), James Walker (Seaman's Hospital, 28th of November, 1852), and Antonio Fernandez (Colonial Hospital, March, 1853). In Morgan's case the fibrine was so diminished in quantity as almost to be lost. In Laman it seemed to have, in a great measure, lost its power of fibrillation. In Walker both fibrine and cells suffered. The right side of the heart was full of dark thin blood, without clots or fibrinous coagula. The colour of the blood was of a dirty brown, and entirely fluid; the corpuscles were all altered and mis-shapen. In all cases, however, the albuminous element seemed, by the rough test of its becoming solid by heat to coagulation point, to be sufficient. A kind of medium deterioration occurred in the case of John Savage (Seaman's Hospital, 19th of November, 1852). My first observation on his case was as follows:—

"*Blood in Yellow Fever.*—To-day John Savage (second day of illness), was cupped on nape of neck. The blood was of a bright vermilion colour, with good clot. The half of the number of corpuscles, however, were rough and jagged, apparently from the adhesion of nucleoli, or the splitting of cells. A few were evidently ruptured and torn. About one-half were normal. Perhaps the heat and spirit vapour in the operation had something to do with these appearances, —14th of November.

The next note is as follows:—

"*Blood after Death in Yellow Fever.*—I had an ounce of blood from Savage's heart about four hours after death. There was a fair proportion of clot, but it was soft, though well separated. The serum looked very thin, and on revolving the blood in the phial, the sides were scarcely stained. But as to the clot, the whole looked like port wine and water. A drop taken from the bottom of the phial, on being examined by the microscope was found to contain corpuscles, but not numerous. A few were injured, but the vast majority were normal. There were none having the appearance before imputed to the heat and spirit-vapour. When the serum was heated, it all set into a firm coagulum almost dry. It was only slightly alkaline, and gave off no ammonia when heated. Although the kidneys, as usual, were much gorged, their function had been but little impaired; thirty ounces of urine having been found in his bladder, though none had been passed for twelve hours before death."

But during life, also, the blood is sometimes found altered. Thus, in Jackson, whose case has been already referred to, after black vomit was established, on the 20th of April, though the blood from epistaxis was florid, and the corpuscles were numerous, they were misshapen, and showed no tendency to form rouleaus. But a few were still normal. Next day, when hæmatemesis succeeded, black vomit and slight epistaxis returned, the corpuscles were still more altered,—they became angular and elongated, with scarcely one normal cell. In the case of John Bridges, admitted to the Seaman's Hospital, 9th of November, 1852, a drop of blood taken by the artificial leech showed the corpuscles spread over the field of view like a pavement. They all seemed flat and jammed against each other, so that there was scarcely any current or movement among them. There was not a single corpuscle of normal appearance; there were no rouleaus, but it was evident that the cells were entire. They speedily became rough by escaped nucleoli. On applying a little salted water to the edge of the glass, currents were immediately induced, and the corpuscles became normal and plump in appearance; but in about a minute they all burst, and the field showed nothing but cell-walls. He had had black vomit before admission. His tongue was denuded of epithelium. He had had no treatment, and described his illness as of only two days' duration. In Peter M'Quin's case (who died early, epileptic, 11th of November, 1852), the corpuscles were flat, indistinct, and irregular in shape, with many nucleoli adhering; but on adding salted water, they bristled with nucleoli like mulberries. Manuel Fernandez was admitted to the Colonial Hospital on the 6th of March, with yellow fever. He had been perfectly *blanched* by previous attacks of intermittent fever. His tongue showed no capillary irritation, his face was pale, and his case at first was erroneously diagnosed, judgment having been biassed by the previous history of the patient. He was treated, therefore, in the beginning, for an intermittent attack. After death there was scarcely any yellowness of the skin. There was no *bloodiness* of the integuments in making the sectio, nor of the tissues ot any viscus but the kidneys and stomach. Every other part was anæmic. The *liver*, though recorded as "blood-congested," from its deep purplish-red colour, was not bloody when cut into. The urine found in the bladder was highly coagulable, though that passed during life was not so. The blood was *highly* ammoniacal, though not described emphatically so in the report, and *was totally dissolved.* In those specimens which I took away and examined both by natural and artificial light, I failed to detect a single normal corpuscle. When the blood was examined, no decomposition in the body had taken place (seventeen hours after death); the *rigor mortis* was beginning to yield. The blood was like port wine in colour and consistency. In one specimen under the object glass, two or three almost invisible attenuated corpuscles crossed the field of view, but none of any description in the other specimens, and not even the trace of a cell-wall was to be found. Could this total dissolution of the blood have been possible at any instant before death? Was it the joint effect of the intermittent fever, malaria, and yellow fever poison? Was it the solvent power of ammonia? The healthy condition of the blood in yellow fever seems associated with free action of the kidneys, or copious black vomit and alkaline exhalations of the breath. And the deterioration of the fibrine has an obvious relation to the amount of *free* ammonia remaining in the circulation. The changes in the *shape* of the corpuscles are probably due to alterations in the density and saline constituents of the serum. The blood of the cadaver in this epidemic, was in the vast majority of cases more or less ammoniacal. In the case of Antonio Fernandez, the water used was *soapy* to the feel till the fibrine was washed out. This soapiness was noticed by Dr. Shier, who called my attention to the fact, before he was aware of our former observations on the ammoniacal alkalinity of the blood in these cases. In a few cases, however. the blood was acid, as in that of Roberts (Seaman's Hospital, 2nd of November, 1852); but it was rarely that ammonia was not extricated in any case by the addition of lime. Bile constantly, ammonia almost constantly, and some undetermined acid occasionally, were the only foreign substances which we were able to detect in the yellow fever blood. Of course, others may have been, and were, likely, present. In order to ascertain the proportions of some of the normal constituents of the blood, I requested Dr. Shier to undertake a chemical examination of some specimens, in the Colonial laboratory. He readily consented, and devoted every week-day from the 21st of March to the 19th of April, to the subject. With the exception of one day, I was present the whole time. The following were the results of the laboratory operations on the post-mortem blood, after following, as far as was practicable, the modes of procedure recommended in Bowman's 'Medical Chemistry,' fourth section, On Blood.

Abstract of Ten Specific Gravity Experiments on Post Mortem Blood of Yellow Fever.

Antonio Fernandcz,	1·067826+	at 89°	George Cripsey . .	1·062865	at 88°
Maria de Jesus .	1·056998+	39°	Francisco Marks . .	1·852576	88°
Edward Richardson	1·0654624	86°	Manuel de Silva . .	1·059498+	86°
George Sacket (boy)	1·04632+	86°	Juan Paul	1·059137	87
Rich. Hanson (boy)	1·040008	88°	Robert Lawrence .	1·0616605	85

Proportion in 1000 *Grains.*

Water in Richardson's blood	796·522	Oily fat in ditto	·76
Dry matter in ditto . . .	203·478	Total fat in ditto	1·79+
Inorganic saline matter (ash) in dry blood	50·95	Water extractions (minus ash)	3·321+
Fibrine (minus ash) . . .	1·780	Alcohol extractions (minus ash)	1·526
Water in Juan Paul's blood	787·385+	Water in crassamentum . .	723·633
Dry matter in ditto . . .	212·478+	Dry matter in ditto . . .	276·367
Inorganic saline matter in dry blood	37·12	Fibrine (minus ash) . . .	8·38
Water of serum in Antonio Fernandez's blood . . .	895·877+	Inorganic saline matter in dry crassamentum . . .	31·185
Dry matter in ditto . . .	104·123+	Crystalline fat in dry crassamentum	2·538
Inorganic saline matter in ditto	8·516+	Oily fat in ditto	3·501
Albumen in ditto	72·432	Total fat in dry crassamentum	6·039
Crystalline fat in ditto . .	1·03	Iron in dry crassamentum .	·567

The first important fact which was incidentally observed in conducting the experiments, was the rapidity with which the blood decomposed after being taken from the vessels of the cadaver. It set rapidly, and within half-an-hour all the healthy physiological changes occurred which were to be expected, and soon after decomposition commences. Frequently no separation at all occurred in the coagulum. The blood was taken from Antonio Fernandez and Maria de Jesus in the afternoon, put into beakers, the ground edges of which were greased, and a plate of glass closely applied over each. These beakers were then put into an ice box till morning. Next day the blood of the female was apparently uncoagulated and somewhat fetid, and in the evening was so offensive, that it had to be thrown away. Immediately on the blood being removed from the body on the previous day it became firm, as well in the beaker as in the specific gravity bottle. Decomposition afterwards, no doubt, caused its fluidity and attenuation. In the case of Antonio Fernandez the clot was large, without buffy coat, with slightly cupped surface, and imperfectly separated from the serum, which was of a dark reddish yellow colour. The odour of the blood was then slightly fetid. The serum was removed by a pipette from two specimens, one to determine the proportion of *albumen* and the other the proportion of *water* in the serum. To verify former observations, 31·605 grains were taken for the application of the bile test. But the serum was not perfectly clear, and our inability to obtain the requisite quantity necessarily caused a deviation from the plan prescribed by Bowman for the analysis of coagulated blood. The following note occurs in our journal of the 9th of April:—

> "It may be necessary to record that, in estimating the *albumen, salts,* and *extractions* of Antonio Fernandez' blood, we could not follow the plan of Bowman in his section on the quantitative analysis of coagulated blood, from the smallness of the quantity of serum obtainable. The albumen was therefore obtained by the method recommended by the same author for obtaining it in diseased blood. The washings are being treated with ether for the fat. But as hydrochloric acid was added to neutralize the alkalinity of the serum, of course no true estimate could be made of the *salts* from that sample. The salts of the serum were therefore estimated from the dry matter obtained in estimating the *water* of the serum. In addition to an estimate of the fat of the serum, we have now under treatment a sample of *dry blood* of Fernandez, to ascertain its proportion of *fat.* We shall proceed afterwards to estimate also the saline constituents of the clot. In reference to the estimate of the *water* in the clot of Fernandez, it may be remarked, that after being weighed it was dried during several days on a chloride of calcium bath, an uniform density of the fluid being maintained by a tube returning the condensed vapour. While drying, the clot was carefully and repeatedly broken up into small fragments to facilitate the escape of vapour, and the drying continued till it ceased to lose weight. It was not minutely pulverized, however, and again dried, as it probably should have been before being finally weighed. No instruction to that effect being given by Bowman, and having ourselves made no analyses of healthy blood whereby a standard might be obtained, we deemed it prudent in this instance not to exceed our instructions, lest comparative results might be affected by a want of parallelism in the methods of procedure."

The present mode of defibrinating the blood is coarse and unsatisfactory, and some new or more chemical method is desirable. While, however, the present methods are pursued, and may be continued for comparative results, the degree of fineness of the muslin bag in which the blood is washed should be stated. In that employed by us in defibrinating the blood of Fernandez, the tissue contained two hundred threads in the superficial square inch. Richardson's blood was attempted to be defibrinated by agitation with pieces of lead; but finding this mode inoperative, and that very little fibrine had attached itself to the lead, we completed the operation by subjecting the blood to a gentle stream of pure rain water in a *calico* bag, which, when wet, was almost air-tight. I subjected the washings in both instances to microscopic examination, after they had remained about sixteen hours to settle. They were then fetid. In Fernandez' I detected numerous fibrillæ and granules among the wreck of the blood cells, and numerous oval and bottle-shaped polygastric animalcules, which moved with great velocity, and were each about four times the size of a blood corpuscle. There were also bodies in the sediment like quartz-shaped uric acid crystals, but on treating them with nitric acid and ammonia, they did not prove to be such. Some of these bodies seemed black to the naked eye, and bluish black by transmitted light, but by reflected light they showed the same quartz-like lustre and appearance. These, likely, had been stained by the dye of a black or dark-blue string, with which the mouth of the muslin bag had been tied. Richardson's blood had coagulated like currant jelly, and the serum had never separated. It was so ammoniacal, that fumes were detected by the muriatic acid rod at the temperature of the atmosphere, without being heated. The washings of his blood yielded considerable sediment. This sediment had a yellowish, curdy, flaky appearance to the naked eye, exactly like the "curdy" urinary sediment which I had observed, and is described before. The sediment showed no fibrillæ, but had a granular appearance, as if the fibrillæ had been disintegrated, or their granular structure disconnected, which may be the mode of the destruction ot fibrine by yellow fever. If so, the amount of fibrine estimated in yellow fever blood by the usual methods must be considered as not the whole fibrinous matter contained in the blood, but only the whole which has escaped disintegration, or which retains its power of fibrillating, and thereby capable of being collected by the present methods. There were some singularities in the case of Antonio Fernandez. He had suffered from an attack of pleuritis extending over about three inches of the left side, coming on during the progress of the yellow fever, and occasioned probably by the situation of his bed, which was in nearly a thorough draught. He also had the scarlatinoid rash more markedly and more extensively than I ever saw before, and it was accompanied by a turgescence of the skin, which subsided on the advent of black vomit. As the black vomit was not very copious, and lasted only one day, and although he lost a considerable quantity of blood by urine, as he had suffered from an inflammatory complication, I naturally expected to find his blood rich in fibrine, and but little deficient in its saline constituents. It may be here noticed, that in obtaining the specimens for examination from the large vessels of the chest, care was taken that no fluid from the pleura, mediastinum, or pericardium, should be mixed with the blood. Some effusion had taken place as a consequence of pleuritis in the case of Fernandez. In washing out the fat from the albumen by boiling ether, the filters on the second day refused to act, probably from an obstruction of their pores, and steam-washing was had recourse to during two successive days, until no trace of organic or saline matter passed through, the criterion of which was the stainless evaporation of a drop of the filtered fluid on a piece of glass heated over the spirit lamp. It may be remarked that this criterion would not be applicable to filter-washing with *commercial* ether, which leaves a stain of itself, from probably an impure spirit (containing fusil oil) being frequently used in its manufacture. Without attention to this fact, the ether filtration may go on to an indefinite period—for the stain on the glass will be well marked, and will *volatilize before being charred*, leading to the inference that the fat is not yet completely separated from the albumen, which may or may not be correct. The alcohol used for the *extractions* was of the strength of 45° of Baumé, and four washings were applied.

On the 13th of April, an estimate of the *iron* in the inorganic matter (ash) of the *dry blood* of Fernandez, which had been incinerated the previous day, was commenced. As this analysis was independent of the instructions contained in Bowman's 'Medical Chemistry,' it will be right to describe the method pursued:—To the ash was added an excess of muriatic acid, and then it was digested in a bath till solution was effected, and a few drops of nitric acid were added to peroxidise the iron. It was afterwards filtered to free it from some particles of impurity, and treated with ammonia for precipitation, and allowed to remain till next morning. The ammoniacal precipitate of iron was then steam-washed till only a trace of stain could be detected on evaporating a drop of the filtered liquid on a piece of glass. The filter (No. 5) and its contents were then dried in another filter paper, so as to be removed into an agate mortar, when it was mixed with an excess of *pure* carbonate of soda. The filter paper was then ignited and placed in a counterpoised platina crucible, and further incinerated over a spirit-lamp, and then mixed with an excess of *pure* carbonate of soda, and added to the other portion so treated. The whole was then fused, and after fusion lixiviated with distilled water and left till next day. On the following day, after maceration, filtration, and steam-washing on filter paper of known weight, the precipitate and paper were dried and weighed in a counterpoised tube. The net weight of the iron was found to be ·23 of a grain. The great care in removing the phosphates may show a weight of iron comparatively light. It may be remarked, that throughout the analysis of Fernandez' blood, the phosphates caused much trouble and delay. A crucible could never be used a second time after simple washing, or the use of an acid. Fusion by microcosmic salt, or other flux, was required after each incineration. Notwithstanding this strong crusting of the crucibles, the ash of the several incinerations was very deliquescent, no doubt from the presence of potash. After deliquescence spangles were seen over the surface. On examining one sample of residuary ash on the last day of operations with a one-inch object-glass, I found that these spangles were long flat prisms, associated with amorphous opaque crystalline matter, tinted brown, probably from the presence of iron (it was the sample from the albumen washing). The whole of the prisms rapidly disappeared on the addition of nitric acid, and a great part of the amorphous matter. An accident happened to the specimen, when ammonia was added and the glass put in the sunshine. So I cannot say that the crystals were restored.

It is probable, however, that the prisms were triple phosphates, and the amorphous matter phosphate of lime. The estimate of *water* in Richardson's blood appears high. This may be in part owing to the extreme care with which the drying process was conducted. A water bath was used in the first instance, and the mass was then pulverized, and the drying finished off with a heat of from 230° to 240° in a chloride of calcium bath.

In extended investigation on the specific gravity of yellow fever blood, a law of age and sex might be evolved. It may be noticed in the instances already given, that the lowest gravities are in the boy and woman. Other circumstances being equal, the copious ejection of black-vomit should increase the density of the blood by diminishing its proportion of water, and this view would seem to be borne out by the converse fact that Francesco Mark, who died without black vomit, has blood of the lowest density of all the adult males; and Juan Paul, who had but little black vomit before death, is the next lowest in the same category. In Francesco Mark's case, the specific gravity bottle was filled seven and a half hours after death, and the blood set firmly in it. Robert Laurence was fifty years of age; the specific gravity bottle was filled twelve hours after death, and was weighed two hours afterwards. The blood had then set firmly without separation of serum. He had had copious black vomit before death. In George Crispy's case, the bottle was filled eight hours after death, and the blood set well in it. A quantity was also placed in a small beaker, and twenty-four hours afterwards a soft coagulum filled the whole space occupied by the blood, without separation at the sides. But there was on the upper surface about half-an-ounce of very dark serum, with floating pellicles. This serum may have constituted about $\frac{1}{10}$th of the whole. The blood smelled decidedly *urinous*. On examining it with Pritchard's $\frac{1}{4}$th or $\frac{1}{3}$rd inch object glass (for the power is not stamped on the case) the pellicle seemed to consist of granules, such as are seen in the curdy sediment of albuminous urine. There also appeared what seemed to be oil globules, and other bodies which were not round, but somewhat prismatic in shape and luminous in centre, such as are delineated in drawings of hippuric acid. As regards P. Burke, on the 12th of April, 1853, I tried to take the specific gravity of his blood two hours after death, but it coagulated so rapidly that it could not be got into the bottle in a homogeneous state, and there was an excess of serum introduced. It was weighed, notwithstanding, and its specific gravity in the condition stated, was 1·04508, at a temperature of 87°. In the bottle the serum separated clearly and in large quantity, and was as usual alkaline. He had had copious black vomit before death. Some of his blood was also set up in a beaker, and after twenty-four hours inspected. It then had the clot and serum separate; but the clot was very weak and the serum of a bloody orange colour and turbid, from bile and the hæmatosine of ruptured cells. No fetor was observable. On the 9th of April I wished to take the specific gravity of the blood of Charles Bush, who died in the Seaman's Hospital on the previous day, in order to compare it with that of Francesco Mark, the former having had copious black vomit before death. But he had already been dead twenty-four hours; and very soon after the blood was put into the bottle it began to bubble with gas, and actually frothed soon after. When weighed while the bubbles of gas were rising, its specific gravity was still 1·050, and in this state of decomposition it was very fluid, and readily flowed out of the bottle when inverted.

Numerous quantitative analyses of the blood of yellow fever would obviously be of the greatest importance to the elucidation of the pathology of the disease. But such an undertaking is impracticable; though a working chemist, associated with an intelligent physician, might well occupy five or six years in such pursuits to the great advantage of the whole tropical world. In the meantime much valuable information might be added by the zealous practitioner to the general stock, by multiplying accurate observations of the blood's density, and its alkalimetry according to Griffin's method; these, with a table of some of the most important symptoms, and the age and sex, &c., of each patient, attached, would yield excellent results.

CHAPTER VIII.

Immediately after becoming acquainted with the discovery of Frereichs, regarding the conversion of urea, its applicability to our yellow fever investigation was at once apparent, and I forthwith proceeded to the Seaman's Hospital, with the view of ascertaining if the *breath* of the patients in the advanced stages would give any signs of the presence of ammonia. The attempt failed on that occasion to afford any indications; and a note was not even made of the experiment as to the date or result. The impression for the time was fixed, that the urea necessarily in the blood in cases with suppression of the function of the kidney remained unchanged. Up to this period the alkalinity of the blood, which had been frequently observed, was supposed to be that of its normal condition, and our object of research was chiefly the recognition of its departure from this state—its acidity. The examination of the blood, and the detection of ammonia in it, in the case of Lannan, renewed our attention to Frereichs' theory, and we argued that the carbonate of ammonia might be formed, but concealed by a nascent combination with some acid. The next experiments were entirely successful.

The following is my note of it:—

"*Carbonate of Ammonia detected by me to-day in the breath of Stress and Whittaker, both of the Seaman's Hospital; yellow fever cases.*—I found that Stress had passed about one ounce of urine to-day, which was rather mucous, with a sediment of epithelium tube casts, and several *crystals of triple phosphates.* It was highly coagulable. After allowing the coagulum to subside and separate, I took a watch-glass, and reduced the clear urine slowly by the heat of a spirit-lamp to one-third its bulk. I then added an equal quantity of nitric acid, and placed the watch-glass on a piece of ice. The experiment was carefully repeated thrice. A mere trace of nitrate of urea was obtained. At first it was supposed to be entirely absent, and it is so entered in the case-book; but on adding alcohol, and then evaporating a drop on a piece of glass, traces of the nitrate of urea were observed by the microscope. The case of Stress at the time of observation was that of incipient black vomit. He was distressed, breathing quickly, and somewhat nasal. I moistened a neutral test-paper (Griffin's), and held it to his breath for about half a minute, when it became blue. I then held a glass rod, dipped in muriatic acid close to his mouth, when white fumes were visible. Whittaker's case was more advanced. He had intense black vomit. Eye yellow; respiration nasal; inclination to restless torpor. No urine to examine. The instant he breathed on the wet test-paper, it became blue, and muriatic acid showed white fumes. I prescribed lemonade *ad libitum*, with a little sugar, and immersion in an acetic acid tepid bath, of the strength of about one-sixteenth of vinegar to the whole bath. 26th Sept., 1852."

After this, the investigation was quickly followed up, and it became apparent that the urea of the suppressed urine is eliminated from the system as a volatile salt by its metamorphosis into a carbonate of ammonia, which, as such, is frequently found in the breath, occasionally in the black vomit and hæmatemesis, and almost always in the stool, twice in the urine (Ellwood and Macey), always in normal black vomit in combination with an acid; and, indeed, apparently pervading all the tissues of the body.

We have made many attempts to detect urea and uric acid in the circulation and in the serum of the ventricle of the brain, but uniformly without success. This certainly may have arisen from the incompetency of the operators, and subsequent manipulation may accomplish it. Still, it seems probable that the mode by which an attempt is made by nature to unload the system of the urea when its natural channel is obstructed in yellow fever, is, failing a restoration of the function of the kidneys, the conversion of this substance into ammonia, which is eliminated in the manner before described. The uremic condition of the blood seems a fugitive affection. In the case of Flynn, before referred to, it seemed to have passed off before death. The blood was only slightly alkaline, although the breath, when examined, was highly ammoniacal. But by the breath, black vomit, and a partial restoration of the function of the kidney (he passed on the day of his death eight ounces of urine, specific gravity 1·033, at 80°), the circulation had been relieved. Such was the case also in George Balls. His blood was tested for ammonia, and found free of it after death, although during life the breath had been highly ammoniacal. Josea Joachim, before referred to, on account of being a subject wherein the peculiar black vomit appeared, and whose breath and vomit showed a saturation of the system with ammonia during life, had large fibrinous coagulæ in his heart after death, and no *free* ammonia in his blood. The blood was acid, and ammoniacal vapours were produced only after the addition of *liquor potassæ*. In this case the system seemed relieved of its urea by a tertiary combination—the formation of a neutral salt by the ammonia with an acid (phosphoric?). Sometimes, where you have suppression of urine and symptoms of uremic poisoning, you may find little or no alkalinity of breath, as in the cases of Clarke (Seaman's Hospital, 19th of October, 1852) and Walker (Seaman's Hospital, 28th of November, 1852). In some of these instances I can suggest no explanation but that urea of itself is adequate to all the phenomena, as originally supposed, or that sometimes the feebleness and *shallowness* of the expiratory movement, when the lungs are undergoing engorgement, and the diffusion of the exhaled air through both mouth and nostrils, may prevent the test-paper or glass rod from being affected. I have been in the habit, from the latter consideration, of always closing both nostrils with the finger and thumb, and permitting the patient to breathe through the mouth only while the test is being applied. The degree of alkalinity may be roughly estimated in these experiments by the number of expirations required to strike a distinct blue on the moistened end of the test-paper. Uræmic symptoms are most severely and distinctly brought out when the urine is suppressed after black vomit, has commenced copiously and afterwards ceased, and the mucous diarrhœa also ceased, as in the cases of Mr. Glynn (at Mr. S.'s), Mr. B. J., the cook of the *Susan* (Seaman's Hospital, 5th of February, 1852) and a mate about the same time. Then, instead of the placidity of mind and freedom from suffering for which the fatal termination of yellow fever is often so remarkable, the whole train of manifestation is changed. The pulse, instead of its usual loss of power and threadiness, revives or remains full and strong, the pupils become contracted, the eye sometimes again assumes a glistening appearance, and delirium, convulsions, fearful shrieks, and stertorous breathing may close the dreadful scene. Of course this condition varies in degree, sometimes amounting only to uremic intoxication, in ratio with the extent of the locked up secretions. In the protracted case of the mate of the *Sabraon* (private practice; black vomit on the eleventh day, and death on the thirteenth) the uremic symptoms appeared on two occasions. At first they occurred before the black vomit. They were marked by apathy and despondence, to which succeeded low muttering delirium and *subsultus tendinum*. This condition lasted two days, after which black vomit ensued in immense quantities, and *forthwith the intellect became perfectly clear*, and the subsultus much diminished. But an incessant hiccup supervened. The vomit changed into a port wine-like fluid, at first a little acid, but in a few hours alkaline, and it emitted ammonia on the application of heat without *liquor potassæ*. Under the microscope not a single entire corpuscle was to be seen, but much *débris* of blood-cells. The urine had been tolerably free throughout this protracted case. On the day before his death he passed twelve ounces. It was highly acid—rather turbid, with a thin layer of blood corpuscles, and with a very few amorphous and not large masses of the material of tube casts. Its specific gravity at 86° was 1·018. It was highly albuminous. After being heated to evaporate the albumen, it was then carefully tried for urea, and the evaporation conducted very gently. It formed no crystals of the nitrate of urea. But a trace was discovered in the mucous-looking ring, at the edge of the watch glass, by alcohol, under the microscope. It was tried thrice with the same result. Twenty-four hours before death the urine became suppressed; vomiting also ceased. He then became exceedingly restless; much jactitation, intolerable sense of internal heat complained of, beginning in the throat and epigastrium, and subsequently extending to the feet and hands, while the surface is actually cool. Refuses everything. Tongue, that was yesterday clean, now dryish and incrusted with blood. Delirium and coma closed the fatal scene at midnight, 23rd of January, 1853. This modification of the uræmic symptoms in the first instance may have arisen from a moral cause. This patient was doing well, and apparently convalescent, after having had black vomit *incipiens* on the fourth or fifth day. But the master of the vessel visited him, and rudely reproached him for the expense he put the vessel to, by his lying up in private lodgings, instead of having gone to the hospital. He visibly took these taunts to heart, and the unfavourable and fatal symptoms commenced directly.

In general, the yellow fever cadaver remains in a suitable condition for dissection as long as that of any other disease. But when the urea has not got vent during life, and the putrescent elements are retained, decomposition is rapid. A case in point was that of Tomlinson (Seaman's Hospital, 9th of October, 1852). The following is my note:

"*Singularly Rapid Decomposition after Death in the case of Tomlinson.*—Yesterday at noon he had no appearance of illness; skin was cool, pulse little excited, although in the third stage of yellow fever. There was, however, an almost total suppression of urine, and his breath was highly alkaline. He died yesterday evening, and this morning the corpse had the appearance of having weltered in the sun many days. It was black, enormously distended, and covered with large vesications. It was in an unapproachable state for dissection. He had had no well-defined black vomit. There had been some white vomit, and about four ounces of imperfectly-developed black vomit altogether; and only one scanty sanious stool of about three ounces. I examined three ounces of urine, which had passed yesterday at two P.M. The coagulum, when heated, did not readily subside, and amounted to about one-fifth of the whole. When tried for urea, no crystals formed. A slight haze, however, formed on the surface, which, when treated with alcohol, showed traces of the nitrate under the microscope. The urine was strongly acid;—9th October, 1852."

In some moribund cases I have observed strong alkalinity of breath, while acid beads of perspiration stood on the face and forehead, which when evaporated and examined microscopically showed, after evaporation to dryness, dagger-shaped and cross-slit crystals, as in the case of Cook (Seaman's Hospital, 9th of December, 1852). But uræmic poisoning is not the only mode of death (see cases, Patterson, Seaman's Hospital, 9th of July, 1853, and Antonio Gonsalvo, Colonial Hospital, same date); nor are the cerebral symptoms always due to that cause. Hyperæmia is capable of inducing similar symptoms, as in the case of Laird (Seaman's Hospital, 16th of October, 1852), in which, indeed, both classes of symptoms were present, but the latter antecedent as to time.

Respiration in the last stage of yellow fever is sometimes very laborious, and frequently at each inspiration the nostrils collapse and shut, and if the half-comatose patient keeps the mouth shut (as in the case of Juan Martinez, Colonial Hospital, 13th of June, 1852), asphyxia may ensue therefrom. It is obviously difficult in many of these cases to refer the symptom to its true cause—to distinguish the effects of a poisonous circulation on the brain generally from those of direct pressure on the medulla oblongata. There is a description of the respiratory act, named, I believe, by Dr. Graves, "cerebral respiration." This epithet frequently occurs in our case books. In many of the cases to which it is applied, the intelligence is not much, if at all, impaired; and the name of suspiration might perhaps judiciously be substituted for that of cerebral respiration. It is a hurried sighing respiration, in which the nostrils also take part; it is frequently accompanied by restlessness and jactitation. In the generality of cases, this state is really independent of all nervous influence, as far as any affection of the body can be, and is the direct effect of congestion of the lungs, threatening impending pulmonary apoplexy.

(*To be continued.*)

CHAPTER IX.

In general, after convalescence from yellow fever, the recovery to perfect health is rapid and thorough. I have notes, however, of five seamen who before discharge from hospital suffered from paroxysms of intermittent fever, in the months of July and August, 1852. In the Colonial branch, at various times, parties discharged cured of yellow fever have returned within from ten to fifty days, suffering from the endemic intermittent. These were chiefly Portuguese immigrants, who are very prone to this disease at all seasons, and during non-epidemic periods. These sequelæ, however, were more common in the months of July and August than in other months. I have a note of only one case in private practice (that of Mrs. M.), where intermittent fever appeared clearly as a sequel of yellow fever. Bloody furuncles, as before noticed, are so close on the primary affection, and so obviously one of its morbid processes, and so frequently co-existent with black vomit (in the case of Mr. L. M., a white native, who died on the third day of illness with black vomit, a malignant-looking furuncle appeared on the upper lip), that it is doubtful if it should be rated as a sequela only. But they frequently appear during apparent convalescence, and of course retard it, and become associated with abscess and ulcers, which readily heal. The parotid gland suppurated in four cases of Portuguese, as the result of a bloody furuncle in each; and in one sailor, Devine (Seaman's Hospital, 17th of November, 1852), there was one instance of a bubo over Poupart's ligament as a sequela. In convalescence, also, small boils frequently appear over the face and other parts of the body, but it is difficult to ascertain whether these should be referred to the previous disease or the irritation which follows the application of vesicatories, and is observed so often as their effect when applied in other affections. Gangrene of the prepuce occurred in the case of Ernest Home (Seaman's Hospital), who was suffering from gonorrhœa before and during the attack of yellow fever. Anasarca of face, hands, and feet, without desquamation of cuticle, while the skin was still yellow, appeared in the case of Grammage (Seaman's Hospital, 2nd of October, 1852). In the case of Mr. Mackinnon, of the *Jane Brown*, urticaria came on while the skin was still very yellow, accompanied with abdominal pains. Then a recession of the rash took place, and dementia (preceded by oscillatory movements of the eyes), quadruple vision, and death. Oozing from the gums was frequent in convalescence from the "smouldering" forms of the disease. Retention of urine occurred in the case of Anderson (the Swede). One seaman was readmitted to hospital on account of debility, after his attack. Where the attack had been severe and profound, wasting of the body was sometimes found to have taken place, as (markedly) in the case of the master of the brig *Speculation*, who had had black vomit before recovery; and Peter Daly, Major, and Anderson, before referred to. When veneesction had been used in treatment (as in the case of Havish, Seaman's Hospital, 16th of January, 1853), convalescence was much protracted. Bright's disease was a sequel in the instance already mentioned. The patient, Manuel D'Alvia was admitted to the Colonial Hospital on the 5th of April, 1853, with a violent attack of the epidemic, from which he recovered. He was discharged on the 16th of July, at his own urgent request, with his urine still albuminous. He had been cupped for the sequela over the kidneys: had issues then applied: had taken a long course of gallic acid, but without benefit. His ailment somewhat emaciated and anæmiated him, and gave to his countenance an expression of gravity; yet there was no œdema or dropsy, and his appetite was tolerably good. As he felt well, he could not understand why he should remain in hospital. The most singular sequela of yellow fever, if it be really one, was that in the case of Miss G., a subject of one of the anomalous cases of black vomit before referred to. I saw her in consultation about five months subsequent to the period when I had seen her in the primary affection. She suffered from a compound of anæsthesia, pain, and atrophy of the left hand and fore-arm. It began about two months before with numbness, and afterwards with some peculiar occasional pains; but no physical alteration was noticed, and she was supposed to be fanciful. Since then, the symptoms have much increased, with both numbness and tenderness on friction, chiefly along the course of the ulnar nerve. The fore-arm is much wasted, but the startling atrophy is in the fingers. Motion is perfect; there is no breach of surface; but the fingers have an attenuated ivory appearance. She has not been using her left arm, or but rarely, for some time, even before she began to complain. But the wasting is not muscular only. It seems as if all the tissues, and even bones, had wasted symmetrically. She had never been quite well since the black vomit, and has had several attacks of intermittent since then, and since the commencement of the present affection. Chalybeates and galvanism were recommended, and change of climate, which being adopted, the case has been lost sight of, and the result unknown. Two cases of abscess of the liver followed as sequela of yellow fever; one in a Portuguese man, Josia Joachim, admitted to Colonial Hospital, 24th of February, and discharged 28th of May, 1852. In this case the liver was twice opened, and an enormous discharge of purulent matter let out. The other case was in a negro girl, Lucy, a native of Barbadoes, once punctured, and cured. Inflammation, acute pain, and swelling of the joints occur sometimes in convalescence, when the action of the poison has been profound; but though often threatening suppuration, these painful swellings have always terminated by resolution. One of the most common sequelæ is jaundice, and this greatly retards the period of perfect recovery. Most of the cases that remain long in hospital after convalescence, and of which period no report is made in the case books, have been detained by this affection. It is a true sequela, and not to be confounded with the lemon tinge and orange eye which are present in the advanced stages of the disease. As has already been noticed, in this sequela the eye is smooth and unvascular, and the skin yellow or tawny, the urine is copious, and loaded with bile, and the fæces are formed, grey, and abilious. The one state seems the result of *excitement* of the liver, and the other of *obstruction* of the bile-ducts. One of the most uncommon ailments of females, colica pictonum, attacked Mrs. B. in early convalescence from an attack of the prevailing disease, which had proceeded to the stage of acid elimination, on exposure to the atmosphere of a lately painted room.

Relapses were of frequent occurrence, occasioned most likely, in great measure, by a return of the patient to the focus of infection after discharge from the hospital. These relapses were almost exclusively among the aborted cases. They frequently recurred, and were aborted several times. The primary attack was generally without albuminosity of urine, and frequently the relapse also, as in the captain of the *Undine* (private practice), and Thomas Wright (Seaman's Hospital, 17th of June, 1852), &c. &c. But in the relapse there was often an accumulated power in the disease, and albuminous urine was expected, even if the disease were again aborted, during convalescence. Relapses, however, occur in which, and in the primary attack, the urine was albuminous, as with R. Fuyakerly (Seaman's Hospital, 25th of July, 1852), &c., in whom the relapse was easiest of abortion; and in George Macey (Seaman's Hospital, 12th of February, 1853), and Peter Francis (Seaman's Hospital, 5th of March, 1853), in whom the relapse was fatal. These, however, were comparatively rare, and we have had only two relapses after the disease had run on to black vomit—viz., those of Anderson (Seaman's Hospital, 16th of February, 1853), and Adam Smith (Seaman's Hospital, 31st of December, 1852), both of which were readily aborted, although the last-mentioned relapse proceeded to albuminous urine. Duncan Livingston (Seaman's Hospital, 3rd of August, 1852) sustained a relapse or second attack. His first was on the previous 12th of July; and though on that occasion his urine was far from albuminous, the eye was tinged. Daniel Clarke (Seaman's Hospital, 29th of June, 1852), was then in hospital for a relapse, and stated that he was very ill ten or eleven years ago, in Demerara, with yellow fever. On the 23rd of August, 1852, the steward of the *Maria* was admitted to hospital with an attack of the prevailing epidemic, which was aborted by two doses. He stated, that four years ago he was very ill at Vera Cruz with yellow fever, and suffered afterwards at the same place with intermittent fever. Relapses were more numerous than appear in the case books, when they occurred in hospital. Such were promptly and extemporaneously prescribed for; and if they were aborted by the first dose, as they frequently were, no report was made of them. The tendency to relapse or second attack was generally within the first month after the primary attack. Master J. B., and Mr. M. C. (of the house of Irvine and Sons) had each a second attack exactly one year after the first, both recovering. The primary attack in the former having been severe, and the second mild, exactly the reverse of what happened with the last-named patient. A case of yellow fever, alternating with intermittent, and ending fatally, occurred in the Colonial Hospital. Manuel de Frytas, only three months in the colony, had several attacks of intermittent, one of which was on the 27th of October, 1852, but on the 10th of November following he was admitted for an attack of yellow fever, which proceeded on to albuminous urine and scrotal excoriations. He was discharged on the 28th of November, cured. On the 7th of December he was re-admitted for intermittent fever, and cured; and on the 4th of January following he was again admitted with yellow fever, urine highly albuminous on that day, and he died suddenly on the 6th of January. There was no post-mortem examination. Relapses, then, were frequent after aborted attacks, but very rare after the disease ran to its second stage, whether it stopped with the first stage of acid elimination or proceeded to black vomit.

When the epidemic has terminated, and the harvest of facts are gathered to their granary, then, by the application of the *numerical method* to this and other branches of the subject, the vague terms "frequent," and "seldom," may be dropped, and the ratio of frequency of the several symptoms can be stated with precision. The present estimates are rather qualitative than quantitative.

CHAPTER X.

The mode of death in uncomplicated yellow fever has four distinct varieties, and these are sometimes blended—viz., syncope, uræmia, apoplexy, and asphyxia. When the black vomit is plentiful or the urine free, the intelligence remains clear and unclouded; but the skin becomes cold and damp; the pulse small, and, finally, extinct at the wrist, and the patient dies of gradual exhaustion and syncope. Lamont (Seaman's Hospital, 5th of December, 1852) died apparently from rapid collapse, following excessive discharges of black vomit. The description of the mode of death by uræmia has already been in great measure anticipated in the foregoing chapters. If before death the urine be suppressed, and the black vomit is not copious, or has ceased, the circulation becomes contaminated; and when this condition operates on the brain in its mildest form, the effect is not unlike alcoholic inebriation; as in the case of the master of the *Hindu*, who, on the night of his death, sat up in bed, drank beverages, and joked with the ship-masters around him; and the carpenter of the *Eleanor*, who, within a few hours of his death, and while pulseless, I found, on my visit, sitting up in his chair, and regaling himself with his tobacco-pipe. If all the excretions and secretions be locked up, as occasionally happens (the master of the *Honor*, for instance), the symptoms of uræmic poisoning become violent, the sensorium painfully affected, irritability of temper, screams and wild ravings, coma and convulsions, ensue. Death from syncope does not arise from excessive discharges of black vomit alone. It is often the result of hæmorrhage, as in the case of the uncontrollable epistaxis in W. Smith (Seaman's Hospital, 25th of March, 1852); or bleeding from the mouth and gums, as in the case of Ferguson (Seaman's Hospital, 2nd of November, 1852). Frequently these two causes—i.e., black vomit and hæmorrhage—combine in inducing this mode of death, as in the case of Mrs. W. The following extract note, written on the day of her death, illustrates this point:

"Before black vomit appeared, the catamenia came on prematurely, the bowels became spontaneously relaxed; and last night there was much flatulent purging of blood, and a considerable hæmorrhage from vagina. After a cessation of twenty hours, black vomit again returned to-day. After total suppression for twenty-four hours, four ounces of alkaline urine was drawn off by catheter. On my visit at daylight this morning, she was quiet, and apparently suffering no pain, and rather apathetic. The marked change which I found in her case, was a deterioration of the pulse in volume. The nervous symptoms of the preceding day, which threatened inebriation or convulsions, had disappeared. The pulse became gradually weaker, until about eleven A.M., when it could not be felt. She was aware of her hopeless condition, and tranquilly disposed of her trinkets to her friends and relations. As she approached her end, the breathing became quicker and shorter, until it ceased in a few little gasps at long intervals. About half an hour before she died, she apparently lost her vision, then her hearing, and sensation, first of the mouth and nose, and then of the arm, in quick succession, and in the order stated. It was an appalling scene, to see her lying silently on her back, and trying to *rub back* vision and hearing and feeling, with her hands. She spoke not a word during the time; but it was evident that the senses were all being blotted out one by one while consciousness yet remained. Before death, at two P.M., one or two slight convulsive jerks of the shoulders were the last respiratory efforts.—17th of February, 1853."

After death in this case, a large quantity of black vomit escaped from the mouth in turning the body. The mode of death by apoplexy, caused by congestion, and effusion and extravasation of blood on the brain, is instanced in the Seaman's Hospital cases of R. Williams (3rd of March, 1852), Peter M'Guire (13th of November, 1852), and Peter Thomas (17th of December, 1852). The following Seaman's Hospital cases furnish instances of death by asphyxia:—Moses Dillon (25th of July, 1852), laryngeal suffocation; the Portuguese sailor (31st of August, 1852), and Milligan (14th of December, 1852), from pulmonary apoplexy. Cases sometimes terminate suddenly, as if by *explosion*. Thus, Peter Scott (Seaman's Hospital) was doing well. On the 1st of November, 1852, at noon, he suddenly became ill, vomited black vomit, and died within a few hours, with alkaline breath. Patterson, who died on the 18th of February, 1853, had his stomach perfectly quiet till within a few hours of his death, when he suddenly disengaged immense quantities of black vomit. His urine had been tolerably free up to a short time previously. Alexander Stewart, who died on the 14th of February, 1853, in Seaman's Hospital, is another instance. He became suddenly ill at four P.M. on his fifth day, and died at six P.M.

The causes that disturb the current course of the morbid phenomena have not yet been satisfactorily investigated. They may be due, perhaps, to sudden formation of some poisonous compound in the blood, or the hæmorrhagic yielding of the bloodvessels. Some modes of death may be purely accidental: thus Juan de Susa died from rupture of the spleen, caused by jumping out of the window while delirious. The essential modes of death are modified by those inflammatory complications to which yellow fever is so liable in its course. Thus, M'Kechnie (Seaman's Hospital, 30th of January, 1853) became delirious from evidently neither uræmia nor hyperæmia, but sympathetic suffering arising from pericarditis. Milne's (Seaman's Hospital, 28th of February, 1853) symptoms were modified by an atrophied heart. Devine (Seaman's Hospital, 17th of November, 1852) died with gangrenous lymphatitis; and Savage (17th of November, 1852) died from the shock and pain of acute lymphatitis. The hospital case books are perhaps more deficient in illustration of the mode of death, than of any other of the phenomena of yellow fever. If the death did not occur about the hour at which the reports were written, the final symptoms were seldom described, as, except in extraordinary cases, a single daily report only was made. This arose from the inadequacy of the staff of resident surgeons—the reporters—for the wants of the epidemic period.

There are no sufficient materials to authorize an estimate of the natural mortality of the present epidemic. Many untreated cases were brought into both hospitals, as the case from the *Rowley* (Seaman's Hospital, 8th of August, 1852), or Francesco Pisthano (Colonial Hospital, 7th of May, 1853), presenting all the well-marked characteristics of the disease; and were, I believe, uniformly fatal. But those untreated cases which recovered, if any, would not of course present themselves at the hospital; hence no comparison can be instituted. It would be a most desirable consummation to ascertain the law of mortality of this disease, as a base line by which results of treatment might be compared; and if some honest homœopath, careful in his diagnosis, would tabulate the results of his practice, he would confer an immense benefit on the science of medicine, by determining the true ratio of mortality of untreated cases. I am in possession of only two well-authenticated narratives which can throw any light on this subject, and they refer to the mortality occurring on board of two vessels after their departure from our harbour. The note which records the case of the brig *Atalanta*, I herewith copy in full, although it contains matters not strictly relevant to this branch of the subject.

"This vessel arrived on the 1st of July. She took up her moorings off 'Holmes Stilling,' nearly in the same place where she lay in January (her last voyage), and in which she was perfectly healthy. Her crew consisted of eight hands and the master; total, nine. On the 19th, two men sickened, and were sent to the Seaman's Hospital. Both cases were very violent, and one died. On the 21st, the vessel sailed up the Demerara river, to take in a cargo of wood at Berlyn, about seventy miles up from town. While proceeding up the river on the evening of the 21st, the second mate sickened. He had refused his supper the evening before. The captain gave him 10 × 12 grains of calomel and quinine, and repeated the dose in three hours, and afterwards gave him castor oil; after which he perfectly recovered, and subsequently was left alone in the vessel to take charge of her. On the 23rd, the first mate and the steward and a boy sickened, and the captain repeated on them his former prescription; but the dose was instantly vomited, and not again repeated. On the 26th, the captain finding that the men were still ill, sent them carefully down to town, in the long boat, to be sent to the hospital or to sick lodgings. They did not reach town till the morning of the 28th, before which all three had died with black vomit. On the 27th, the captain fell sick, and one man, but were quickly pulled down to town in the small boat, and arrived before the long boat, and they went to sick lodgings. After this, the last remaining unaffected man sickened, and was brought to town, and died on the 1st of August. The importance, I conceive, of this instance, is in affording some information relative to the value of treatment, and the probable mortality of untreated cases. They were all gravior cases. The three men who got each the dose which was instantly rejected, may be considered as untreated cases, and all died. Of the two admitted to the hospital, one, or fifty per cent., died. Of the four treated in private lodgings, on the same plan of therapeutics as is pursued in the hospital, one died—the last arrival. The captain states that his former crew had been trading here for three years previously; but that the present crew had not before been in the tropics. This, however, I think will not account for the exemption in the first instance, and the excessive malignity in the present. The lines of infection often shift, and the malaria occasionally operates in vortices; for sometimes one vessel will be suffering from fearful sickness and mortality, while another in the neighbourhood, perhaps not two cables' length off, is exempt. The case of the *Atalanta* contrasts strongly with that of the *Camillus*. In February last, the last-named vessel lost five or six of her crew. The sickness commenced on her tenth lay day. She returned here from London in the latter end of June, took up her station in the same place (furthest tier out in the river, opposite Johnston and Bros Stelling), and on the *same lay day* the yellow fever again broke out. I advised her being unmoored immediately, and anchored further up the river, which was done, and she has suffered much less this voyage. The *Camillus* seemed to lie in the wind-line of *permanent* infection, blowing from off the Blissingen sluice and the slaughter-house. The unfortunate *Bilair* and the *Honor* lay in the same line.—7th August, 1852."

The brig *Sarah*, of North America, after remaining a few days in harbour, proceeded on to Surinam. She left our port towards the end of July, and after rather a tedious voyage, arrived in Surinam early in August, where she was put in quarantine, and taken in charge of the American consul, till he could send to the United States for a crew—the *captain and all the white crew having died* on the passage from Demerara. This intelligence reached Demerara on the 9th of August, and was published in the local newspapers. The vessel had been piloted from Demerara by a Mr. de Vivre, who on his return gave me information substantially the same as that published. The following is my note of it, made at the time:

"To-day, met Captain de Vivre, who returned from Surinam ten days ago, leaving the *Sarah* still there, under the charge of the American consul. The *Sarah* left this port on Friday evening, with nine of a crew—viz., the master and two mates (*white*), a light-coloured man, three negroes, all natives of North America, and two negroes, natives of Hayti, or St. Domingo. There were also three passengers, two of them females, belonging to Demerara, and a gentleman of Surinam; also Captain de Vivre, who acted as pilot (white creole of St. Eustatia; family several generations in the West Indies, and he many years resident in Demerara), and a negro (?) boy, whom he took with him. One of the crew, a St. Domingian, had been to the Seaman's Hospital for some trifling ailment. *All the crew fell sick the same night* on which they sailed hence. The master, two mates, and light-coloured man died on the following Friday, all within eight hours of each other; all with black vomit, but the master with convulsions. The three North American negroes seemed at one time very ill, but recovered, with the St. Domingians. Nobody else was in the slightest degree affected.—6th October, 1852."

The impression is general throughout the colony that the present epidemic is much more intense than was the preceding; and this opinion is countenanced by the fact that several fatal cases have occurred among the white creole population. As far, however, as documentary evidence goes on the subject, a parallel cannot yet be drawn between the past and the present epidemics; for on the former occasion, eighteen months elapsed before the Seaman's Hospital was established, and trustworthy and extensive records kept; and as the beginning of an epidemic is generally its most virulent period, a comparison with the present is not yet admissible. The following table of thirteen months' admissions and deaths is given, with the explanation, that in the public hospitals of Demerara and Essequibo, in both its main departments, no patient, however ill, is refused admission, *if alive* when the conveyance is sent for him, or when brought to the hospital: that in the colonial department many cases are admitted moribund; that in the selection of cases, when the hospitals are crowded, the preference is always given to the gravior case; and that the hospitals are very frequently made the receptacle of the hopeless cases of private practice.

RETURN

Showing the Number of Admissions, Discharges, and Deaths of Yellow Fever in the Colonial and Seaman's Hospitals, from December 1st, 1851, *to January 31st,* 1853.

	1851.				1852.				1852.				1852.				1852.				1852.				1852.				1852.				1852.				1852.				1852.				1852.				1852.				1853.			
	Dec.				Jan.				Feb.				March.				April.				May.				June.				July.				Aug.				Sept.				Oct.				Nov.				Dec.				Jan.			
	Admitted.	Died.	Cured.	Remaining.	Admitted.	Died.	Cured.	Remaining.	Admitted.	Died.	Cured.	Remaining.	Admitted.	Died.	Cured.	Remaining.	Admitted.	Died.	Cured.	Remaining.	Admitted.	Died.	Cured.	Remaining.	Admitted.	Died.	Cured.	Remaining.	Admitted.	Died.	Cured.	Remaining.	Admitted.	Died.	Cured.	Remaining.	Admitted.	Died.	Cured.	Remaining.	Admitted.	Died.	Cured.	Remaining.	Admitted.	Died.	Cured.	Remaining.	Admitted.	Died.	Cured.	Remaining.	Admitted.	Died.	Cured.	Remaining.
Europeans (residents)	...	...	...	...	...	...	...	...	3	2	1	...	...	...	...	...	1	...	...	1	...	...	...	1	1	...	1	1	1	...	1	1	...	...	...	1	...	...	...	1	...	...	...	1	1	...	1	1	...	...	...	1	...	...	...	1
Madeirians	3	1	2	...	65	26	18	21	114	36	55	44	134	56	60	62	71	37	40	56	63	26	42	51	51	15	23	64	31	8	32	55	33	9	24	55	16	2	13	56	7	1	7	55	20	...	22	53	18	8	49	14	72	21	40	25
Creoles and Barbadians (coloured)	...	...	...	...	3	...	2	1	1	...	1	1	1	1	1	...	...	...	...	...	2	...	2	...	2	...	1	1	5	1	2	3	2	...	4	1	...	...	...	1	1	...	...	2	...	...	1	1	...	...	...	1	...	...	1	...
Aborigines	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
East Indians (Coolies)	...	...	...	...	3	...	...	3	3	1	3	2	7	1	3	5	4	2	1	6	6	2	7	3	5	...	2	6	5	1	5	5	4	1	6	2	4	1	...	5	1	...	2	4	...	...	1	3	...	...	...	3	...	...	3	...
Seamen of shipping	20	2	18	...	86	13	47	26	93	27	64	28	54	12	40	30	69	17	41	41	76	24	60	33	94	24	49	54	90	26	65	53	88	18	65	58	55	17	57	39	93	19	60	53	110	32	65	66	128	49	99	46	186	48	139	45

TOTAL.

	Admitted.	Died.	Cured.	Remaining.
Europeans (residents)	7	2	5	—
Madeirians	698	246	427	25
Creoles and Barbadians (coloured)	17	2	15	—
Aborigines	—	—	—	—
East Indians (Coolies)	42	11	31	—
Seamen of the Shipping	1242	328	869	45

CHAPTER XI.

The first general anatomical fact in reference to the disease under consideration, is the almost universal *bloodiness* of the tissues of the body in a patient who has died from yellow fever, having been previously healthy, and not dying from actual hæmorrhage. *Hyperæmia* does not express correctly the idea of this condition, for the dissection of the body shows not only too much blood, but also blood in the wrong place. It flows out from the sub-cutaneous areolar tissue; the mesentery is loaded with it; the areolar tissue forming the attachments of the windpipe and gullet is bloody; so is the mediastinum and the fat around the kidneys. The intestines appear, externally slate-coloured, or gangrenous-looking through the peritoneum, from congestions and extravasations in the mucous and sub-mucous coat. The pleura costalis participates in the same sanguineous appearance as the connecting tissue of the throat, from the universal hyperæmia and extravasation beneath that membrane. If we look into the trachea, and bronchi, and œsophagus, stomach and intestines, and coverings of the brain, and lining of the bladder, we find a similar condition in some, and it may be in all these surfaces. If we wash away the mucus or blood which obscures the view, we may find the part highly sanguineo-vascular, the capillaries in a state of distension, without breach of continuity; if in the stomach, leashes of them may be seen torn and disorganized; or the part may show structureless unvascular ecchymosis, and dots or wavy lines, or patches of greater or less extent, or splashes, as if red ink had been projected from a pen. The membrane of the stomach shows the most varied hyperæmia; sometimes it is arborescent, as if the arterial twigs were chiefly affected by engorgement. Sometimes it occupies the rugæ and villi in wavy lines; in other cases it is in rude hexagonals, as if the capillaries surrounding the mucous follicles were alone affected. At other times, the predominating appearance is an universal rosiness, or deep claret or purple, as if the sub-mucous tissue had been infiltrated with pigment; but generally, most of these varieties of hyperæmia are blended together. If we examine the parenchyma of the great viscera, a similar condition of *bloodiness* obtains. The kidneys are sometimes ecchymosed below the capsule, and a section of them is *always bloody.* The liver is very frequently in the same condition, and is sometimes enlarged from general engorgement, and softened and friable in spots, as if from broken-down structure. The lungs are often apoplectic, with the interlobular spaces broken and infiltrated, so as to lose all physical appearance of pulmonary tissue, and resemble huge clots of blood. These conditions are generally found in the most dependent parts, but frequently the upper and front part of the lungs and liver, and pelvis of the kidneys, are so affected. The appearance is therefore clearly not hypostatic, although gravitation must exercise some localizing power. Sometimes one viscus may be engorged, and a neighbouring one anæmiated and dry. This relation sometimes exists between the kidneys and liver, as in the cases of Gibney and Morgan (Seaman's Hospital, 21st of September, 1852); hæmorrhage during the disease, and previous anæmia, have a similar modifying effect.

The next general anatomical characteristic is the altered condition of the mucous membranes. In the mouth, œsophagus, stomach, and intestines, it has suffered some serious alteration. The epithelium is peeled off, generally or partially, or the whole depth of the membrane is softened, as if acted on by an alkali, or is eroded through to the sub-mucous coat.

These are the two general facts common to all normal cases, and obvious to any careful observer. Inflammatory diseases seem congenial to the action of the yellow-fever poison; and during the progress of the disease, we have frequently found them supervening as epiphenomena. We have also seen yellow fever apparently excited into action by their presence, and consequently the traces of these accidental complications will occasionally be found in the post-mortem examinations. But the lesions of yellow fever seem to have little or nothing in common with those of simple inflammation, and the only *quasi*-inflammatory condition which seemed a result of the disease was, in some instances, where the capsule of the liver—such as in the case of Ellwood (Seaman's Hospital, 21st of February, 1853)—was red and vascular, and, as if in incipient inflammation, excited apparently by the mechanical distension of the engorged parenchyma. It is likely that the suppurations of the liver, which have occurred as sequelæ of yellow fever, were occasioned by the disorganization of tissue which follows the congestions and ecchymoses before referred to; and that, had Macey lived, he would have suffered from an abscess in the softened part of the liver, as the only mode of reparation which nature could institute.

In the post-mortem notes which follow in the reports of some of the fatal cases, the terms "blood congested" and "bile congested" have been used, and require some explanation. The first term is obvious enough, and means what it says—engorgement with blood; but the latter does not always mean engorgement with bile: it has reference more to colour than any other quality; and when the liver was yellow, of whatever shade, the term "bile congested" was applied to it, in contradistinction to the dark purple or slate colour which indicated hyperæmia. Now, this yellow condition of the *enlarged* liver is not yet satisfactorily understood. It is no doubt true that the liver is found sometimes dry and anæmic, from having been drained by hæmorrhage, or vital or physical determination to some neighbouring viscus; and then the capsule of Glisson, tinged by the bile, communicates the ochre or straw or cream-colour to the whole mass. But where enlargement also is present, with or without anæmia, the explanation is not sufficient. Of what does this yellow or ochre enlargement consist? This point has not yet received sufficient attention. But I have found that, in these cases, a small portion of the parenchyma scraped off and submitted to the microscope, showed an abundance of oil globules. In cases which have terminated fatally after protracted illness or apparent convalescence, the bloody condition of the kidneys has passed away, and the cortex is hypertrophied, and of a dull ochrey colour. This condition seems clearly due to the impaction of the tortuous tubuli uriniferi with the same epithelial and fibrinous (?) matter which constitutes the sediment of the urine; and the presence of this matter and fragments of tube-casts can be demonstrated by the microscope to constitute a part of this yellow hypertrophy. But I have never been able to detect oil globules in the kidney; and the instance narrated in the post-mortem notes is undoubtedly a fallacy of observation, the oil most likely having been derived from the blade of the knife. The congestion of the kidneys during life seems to have been attended with no irritation; it is signalized only by albuminosity of the urine. With one exception, nothing like diuresis was observed, nor could have taken place without being noticed, till convalescence became established. Neither was there pain in the congested organ but once or twice (Juan de Nolriga, 24th of December, 1852). The pain so often complained of in the loins is lumbar, and in many of the best-marked cases, careful pressure failed in detecting tenderness of the kidneys. The lesion of the lungs was seldom, if ever, attended by cough or pain or râle, or any sign to attract the attention of the patient or physician until the blood extravasation demanded expectoration. On the liver there seemed to have been induced an irritant effect. The suppression of bile in the last stage had always been preceded by an erethism of that organ, as indicated by the copious secretion of bile, independent (as in the case of the master of the *Undine*), although no doubt increased by the action, of the resolvent dose; and there was frequently tenderness of the epigastrium towards the right side early in the disease, and before it could be occasioned by, as it no doubt frequently is in the last stages, the distension of the capsule: the kidneys and lungs, therefore, seemed to suffer passively, while the liver suffered from active congestion. There was not always a perfect correspondence between the lesion of the kidneys and their functional disturbance during life. Occasionally I have noticed the kidneys in an almost apoplectic state, and yet their functions were scarcely interrupted; and, on the other hand, I have been disappointed in the amount of congestion in instances of entire suppression. In the former case, perhaps, the engorgement occupied less the secreting than the ductal tissue; but this point requires much more investigation. In the post-mortem notes, the weight of the several viscera is given. This precision would have been enhanced had the total weight of the body in each case been also given. Still the weights assist in forming an estimate of the condition; but weight does not in all cases represent the proportional degree of congestion:—thus, in Maxwell's case (Seaman's Hospital, 12th of March, 1853), the kidneys seem to have been naturally small; and although their weight was not extraordinary, yet the engorgement was so intense that their shape became altered to globularity. About one hundred dissections were made since the beginning of the epidemic; but the notes of the first series became confused, and are rejected; and several examinations were made of which no record was kept.

CHAPTER XII.

The common, or *gastro-hepatico-renal, form of yellow fever*, with its stage of febrile reaction and unhealthy subsidence, may be protracted far beyond the average duration of fatal cases; as in the case of Milne (Seaman's Hospital, 24th of February, 1853), or the mate of the *Sobraon*, already referred to. These prolongations of life will generally be found associated with free action of the kidneys, although the urine be albuminous. On the other hand, as in the case of Philips (Seaman's Hospital), who had black vomit within forty-eight hours; and Mr. Dod, who on the second day had, with copious bilious vomitings, *bile in the blood, albuminous urine, and black vomit*, the disease, by the intensity of the epidemic cause, and the high susceptibility of all parts of the body, may be hurried on to early and rapid maturity. In the preceding chapters, *choleroid* and *diarrhœal varieties* have been already mentioned. These refer to the modes of accession, and point to varieties in which the tongue and eye symptoms are generally less early and less distinctly marked, and in which early excoriations around the *anus* occur. In these, the intestinal variety, the *cæcum* is found in a condition similar to that of the stomach; and the lower end of the ileum corresponds with the condition of the upper end of the duodenum in normal cases. In fact, in such cases the cæcum removed from the body might be readily mistaken for a yellow fever stomach. About the beginning of February, 1852, the intestinal variety, or modification of this all-pervading disease, was very common. As has just been stated, in these cases the capillary irritation of the tongue, eye, and face, are generally less observable. In attacks on *old residents*, also, and the black and mixed races—negroes, coolies, and mulattoes—these symptoms are also less characteristic; and the redness of the tongue shows itself at first in the fungiform papillæ only. In the case of the master of the *Hindu*, there were scarcely any *primary gastric symptoms*, and the disease seemed at once concentrated on the kidneys. When the nervous centres seem early affected, and the disease is complicated with *alcoholismus*, the cutaneous system also shows little erethism, and the surface is sometimes even pale. On the 21st of November, 1852, W. Greig died in the Seaman's Hospital, without having manifested the external erethism. On the 27th of July, a case, in which *alcoholismus* was not suspected, occurred, with pale cutaneous surface, in Norman (Seaman's Hospital), and which terminated suddenly and unexpectedly with jet-black black vomit. Williams's (Seaman's Hospital, the 23rd of June, 1853) was a similar case, in which were nervous tremors, but in which intemperance was denied. With *anœmics*, as might be expected, and among Portuguese immigrants, the surface symptoms were less striking than among natives of northern climates. In the latter, the disease sometimes assumed a scarlatinoid form; as in the case of Thomas Fry (Seaman's Hospital, 19th of May, 1852), whose fauces were rough, with red vascularity; and Thomas Dawson (Seaman's Hospital, 10th of February, 1852), who, in addition, had ulcerated sore throat. Exceptions, however, appeared, and the most intense action on the capillaries of the skin, as before related, was seen in the Portuguese man, Antonio Fernandez. In the cases of Copeland and James Foster (Seaman's Hospital, 8th of March, 1853), the eyes were as red and injected as in the most violent ophthalmia, but without photophobia or lachrymation. Pericarditis, as a complication, seems to obliterate the surface symptoms, and gives a pale, collapsed, exsanguine expression, as in the cases of Mr. Kichner and Moses Cain (Seaman's Hospital). In the *pulmonary form* of the disease there was no difference in the external symptoms from those commonly observed. But unusual heat of the chest sometimes gave early notice of this obscure variety; and uneasiness, jactitation, and heavy breathing, indicated its approach to pulmonary apoplexy. Auscultation and percussion have not yet been sufficiently applied in the exploration of this condition, and chiefly from the restlessness and impatience of the subject of it. The *cerebral variety*—that which arises from hyperæmia (and not uræmia)—is characterized sometimes by intense pain in the head and disordered mind in the early stages of the disease; and, as in the case of Laird (Seaman's Hospital, 16th of October, 1852), uncontrollable irritability of stomach. In the late stages, typhomania sometimes occurs from congestion and effusion on the brain, as in the case of John Williams (Seaman's Hospital, 1st of September, 1852), and in whom, as usual, no lesion of the intestinal glands could be discovered. There is some difficulty in discriminating between the effects of uræmia and hyperæmia on the brain, as both conditions are seldom pure. Thus, in Laird's case the one followed the other, and was mixed with it. But the typhomania of Mr. Williams was readily distinguishable, by the assistance of collateral symptoms, from the occasional delirium and insensibility of such a case as that of Charles Maxwell (Seaman's Hospital, 10th of March, 1853), who, suffering from uræmic intoxication, sat up in bed, and amused himself in scolding the nurse and every person around him. Yellow fever, as has been observed already, occasionally came into collision with small-pox; and the latter prevailed, and excluded the former, if in the incommutal stages. It was found *mixed* up, both as a primary or secondary affection, with the following diseases:—Pneumonia (single and double), pleuritis, pleuro-pneumonia, pericarditis, meningitis, lymphatitis, delirium tremens, and intermittent fever. Of all its admixtures, the most numerous, it would appear, were those with pneumonia. But there was likely a fallacy on several occasions in regard to this complication. The expectoration from a softened and broken-down lung was, no doubt, frequently mistaken for the rusty expectoration of inflammatory hepatization. It was in the cases complicated with *delirium tremens* that the peculiarity in the conditions of coagulability of the urine was observed. Mr. C., an Irish youth, about six months in the colony, had been threatened with delirium tremens fourteen days before the attack of yellow fever. On the third day of attack the epidemic disease left him, but the delirium tremens then recurred with its most marked characters, and he died two days afterwards. The Ursuline Convent is situated in perhaps the most miasmatic locality of the town. One of the nuns who died of black vomit had three distinct paroxysms of tertian intermittent, the last ending with the fatal attack of yellow fever. As all our patients in the Seaman's Hospital came from the shipping, the focus of infection, it was to be expected that outbreaks of the epidemic disease would occur among patients who had been admitted thence for other ailments. These did happen frequently, and gave rise to a modification of attack, which we have denominated "threatenings." An extract from my note-book of some notices of this form will show what is meant by the term.

"*Threatenings of Yellow Fever.*—We have frequently seen such in the Seaman's Hospital—men admitted with other ailments, and requiring 20 + 24* as intercurrent treatment. Two such cases occurred to-day and yesterday, in the persons of Griffith Thomas and Charles Burton, well marked, and subdued by the dose. These cases are prescribed for *pro re natâ*, and seldom entered in the case-books. Sometimes these threatenings finally break out into fatal attacks."—10th August, 1852.

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* For explanation, see page 81.

"*Threatenings and Outbreaks in Seaman's Hospital.*—John Hooper, admitted with phthisis, ending in severe attack. Frederick Taylor, gonorrhœa, ending in severe attack; two doses required. Walker, gonorrhœa and stricture—fatal. Duncan Cameron, fractured clavicle—one dose. William Cransick, second threatening. William Roberts, threatening."—15th, 19th, 22nd, 24th August, 1852.

"These threatenings are generally marked by flushed face, frontal headache, with considerable warmth of forehead, suffused eyes, and quickened pulse. If not stopped (but they generally are) by one resolvent dose, they are highly dangerous. They frequently occur and are prescribed for *en passant*, but when aborted are not noticed in any of our records. The incubation of some of these attacks seemed retarded in the presence in the system of such evacuant diseases as phthisis and gonorrhœa."—25th August, 1852.

"*Threatenings of Relapse.*—Three cases occurred yesterday in Seaman's Hospital in convalescents from yellow fever, all extinguished by one dose each."—13th September, 1852.

"*Attack while in the Hospital with crushed fingers, repulsed*, twice within a few days, by four powders, in case of Mate Anderson."—Seaman's Hospital, 27th December, 1852.

"*Two Threatenings of Yellow Fever*, one coming with chills, among those some time in (Clinch and Freeman), aborted by one dose each. This happened yesterday; close, warm, humid weather, followed by torrents of rain to-day."—18th May, 1853.

There was a form of the epidemic disease which was known in our case books by the name of "smouldering." This type is distinguished by the absence of any rampant symptom. It seems to be a *non-localized* variety, or so equally but mildly spread over the system that no organ is intolerably implicated. This variety *begins* in much the same manner as the common form; but if the attack be not aborted, it proceeds differently. Its peculiarities begin from about the second day. In its mildest degree, the disease then seems to have nearly departed, but the patient remains languid. The skin, which is nearly cool, becomes purplish over the face, arms, and chest chiefly (languid capillary circulation, as it is called in the case books). The eye gets tinged, the urine is free but bilious, and, for a day or two, albuminous. But gradual restoration to health follows. In severer cases these symptoms are all intensified, but still the kidneys are not much involved; the stomach remains quiet; there is little capillary irritation of the mucous membranes visible, and little or no peeling of the epithelial covering of the tongue. But the skin has a macerated, greasy appearance, and beads of acid perspiration stand on the forehead, on the alæ of the nose, and around the mouth, and sudamina appear over the body. Around the anus and over the lower surface of the scrotum, the cuticle peels off; and if any blister has been applied, the surface there is of a claret colour. The blood is alkaline, and the matter of the excoriation also. A small bloody anthrax appears (in the case I have now in view—the mate of the *Alexander Johnston*, private lodgings) on the left elbow. The patient throughout has been and remains taciturn and apathetic, and speaks in a subdued, low tone of voice. This brings his case to the end of the third day. Incipient black vomit may appear, as it did in the mate's case on the fifth day. Convalescence then begins, but is slow. The junction of the gums and teeth oozes blood, which stains the fur of the tongue, as if with tobacco-juice; but the appetite is good; the skin clears between the interstices of the sudamina; these little vesicles dry up and desquamate; the powerful action of the kidneys soon relieves the circulation of bile and decolours the eye, and health is perfectly restored. Few good descriptive cases of this form of the disease are to be found in the casebooks, owing to the circumstance that there are so few salient points to be described, and little indication for any interference with nature on the part of the physician. Such cases, in the hospital reports, break off, generally at the second or third entry, with such a prescription as the following:—℞. Aq. acet. ammon., aq. camphorat., āā. ℥ss. ter in die; and no report is again made until eight or ten days after, when he is marked discharged; the medicine in the meantime having been omitted, probably after two or three days' use. Of such cases were England (Seaman's Hospital, 19th of July, 1852), Lamont, Dixon, Pullin, Jenkins, Russell, Gorman, Forsyth, and W. Davies (Seaman's Hospital, December, 1852). The pathological rationale of this variety of yellow fever is yet to be cleared up; but it would seem as if the less vital structures were the throne of the disease in such cases; that it occupied chiefly the periphery of the body—the cutaneous capillaries instead of those of the central viscera. I am also inclined to believe that, though diarrhœa has not been found to usher in the attack, it is sometimes the manifestation of the *intestinal* variety, or in alliance with it.

Akin to this form of the disease is another. It was noticed in the last epidemic. It appears most frequently among the dark races, and some of the Madeirians. It is, perhaps, a mere extension of the *smouldering* form, but the symptoms are more typhoid. It has strong resemblances to sea scurvy, if we could conceive that to be an acute disease, and hence may be named the *scorbutic variety*. It is generally unattended with gastric symptoms, nausea, or vomiting; and instead of the raw-beefy, clean, epithelium-denuded tongue of normal cases, the mouth seems as if smeared with tar. The following note, which I made in my memorandum-book for another object—viz., to illustrate the *formative* stage of yellow fever—will describe the scorbutic variety in an European—not pure, certainly, but sufficiently characteristic. Although this case was eminently suggestive of other important matters, the phenomena at the time were not duly appreciated nor correctly interpreted, and it was regarded only as illustrative of the mode of invasion of yellow fever.

> "*Formative Stage of Yellow Fever, showing itself by Boils on Forehead.*—Captain Sutherland, of the brig *Hero*, Shetlander, a very tall, narrow-chested, and very old-looking man (although it is stated that he is only forty-five years of age), got yellow fever, in the invasion stage, on the night of the 18th instant. I had seen him every day for a week previously, while I was attending his mate for an attack of the gravior form of the epidemic. For five or six days before the invasion his forehead was covered with little numerous unhealthy boils, which prevented him from being able to wear his hat. As will be seen by the sequel, this was evidently the first manifestation of a peculiar case of yellow fever, closely allied to scurvy. The febrile symptoms were low all along while they lasted (two days). While under them, he walked along a plank ashore to meet me (the brig lay alongside the wharf, and the mate, who recovered afterwards, lay sick in an adjoining house), to save me the trouble of going on board. He was with difficulty persuaded to lie up. When the short, trifling fever left him, his pulse fell much in power (particularly observed in right radial artery, probably from a natural difference of size in the artery), and became very slow—forty-eight. The chief symptom, and which persisted, was a

crimson-tipped tongue, wedge-shaped, and the surface behind tip foul and brown, and inclined to dryness. His gums began to bleed and recede from the teeth. He hawked up rusty sputa from mouth and fauces. His restlessness was easily, and *much too easily*, quieted by a few drops of morphine solution. In fact, this is the first case of the present epidemic in which morphine was detrimental. The boils on the forehead began to fade and become purplish. Other and larger boils, with an unhealthy dark-purple centre and apex, and coppery inflamed areola, came out over arm, body, and legs; and some bullæ, which burst and bled dark grumous blood. Anorexia; extreme faintness and nausea on assuming the erect position ensued. Stomach easily irritated by the swallowing of food and drink, but never decided vomiting. Feebleness of voice; urine copious and dark-coloured. Bowels inclined to torpidity, but easily acted on. To-day, two of the boils on right leg bled much; so much, that I was called to arrest the hæmorrhage. They looked livid and much swollen from a large black clot, which I tried to remove without using much force, but could not. I think it had the appearance which is described as 'bullock's liver' in sea scurvy. A little stream of red blood trickled out from the clots, which lead me to suspect that a small artery had given way. Hanging down the leg or attempting to stand made the hæmorrhage, which otherwise oozed, alarming. He to-day also complained of much *pain in the side* (the dangerous sign in sea scurvy, described by Lind) below the right axilla, midway in side; and from an imperfectly-made auscultation with the ear, dulness of respiration was found over seat of pain, and an occasional coarse crepitus. He now also *expectorated* rusty tenacious mucus. A dry cup was applied over the seat of pain, with some relief. Notwithstanding the most liberal and varied use of cordials, and soups, and anti-scorbutics, he sunk during the day, rapidly, and when I last saw him—about half-past 7 P.M. to-day—he was restless and moaning, and almost pulseless, and unable to ascribe his uneasiness to any local pain. No vomiting or nausea. His breath and body have been exhaling a disagreeable, sour (?), fetid smell all day. I do not expect to find him alive to-morrow, 25th of February, 1852.

"I may mention that, in the foregoing case, I examined his legs to-day, to detect that hardness of the muscles which authors say is found in the legs of those who suffer from sea scurvy. But the muscles and integuments were quite flaccid. He never had been on a very long sea voyage, nor suffered at any time from sea scurvy, and has been in this harbour nearly three months. Does the foregoing case not seem to show that, in yellow fever, the blood is primarily affected? 25th of February, 1852.

"Captain Sutherland died at midnight; his urine was free; he continued moaning, but his intelligence was clear." 26th of February, 1852.

By Peter Daley's case, before referred to, the notion that hæmorrhages of yellow fever have their origin in a dissolved state of the blood, was staggered, and a lesion of the solids looked to, with probability, as at least a joint cause. This modification of opinion was for some time unsettled, and the dubious and unsatisfactory phrase of "loss of vital cohesion," all the explanation applicable to the condition of the arterial tubes which the mind had to lean on. The case of Captain Sutherland is now much more intelligible than at the time when the foregoing notes were written. It was clearly a combination of the scorbutic and pulmonary forms of yellow fever. But so far do I now consider it from proving that the disease is primarily a blood affection, that I feel convinced that the scorbutic variety arises from the yellow fever poison acting chiefly on the *arterial twigs* and capillaries. If this view be correct, it might be well for those who have the opportunity, to re-examine the current pathological opinions on sea scurvy.

It will have been perceived, that the distinctions of variety and form in yellow fever are to a considerable extent artificial; that one variety is seldom seen pure; that they are generally mixed and blended. Still, the distinctions are not without differences, and they assist in understanding the malady in its Protean shapes. The *simplex form* of the last epidemic was not observed in the present, in the coasts and low lands. Neither was it observed in the last till the epidemic had continued upwards of two years.

CHAPTER XIII.

On the subject of diagnosis and prognosis much has been necessarily anticipated in the foregoing observations. I know of no disease with which a well-developed case of yellow fever could be confounded; and to those who understand its habits it can rarely present any serious difficulty of diagnosis in any of its stages and complications. In the early stage alone can a mistake be made. The absence, then, of some of the external symptoms should induce the practitioner to extend his inquiries, and he will rarely find a case in which the capillary irritation is not observable on some part of the exposed mucous surfaces. Should such an instance occur, he will find the fever accompanied by that peculiar form of headache which is decisively diagnostic. This headache gave the earliest warning, and in Surinam and Cayenne was relied on as the most characteristic symptom. Should the capillary irritation not appear after the lapse of forty-eight hours, then he must look out for those complications which have been already indicated—the chief of which is pericarditis. I could conceive much embarrassment on the part of the practitioner in distinguishing yellow fever from benign scarlatina, if the two should happen to co-exist as epidemics; and I am quite unprepared, at present, to assign any satisfactory differential distinction. But when the stage of acid elimination has been marked, all doubt must then be at an end, for this phenomenon is unknown in any other fever. I have seen instances in which yellow fever was stupidly mistaken for ophthalmia, and the disease treated accordingly; but not in the present epidemic has such a blunder come to my knowledge. With the mind of the practitioner alert to the existence of the epidemic constitution, he can, by intelligible signs, recognise even the taint which is communicated to other maladies, and identify the disease, with moral certainty, amidst any of its complications.

On the 3rd of January, 1853, Manuel d'Alrea presented himself for admission to the Colonial Hospital among the crowd of other applicants, and complained only of pain in his side, cough, and fever. On applying the ear to the affected part, pleuro-pneumonia was readily discovered. But on observing afterwards the state of his tongue, the presence of yellow fever was at once recognised. On further examination of the urine, the same day, it was found highly albuminous, and the existence of the double disease was confirmed by the sequel.

On the 29th of June, 1852, an exactly similar case had occurred in Vincent Gomes, in which pleuro-pneumonia was the primary disease, and fatal yellow fever consecutive, but detected in its onset, and through the disguises of the leading affection, by the tongue symptom.

I have had no opportunity since it became important to ascertain the fact, of testing the urine in that intensified form of marsh or malarial fever commonly known as *bilious remittent*, and locally with us, *colony fever*, and cannot say if albumen be present or not in any stage of it. But numerous observations have failed in detecting it in the base or radical of that disease, intermittent fever: and the capillary irritation and acid elimination during life, and the altered condition of the mucous membrane of the alimentary canal after death, are, I believe, never found in bilious remittent. In this latter affection the mode of death, with us, is generally by coma, and the sub-arachnoid effusion and opaque arachnoid membrane explain the *rationale* of it. In it the spleen and liver may be enlarged, but the kidneys are unaffected; and, save the jaundiced tinge, the traces of diseased action, such as in yellow fever, are not seen. When vomiting occurs, it is when the congestive stage has reached its acme at the termination of the chill, and, except in drunkards, rarely extends into the hot stage. Engorgement of the liver and spleen and portal circulation, and not irritation of the mucous membrane of the stomach, seem to originate the vomiting, and be relieved by the copious discharges of the bile that follow. The headache is in the temples or top of the head, or all over it. The stomach, instead of becoming more irritable, is settled as the disease advances, and large draughts of fluid are easily retained. Calomel and antimonial powders, in small and frequently repeated doses, act soothingly. The teeth and tongue are dry, but not red and vascular, and the epithelium apparently worn off at the tip, as if by attrition with the teeth. But the epithelium in reality is only *shrivelled*, and when the remission occurs, the tongue is flat, moist, and pale, and its epithelium is found entire.

Yellow fever, although it may be engrafted on an intermittent, when once formed has no intermissions. It is a fever of one paroxysm, without the crisis of perspiration; and when it is over, health is restored, or the disease goes on inducing its ultimate changes without febrile action. The time of seizure is different with yellow fever from that of our permanently endemic fevers. It generally comes on in the night half of the twenty-four hours; while with us, all our miasmatic fevers, whether quotidian, double quotidian, or tertian, in the immense majority of instances, occur at mid-day. And, if we follow intermittent into its sequelæ, we find no resemblance between the two diseases. There is not the quick restoration of health usual in yellow fever, nor the bloody furuncles of unhealthy convalescence; but instead, enlarged spleen, anæmia, dropsy, and colliquative dysentery.

Although frontal as well as general headache, with lumbar and other muscular pains, usher in the fever of small-pox, the absence of gastric irritation and capillary injection of tongue, lips, and eye, is sufficiently distinctive.

The number of the characteristic symptoms present, and the degree in which they are manifested, furnish criteria of the severity of the case, and the ratio of danger. A slow pulse and moderate temperature of the body and quiet stomach are always favourable indications. But the more fiery crimson the tip and edge of the tongue, the more irritable the stomach, the severer the headache, the worse the prognosis of the first stage, and *vice versâ*. Slight or moderate epistaxis is a sign of little prognostic value in any stage; but a streak of blood in the early vomit indicates much danger from the attack; while the same during the stage of black vomit, or after acid elimination has set in, is favourable, if the corpuscles are found entire. In the second stage, the earlier or more complete the suppression of urine and the more copious the ejections of black vomit, the more imminent the danger. But if the urinary secretion continue, and the black vomit be scanty from the first, or is afterwards suppressed, the patient may yet survive. Urine simply albuminous is a less serious sign than when it also contains tube casts; but if these are thin and few in number, they do not add much to the gravity of the indication. Free, copious urine, no matter how dark or bilious, is the most favourable of any single sign. If the urine be scanty, and it be loaded with tube casts, entangled in epithelial and fibrinous (?) matter, the light buff-coloured curdy sediment before mentioned, it indicates a complex lesion of the secreting structure of the kidney. It is the urine symptom in its maximum of severity, and is as fatal as if the suppression had already occurred. Blood corpuscles in the urine were not looked on with apprehension. A faltering of the articulation is a bad prognostic, and a difficulty of protruding the tongue enhances it. Prognostics are derived from the effects of treatment. If the resolvent dose do not bring away "stools characteristic of the powder," but, instead, thin grey abilious matter; or if early hypercinchonism be induced, it is an unfavourable indication. The danger of the case is enhanced by inflammatory complications, and by hypertrophy of the heart. The recency of residence in a temperate climate; the *race* or complexion of the individual; the fact of his previously having suffered from a gravior attack, or an aborted one, will enter into an estimate of his chances of recovery. It is unnecessary to recapitulate the modes of death. These are signs too late to be of any practical importance.

CHAPTER XIV.

Till the 6th of January, 1852, the profession and the public were unaware of the presence of yellow fever in the community. The fatal cases in the family of Mr. Vervestein had not been recognised by the practitioner in attendance (who had never before seen the disease), and nothing was said of them till after the disclosures from the hospitals had been made to the local government. Dr. Gavin, the Medical Inspector for the West Indies, then in the colony, learned the fact from the government secretary's office. This circumstance is mentioned to demonstrate the difficulty of discovering *first cases* in any epidemic, unless where the opportunities of observation are as ample as the vigilance is unceasing. The manner in which the epidemic invasion commenced in the shipping, is described in the official communication to Governor Barkly, of the foregoing date, how it began by tainting the ordinary endemic fevers, and gradually acquiring intensity, till the disease became a well-developed primary affection. This communication and the appended documents, as well as a statement of the health and meteorology of the non-epidemic period, have already appeared in the third edition of the "Account of the Last Yellow Fever Epidemic of British Guiana." The particulars in regard to the two fatal cases therein referred to, as the earliest in private practice, and occurring about the 22nd of December previously, are as follows:—Mr. Vervestein, an Englishman by birth, thirty-two years old, twenty-eight of which he resided in Barbadoes, took up his residence in Georgetown, Demerara, about two years prior to the outbreak of the epidemic. His domicile was in Carmichael-street, opposite the south-west end of the parade ground, and close to, and directly to leeward of, what was then a very wide, putrid, offensive trench. Mrs. Vervestein and three children rejoined Mr. Vervestein from Barbadoes, exactly three months before the first case of sickness appeared in the family. The children, on arrival in the colony, were remarkably plump, ruddy, and clear in complexion, as if they had arrived from England, instead of the West India Islands. The first case was in the person of his son, five years old, who recovered. Then, and while his brother was sick, a younger son, three years old, sickened and died. Then the father sickened, but recovered. And lastly, the infant, eighteen months old, sickened and died. The two deaths occurred within a week. The mother remained quite healthy. Mr. Vervestein informed me that his father died in Barbadoes, after residing there several years, of yellow fever, at the age of sixty-three. This family had no connexion with any source of human contagion; and the cases which immediately followed them, and the cases which occurred at several intervals of time in the same street—viz., the governor's white maid-servant, Mrs. S., Mrs. B. H., and Mrs. H.'s white maid-servant, had no communication with each other; and except in the Vervestein family, each case occurred in a single form, and without any lateral offshoots.

From the statements already given of the progress of the epidemic from east to west along the coast of South America, it has in this instance every appearance of having been an *imported disease*, though not in the sense usual to that term. The condition of the coast line, which had been observed as coincident with former epidemics, was in this instance absent. The epidemic outbreak also recurred in a much shorter cycle than we had reason to expect from past experience; and however much it might be domiciled, and sustained, and reproduced (for though the pestilence extended on to leeward, it at the same time remained with us), a comprehensive view of its whole march enforces the conviction, that in this instance the prime exciting cause had its origin beyond the bounds of the colony.

It would appear from the observation of the present epidemic that though, as is well established, a certain high average temperature is required for the generation and continued existence of the efficient cause of yellow fever, it has not its genesis from any known combination of meteorological elements, and may appear at a time when they are highly favourable to general health and comfort: that the laws of its diffusion differ from those of gases: that it is impelled by atmospheric currents, but seems to possess some power of spontaneous motion: that though intense energy of vegetative power characterized the seasons antecedent to and during the epidemic invasion, its shifting lines of infection and gyratory movements suggest to the imagination the attributes of insect life: that the development of its power was gradual, from its feeble and diluted manifestation at the end of October till its perfectedness at the end of December, and its maximum of intensity a month afterwards: that during the course of its progress it showed marked variations of epidemic power: that in constitutions apparently the same, the system was affected in various degrees, as if the poison acted in proportion to its quantity, and as a poison and not a ferment: that its first impression on the system seemed in many cases local and circumscribed, although attended with the usual constitutional disturbance: that it can actively occupy the body simultaneously with other affections, and may be either subordinate or paramount in the issue: that though its extensive application or saturation of the system by the efficient cause eventuates in a spontaneous outbreak of the disease in the individual, there are circumstances which accelerate its action and augment its intensity, and others which retard or entirely obviate and render it inert. These circumstances will now be considered, and though little that is new can be added to the subject of aggravating, exciting, determining, and predisposing causes, and the conditions of comparative immunity, the fresh illustrations may be useful in corroborating former experience.

As to the effects of personal *contact*, mediate or immediate, with those sick of the disease, the case of the brig *Sarah* has been already noticed, in which all the crew sickened simultaneously, and all those of the white or mixed race died within a week. The following is the newspaper article which then appeared, and the statements of which were subsequently confirmed. The italicising of some of the words is mine:—

> "*Paramaraibo*, 3rd Aug., 1852.—Last week arrived at Braams Point the North American brig *Sarah*, Captain L. S. Griffin, from Demerara, which, in consequence of the sickness on board, was put into quarantine. His Excellency the Governor immediately ordered the health-officer on board, for the purpose of treating the sick. According to the reports sent in by the doctor to yesterday's date, we learn the death of the captain, first mate, second mate, and one sailor; but fortunately the others had all recovered. We understand that the passengers, *who remained throughout healthy*, will in a few days be allowed to come to town."

Mrs. Vervestein, who must have been in the most intimate contact with the infant who died in her family, alone escaped an attack. In the book of the Colonial Hospital, it is mentioned in the last epidemic that a Maltese woman, named Fannia, died of yellow fever on the 16th of January, 1840, and while she had black vomit, nursed her infant at the breast, without communicating disease to it. The brig *St. Fillan* lost three men in Demerara with yellow fever in the present epidemic, after which she proceeded to Berbice to take in cargo. Immediately after her arrival there, two of her men and the mate sickened and died. They had obviously contracted the disease in our port, and transplanted it to the neighbouring country. But it did not grow. No steps were taken to prevent the spread of contagion, if any existed, but the importation was without issue.

We had two specimens of the West India Island type of the disease imported into Demerara while our epidemic was in progress. Mr. C. T. Chandler and his brother arrived on the evening of Tuesday, the 28th of October, to take shipping for England on their way to Australia; the age of the former was about twenty-two; he was a white, a native of Barbadoes, and had never before been out of that island. He had sickened with yellow fever that there then prevailed, on his passage over. I saw him on Thursday evening for the first time; the brother not alarmed, supposing his illness to have been only sea sickness and its effects. He died with black vomit, on Saturday morning. I watched the influence which this case might have on the health of the inmates of the house in which he died till the 11th of December following, but no effect could be observed.

Mr. B., a passenger from England by the mail steamer, at the end of December, 1852, contracted yellow fever while the steamer lay at some of the infected islands—probably Barbadoes. It was four days old when he arrived in Demerara. A bloody furuncle formed on his cheek during convalescence. No injurious effect followed either to the inmates of the house or his friends, who had the most unrestricted communication with him.

The *Lancaster*, *Atalanta*, and *Flirt*, proceeded up the Demerara river to load with timber, at different times, after having been some time in the port. These vessels lost while there more than half their crews. There is a population of about 500 where they loaded, about fifty of them whites. These people bring the timber, and assist on board the vessels, and the most unreserved communication is kept up between them and the sailors. They were constantly going and coming, and the sick and dead were landed among them. But not a single case of yellow fever occurred among that population.

No case occurred spontaneously in the district of Mahaiconey; but Mrs. M., who resided there, contracted the disease in town, and died after her return home with black vomit. The disease did not spread. Private lodgings, during the epidemic, were in great demand for the ship officers and many of the seamen. In a memorandum of the 17th of January, 1853, I read as follows:

"*Number of Yellow Fever Patients in Private Lodgings.*—Yesterday, when visiting Mrs. Morison, wife of the master of the brig *Hope*, of Carrickfergus, Mrs. Fraser (white), who keeps the lodging-house, brought to my recollection that Mrs. Morison was the forty-second case of yellow fever which she had had in her house, of whom I had attended all but four. I have no doubt but that Mrs. Thompson (mulatto), at the corner of Main-street and Regent-street, had twice as many in her lodging-house; that Mrs. Wood (mestizo), had as many as Mrs. Fraser; that Mrs. Hobbs (mulatto), in Robb-street, had also as many; Mrs. Frances Porter (mestizo), in Water-street, had as many. Besides these, other houses took in sick seamen—such as Miss Catherine Mortimer (negro), and Mrs. Milleman (mestizo). Among them all, however, not a single instance of contagion, or any suspicion of it, has ever arisen. Nor have they, nor their servants, nor visitors, nor washers, furnished a single case of the epidemic disease."

On the 1st of January I find the following entry among my memoranda:

"*Number of Servants in the Hospitals.*—In the colonial branch there are forty-three constantly employed, and frequently changed. There are nine in the seaman's department, constantly employed and occasionally changed. Total, fifty-two constantly employed. Of these, since the commencement of the epidemic, two have suffered from yellow fever—viz., Maria de Monte, who is in charge of the Lazaretto, and is never near the fever patients, and whose case was complicated with pneumonia; and Manuel, the Portuguese interpreter, who never does duty as nurse. A nurse named Caruthers was laid up two days with a 'threatening,' and the head cook (mulatto), while suffering from orchitis, showed some taint of the epidemic. The number of servants stated is exclusive of those employed in clothes washing. The washing is done by contract, and none of those engaged have suffered. The servants employed about the hospital are Madeirians, negroes, mulattoes, Europeans, and coolies, in about equal numbers."

In the fatal cases which have occurred among the white natives, and excited the notice and consternation of the community, and who during their illness had "troops of friends" around them, not a single instance of contagion was suspected to have happened. Having watched, however, for instances (it being easiest to look after and record exceptional cases), I discovered among them one, which may be open to suspicion till the circumstances are explained. Mrs. W., white, native, aged about twenty-five; four or five years without having left the colony, sickened and died on the 17th of January, 1853. Her residence was a considerable distance from Water-street, in a hitherto healthy locality, on the Brickdam-street. Exciting cause of attack unexplained. Her maid-servant, English, elderly, four or five years in colony, had been very assiduous in her attentions to her mistress, but not more so than some of the relatives of the deceased. She, however, sickened, and had black vomit *incipiens* on the 28th of February following, but recovered. At the same time, the son of the deceased, two years and half old, sickened and died. Though the symptoms were somewhat obscure, and the diagnosis unsatisfactory, and the disease ended in convulsions (an infantile affection then prevalent), yet there can be little doubt that the cause of death was yellow fever. Neither the father, nor two elder children, nor son of the nurse (English and about ten years old), nor any other inmate of the house, suffered. The grouping of disease in this family was entirely exceptional among cases affecting *white natives*, for in all the other instances the attacks were isolated and singular. Now, although the distance of time between the first and second cases in this family is considerable, still there is an appearance of probability in the idea that the disease had been communicated from the mother to the nurse and child, until we know the history of the other cases in the neighbourhood. At this time the poisonous atmosphere had evidently extended to the Brickdam, for a fatal case in the person of a Scotch lad, a few months in the colony, followed that of Mrs. W. exactly a month afterwards, and in the next, but *separate* and *windward* dwelling, though between the houses and inmates no intercourse whatever had taken place. On the 24th of February also, about two hundred yards further up in the same street to windward, a second attack of the epidemic, in the person of Master B., occurred; and on the 25th there was another seizure, in the person of Miss S. (mestizo), about one hundred and fifty yards to the south. Both the latter were aborted attacks. Now, it is certain that none of these last-named cases had any personal communication with each other previous to the advent of their respective attacks, and they appeared *successively to windward*, until the nurse's seizure again manifested the infection in the old locality. Although the nurse and child, who remained in the house with the mother, contracted the disease, none of the friends from a distance who visited and closely attended the sick, suffered on their return home, but from fatigue.

There is the most unreserved intercourse between the ship masters and those connected with the public buildings, particularly of the custom-house chambers. All the public offices are in the one extensive building. Of officers permanently employed in this building there are sixty-three, and only two or three of them "coloured." Nine officers are in the custom-house. Two only of the whole number were affected, and these two were lads lately from Europe, and not in the custom-house, but the opposite end of the building, the registrar's office. Between these two cases there was an interval of several months; one recovered and one died. They were treated in the midst of their respective families (whites), without restraint or hindrance, and without any sign of contagion ensuing. On the other hand, Dr. Levin, resident surgeon of the hospitals, died on the 1st of May, after three days' illness, of the prevailing disease. He was a native of Russian Poland. He had been five years in the colony. The exciting cause of his attack seemed to have been fatigue and exposure to a thorough draught when violently heated by the pursuit of a thief whom he detected stealing his property. He also had been previously in the habit of passing the evenings in Robb and Water Streets, in the main site of infection. Drs. Butts and Goring, resident surgeons, each had an aborted attack; but both had lately arrived from cold climates, England and Canada. As already mentioned, many cases of yellow fever appeared within the Seaman's Hospital, in the persons of patients who had been admitted for other ailments; but they had all been exposed to the river influence previously. As the object of our observations was to ascertain the truth, and hold by it wherever it might lead, these cases were watched as sedulously as any partisan of contagion in yellow fever could desire. Among them, only two cases of an equivocal character appeared, and which bore favourably on the doctrine of contagion. They are as follows: George Philips, of the *Harkaway*, was admitted three days after his arrival in port to the Seaman's Hospital for rheumatic ophthalmia, on the 26th of February, 1853. On the 5th of March following, he was suddenly seized with a violent attack of yellow fever. As usual, there had been no separation between his and the yellow fever cases. Now the only suspicious point in this case, as regards contagion, is the fact that up to that period the *Harkaway* had as yet furnished no other case of yellow fever that I could ascertain. The other case was that of William Smith, of the *Montezuma*. These cases occurred very close on each other. After having been one day in harbour, he was admitted to the Seaman's Hospital for bubo, and was literally covered with *acne punctata*. On the 7th of March, after mixing as usual with the yellow fever patients, he was seized with what proved a fatal attack. In this case also none of his messmates had as yet suffered from the epidemic.

I have now stated all the facts that have come to my knowledge during the course of the epidemic, which favour the doctrine of the personal transmissibility of yellow fever. They were earnestly looked for among the countless opportunities for observation, and no others could be discovered. Those which were found have been honestly declared. In such a poverty of positive proof in the affirmative of the doctrine, it is no argument against those who disbelieve in the doctrine of contagion, to assert that their proofs amount to negative evidence only. The experience of the present epidemic has confirmed that of the past, and the idea of contagion, which was then unanimously relinquished, has not been revived. Neither do facts countenance the fanciful compromise which some have offered as a settlement of what is scarcely a question among those who in modern times have seen the disease with their own eyes—viz., that it is *the type of disease in which black vomit appears only which is contagious.* In Demerara we would as soon think of asserting that intermittent fever in some of its forms and types is contagious, as to predicate it of any of the manifestations of yellow fever. It has already been observed that the state of the weather exercised a modifying influence on the manifestations of the epidemic. Heavy rains, with calms, creating a damp, hot, steamy atmosphere, or the prevalence of land winds, which are cold (comparatively), damp, and of low dynamic power, intensified the action of the poison, augmented the number of admissions, and increased the severity of the symptoms. The return of a dry, cool, clear, elastic atmosphere, with sweeping trade winds from the ocean, was always followed by mitigating effects. The *rationale* is easy. The condition of the weather first referred to oppresses the cutaneous and pulmonary functions, and thereby lowers the tone of health, and its power of resistance to the action of noxious agents, at the same time that the stagnation of the air is favourable for the accumulation of the atmospheric poison, whatever it may be. Moreover, it is likely that, at such times when the wind is from the land, and the sky is darkened (as it always is when land winds prevail) by dense black clouds, which overspread it from zenith to horizon, the air is positively vitiated by an excess of carbonic acid gas. The whole country is thickly covered by the most luxuriant foliage of grass, shrub, and tree (rarely fleshy leaved), the purifying influence of which on the atmosphere must be impaired during the temporary diminution of light. Whether during the darkened stagnant state of the air which then exists, the vivifying supply of oxygen be lessened, or the amount of carbonic acid be increased, effects detrimental to the vital force must ensue, which would be favourable to the ravages of the epidemic. It arrived on our shores at the latter end of the year, between the autumnal and vernal equinoxes, when the trade wind blows day and night over the face of the country, and it may have been owing to this accident—anachronism—that the invasion was so feebly commenced, and required so long a time to muster its forces.

The most important influence that could be brought to bear against the susceptible, was that of locality. As in the former epidemic, and as already noticed, the focus of infection was the shipping and Water-street. The very same houses that before signally suffered, were again visited with a like severity. The poisonous agent persists in its predilection for low, damp, crowded places, and putrid exhalations, and woe to the unwary or reckless who lived or lingered there. In the house next to that which Mr. Vervestein occupied, Mrs. B. died about two months after the first cases. Two tenants who successively occupied that house also suffered, but recovered. Persons who on business or for change of air came to town from the uplands of the interior, suffered. The Rev. Mr. L., safe at the missionary station near the penal settlement, came to town on a visit, and died. Mr. Charpentier, from a more remote position in the Upper Essequibo, brought his family to town for the sake of coast air, they having suffered from intermittent fever, anæmia, and enlarged spleen, in the interior; two died with black vomit. Vessels that carried coal in bulk or patent fuel, were severely visited. The history of the *Syrophœnician* and *City of Peterborough*, and several other instances, illustrate this point. I have been informed of the case of a Portuguese boy, who appears to have contracted yellow fever by being much on board the *Grafton*, while discharging coals at Plantation Houston, at a time when the crew also suffered. The plantation was at the time healthy, and remained so till, long after any influence could have been exercised by the presence of the sick boy, and between whose case and those that followed, no connexion could be traced.

Fatigue and checked perspiration and long-continued solar exposure precipitated the attack. Sometimes the heat of the berths below forced the officers to get up on deck at night, where they sat or lay with little covering to either feet or body; and a chill following was the signal of seizure. The *tolerance* of the poison which those residents who passed through the epidemic from its first feeble manifestations, had acquired, was seriously impaired by even a temporary removal from the colony, and a return to it within a few weeks. The crews of East India ships were severely visited. It might have been from the nature of the previous cargo, rice and immigrants, or from the long voyage inducing in the crew a scorbutic diathesis favourable to the reception of the epidemic poison. The depressing emotions of the mind were highly favourable to the action of the poison. Worry and vexation, crushing sorrow, panic, and even overwhelming joy, have each had its victim. Among the shipping, when the disease began, panic multiplied it; and the same emotion, in the open wards of the hospital, no doubt swelled the mortality. Constant and brisk employment, under awning, was the best prophylactic for the seamen. A week's idleness, which enabled them to gossip over the exaggerated tales of sickness and death, was enough to start the infection. The boy Lawrence (Seaman's Hospital, 11th of June, 1852) was convalescent, and in the morning of the day of his fatal relapse, was laughing in the convalescent ward. He was moved from his bed, which was sheltered from the wind, to make place for a bronchitic patient. He mistook the motive, became alarmed, wept and sobbed, and was not to be pacified; was seized with fever immediately; demanded to be sent back to us (the acute case ward), where he died. One of the most singular instances of what could only be accounted for by the effects of moral emotion in the genesis of yellow fever, occurred at her Majesty's penal settlement, in the high lands of Essequibo. A convict, native of Madeira, who had been imprisoned there for eighteen months, died on the 29th of July, 1852, of a well-developed attack; a minute and very interesting account of which was furnished me by Dr. Ringer, the resident surgeon of the settlement. Till then, no case of the epidemic had been seen or heard of in the uplands of the interior, and none followed till November, when the *simplex form* ran through the whole settlement, but without a single death. On the 17th of August, 1852, Dr. Ringer writes—"I am happy to say that this is the only case that has occurred here; and lately we have had below the average amount of intermittents, and, with the exception of a few cases of bronchitis, we are at present very healthy." No exciting cause can be assigned for the appearance of this isolated case but a distressing and engrossing mental impression which the patient had endured by the intelligence of the death of his sister by *yellow fever* in the Colonial Hospital, a short time previous to his attack. Can the intense action of the faculty of Attention, long sustained, generate the peculiar morbid processes of yellow fever? or was the poison already wafted into the interior, and present, but too feeble to take effect, till the energies of the mind co-operated? A case not unlike this occurred in Berbice. The following is my note respecting it:—

"*Yellow Fever induced during the Epidemic Period, probably by the Action of the Faculty of Attention.*—Last week, Mr. S——n, who had charge of Mr. S——y's drug establishment in New Amsterdam, Berbice, died of yellow fever on the seventh day of his illness. Delirium and black vomit before death. He was a young Englishman, a *resident of the colony for five years and a half.* On the 12th of February last, he wrote a letter to Mr. S., in which he deplores the ravages which he understands were committed by the epidemic outbreak in Georgetown, and hopes that the disease will not extend to Berbice, as Dr. C. has informed him that he is just the very subject for yellow fever. Mr. S. wrote back an assuring and comforting reply; but which was never acknowledged. About six weeks ago, when cases appeared in Berbice, he again wrote to Mr. S. in the same desponding mood and manner. During the prevalence of the disease there he was even obtrusive in his visits to the hospital and dead-house, and dwelt with solicitous discrimination on the various post-mortem appearances, such as the different shades of colour in the liver, &c. It seemed as if the idea of yellow fever had taken complete possession of him as a fascination, and elaborated the fatal phenomena of the disease.—24th August, 1852."

May the early effect of exposure, in the cases of George Philips and William Smith, before referred to, be not due in part to the spectacles of disease which of necessity they witnessed in the wards, and the mental impression consequent thereon? Among the exciting causes of yellow fever may be mentioned the presence in the body of other febrile and irritating affections. A paroxysm of intermittent fever would sometimes set the morbid train in motion. I have notes of one case in which '*Rose*' (our colonial term for *lymphatic* inflammation), which is usually attended by one violent fever paroxysm, induced a mild attack in a native mulatto. The primary and secondary fever of small-pox also seemed to excite it. In illustration of this, I copy the following notes from my memorandum-book:—

"On the 6th inst. I was called by Mr. M'F. (engineer), who resides in a low, unwholesome part of Lacy-town (George-town), to see his little daughter, about four years old, white (creole), who had fever of two or three days' duration, but not severe, for which the father had purchased and given her 'Stable's Worm Mixture,' an American nostrum, of a pellucid syrupy appearance, the day previously, supposing her ailment to have been occasioned by worms. A dead lumbricoide (an entozoon present perhaps in every child in the colony) was passed. However, her fever continued, and when I was called in her tongue was red tipped, but there was no suffusion of face nor injection of eyes, and the fever very much abated after she vomited the contents of the basin which was at my visit shown me. This basin contained about three ounces of apparently genuine black vomit.

I prescribed bicarbonate of soda and creosote, and when I returned in the evening I found that she had vomited only once, and that consisted of several ounces of clear acid fluid (white vomit), with a mere sprinkle of snuff-like black vomit. Next morning all the gastric and fever symptoms were quite gone, but her face and body were moderately covered with small-pox (a disease which is very prevalent in town, particularly Lacy-town). The residence of Mr. M'F. is in the immediate neighbourhood of that of Mr. Charpentier, whose children and niece from the Upper Essequibo suffered so severely. I regret I did not think of examining the child's urine for albumen on the day I first saw the case. She is now running about with a mild small-pox eruption, and to-day I requested the mother to make the necessary examination, and explained how.—9th of November, 1852."

"*Another Case of Black Vomit excited by the Fever of Small-Pox.*—Mr. M'F.'s eldest son, aged ten, was taken with fever the day before yesterday. There are two cases of small-pox in the second house to windward of Mr. M'F., and his youngest child is passing through the disease in a mild form. Yesterday the boy had epistaxis; last night, bilious alkaline vomiting, but not a red tongue. To-day clear acid vomiting, with specks of incipient black vomit, and tongue fiery. Urine is not coagulable. To-morrow I expect the small-pox to appear. None of the family have been vaccinated.—13th of November."

"The second son, aged nine, has fever this evening, for which I have given him the resolvent dose in proportion to his age.—13th of November."

"Yesterday evening, after my visit, the eldest son again vomited the clear acid fluid, with specks and streaks of black vomit. Some efflorescence of face, and fleabite-like spots on legs and back, to-day. Still some fever. The younger son, to whom the resolvent dose and oil were given yesterday, is quite free of fever this morning. The eldest daughter, now the last of the family, twelve years old, got fever this morning, and I have prescribed for her the resolvent dose. The eldest boy's urine is still non-coagulable. There has been no yellow suffusion.—14th of November."

"The eldest son's small-pox is developed to-day. The eldest daughter's fever has been extinguished by the dose.—15th of November."

"*The Resolvent Dose aborting both Yellow Fever and Small-Pox.*—There has been no return of fever, nor a trace of any exanthem, in the two cases of M'F.'s children to whom the calomel and quinine were given. They are running about in perfect health. The two cases in which this treatment was not adopted are going on to maturity, but mild. This is an extraordinary *dénouement*. There can be no reasonable doubt of all their fevers having been identical, and originating from the same compound causes.—17th of November."

"To-day I was again called by Mr. M'F. The boy whose fever seemed evidently aborted, got it again yesterday. There has been no separation of the family, and the two cases of small-pox have been allowed to run their course, the whole family sleeping in two small adjacent rooms, the one having constant communication with the other, and the children all together in the same room. I again prescribed the resolvent dose, but as the fever has been allowed to run on full twenty-four hours, I can scarcely hope to have it aborted again. When I went to see him I found him lying in the same bed with his brother, who has now got the secondary fever. Another cause of my visit was to see Mrs. M'F., who was delivered this morning of a dead child apparently full-sized, but which she reckoned only at eight months. Mrs. M'F. had had small-pox about twelve years ago, and was complaining much of ill-health (*malaise*) about ten days ago. She thinks she did not feel the motions of the child for the last week. The cuticle was separated off the entire body of the infant, except the hands. But there was no pustular eruption that I could notice. Was it the small-pox poison, circulating latently in the mother, which destroyed the fœtus in utero?—28th of November."

"The small-pox exanthem has showed itself to-day, chiefly on the forehead, and a few on the hands. The fever is quite gone. The resolvent dose was thus too late. There has been no black vomit, nor irritability of stomach.—28th of November."

"A few (four) vesicles, with central depression, have shown themselves to-day on the face of Miss M'F. They were not preceded by fever, but are evidently variolous. In the case of this family I think the power of the resolvent dose is distinctly seen.—3rd of December."

Cases of pneumonia, and even bronchitis also, have appeared to rouse the latent poison of yellow fever. This opinion of the effects of febrile and irritant diseases developing the epidemic influence is derived from numerous observations. In two cases attacks were induced from the irritation of passing a bougie. In one case in the Seaman's Hospital the pain and irritation of a whitlow appeared to set the morbid process a-going. The case of the head cook, with orchitis, has been already noticed. In the case of Philips, before referred to in this chapter, may not the analogous pathological condition of the eye, or the spirits of turpentine which he had been using internally, have contributed to the early evolution of the epidemic symptoms? It is possible that the *sea sickness* from which Chandler suffered may have had a similar effect, in the development of the Barbadoes malaria, against which he had heretofore been proof. In one case a relapse was evidently induced by the local and constitutional effects of *alcohol.* The particulars of the case are as follows:—A young man, named Cherry, of the *Maria,* aged seventeen, was attacked by the epidemic, but being the son of a ship-master in the same employ with the captain of the *Maria,* he was sent to private lodgings to be treated on the 8th of September, 1852. The fever and frontal headache, which had been very severe, were completely removed by two doses of the calomel and quinine. The eye, however, remained slightly injected, and a few fungiform papillæ continued very red towards the point of the tongue. I watched him for two days, and finding that notwithstanding the appearance of the tongue and eye, he seemed quite recovered, and had appetite, I left him sitting out on the gallery of the lodging-house. On the 11th, however, I was called back, and learnt that at midday he had been suddenly seized with severe supra-orbital headache and violent fever without rigors; I found him very flushed in the face, and stomach very irritable. He rejected instantly two successive doses of medicine. The turgescence of the face was so great that I opened the temporal artery. Convulsions came on about eighteen hours after, which lasted several minutes, and affected chiefly the right side, and a loss of consciousness remained for some time afterwards, and delirium supervened at night. At the same time, the sparse injected papillæ passed into an uniform crimson edge and tip. But the urine was not coagulable during the first thirty-six hours of this relapse. After forty-eight hours it was bloody and very coagulable, and he died on the morning of the 15th, having vomited and purged black vomit all night previously. He confessed on the day of relapse that he had drank a large quantity of "high wines," out of a bottle of it which was in his room for cooking purposes, mistaking it for rum, and that instantly he was taken ill. Yet total abstinence from alcoholic drinks during the epidemic seemed to yield no protection whatever. Perhaps the largest proportional mortality in the shipping during the exacerbation of June, 1852, was on board of the *John*

Bunyan, a temperance ship. All hands were sick, including the captain; and six of them died. The captain, after his recovery, was much comforted by the moderate use of alcoholic drinks, which he took under a medical dispensation. The *Emily*, also a temperance ship, suffered very severely at the same time; and the *Janet Wilson*, although in her the *mortality* was not unusually large.

The stench of *bilge water* seemed sometimes to be an exciting cause. The master of the *Chevalier* volunteered the following information:—The pumps, he said, required overhauling, and one was brought on deck. The carpenter was told to put in his hand at the end of the pump, and haul out the filth which had obstructed it. He did so, and the stuff removed was very offensive. The carpenter complained and sickened immediately after, and died of his attack. He added, that the mate, then ill, also sickened after being engaged in work about the pumps.

One of the most favouring causes of the action of yellow fever poison was *infancy*. The constitution of the new-born or young white creole was highly susceptible. He or she was truly in the category of new-comers. Not only did the first fatal cases in town occur in children, but they followed numerously and repeatedly, as in the family of the Rev. Mr. C., and Mr. W., and others, too many to recapitulate. As these infants and children were not exposed to some of the physical and moral influences which favoured the attack on adults, their high susceptibility can be imputed to structural difference only.

The instance of Mr. Vervestein would lead to the supposition that the tendency to yellow fever may be *hereditary*. Many facts came to my knowledge which showed that *family predisposition* for this disease exists and is evinced under varieties of exposure. It was noticed in several cases that a scorbutic diathesis or sponginess of gums in the individual attacked, prognosticated the worst results. But the great predisposer—the pabulum on which the epidemic revelled—was the organization of the white who had recently arrived from an elevated or mountainous country beyond the tropics. Between the 1st of January, 1852, and the Christmas-day following, the total admissions to the colonial hospital were 3712 in-door patients, of which 662 suffered from yellow fever; while during the same time, in the Seaman's Hospital, there were admitted 1308, of which 1049 were cases of yellow fever. Thus in the former, which contains and represents the resident population, the ratio of admissions was only 18 per cent.; while in the latter, which represents the European and North American transient population, the ratio of yellow fever admissions is upwards of 80 per cent. And even this disparity would be greatly increased if the Portuguese immigrants who have but lately arrived in the colony, be struck out of the computation, for of this class does the great majority of admissions for yellow fever to the colonial hospital consist.

On the other hand, in looking for the causes which operate in retarding, or mitigating, or entirely shielding the action of the yellow fever poison, in the infected localities, we find that cheerfulness of mind, active but not laborious occupation, regularity of habits, and avoidance of night air, sustain the tone of health, and militate against the inroads of the prevailing disease. The appearance of the eruption of small-pox seems to supersede the yellow fever poison. The presence in the system of evacuant diseases, such as the advanced stages of phthisis when the tubercles have softened, and even gonorrhœa, seems to have a retarding power. Several instances in the hospital were observed of attacks supervening on the *healing up* of the discharging surfaces of burns, scalds, and wounds. On board the *Glenelg* and several other vessels, the free use of Sir W. Burnet's disinfectant fluid was found inefficacious. On board the *Susan*, in March and April, 1853, the use of chlorine gas seemed to arrest the infection. Four of her men sickened in succession and were taken ashore, and though they received the utmost attention, all four died. After the fourth case, the ship was well and repeatedly fumigated till the time of her sailing, three weeks afterwards, with chlorine gas. No other case occurred on board. I am not aware if the experiment was ever repeated, but it deserves to be.

Of all the protections, that of *complexion* was paramount. When the ships' crews were disabled by sickness (and that was in the majority of instances), their places were supplied by negro sailors and labourers. On board of many vessels, black labour alone was to be seen employed, yet among these labourers and stevadors a case of yellow fever was never seen. If to the table of thirteen months' admissions to the hospital, already given, be added a classified census of the population of the colony, information is furnished which enables us to arrive at something like precise knowledge on this subject. The following is the additional table:

Population of British Guiana on the Night of the 31*st of March*, 1852.

Natives of British Guiana	86,451	
Natives of Barbadoes	4,925	
Natives of other West India islands	4,353	
African immigrants	7,168+ 722=7 ,890	Added by immigration till 31 Dec. 1852.
Madeirians	7,928+2,363=10,291	
Coolies from Calcutta and Madras	7,682+3,296=10,978	
Old Africans	7,083	
English, Irish, Scotch, Dutch, and North Americans	2,088	
Not stated	17	
Aborigines, estimated at	7,000	
Ship's company of H. M. S. *Inflexible*[*]	150	
Merchant seamen	295	
Strength of 2nd West India regt.	369	
Strength of 3rd West India regt.	298	
Strength of 72nd Highlanders	187	
Total on 31st of March, 1851.	135,994	
Additions by inmigration till 31 Dec. 1852	6,381	
	142,375	less 337 sailed=142,038 on 31 Dec. 1852.

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[*] The white troops were removed on the breaking out of the epidemic. The steamer *Inflexible* sailed before that epoch. The European and North American population are white. About an equal number of the native population, or creoles, may be estimated in the same category. The Madeirians and merchant seamen (although sometimes the cook or steward on board the merchant ships are negroes) may also be included in it. Deducting then the white troops and crew of the *Inflexible* from the grand total, it will appear probable that *on any day* in the year 1852, the relative proportion of the *white* to the *dark* races was as 14,726 to 127,276; while the admissions to the public hospitals for yellow fever were 1947 of the former to 59 of the latter.

From this it would appear that the liability of the white races to yellow fever, as compared with the dark, is as 13·19 to ·00004. But this would be rather an over-estimate of the risks of the whites, for although the calculation is correct for one day, it is not for the whole thirteen months. During the year 1852, 7670 seamen, the crews of vessels, arrived at the port of Georgetown. If we add one-twelfth to this sum, it will make a total of 8309 persons, estimated all as white, who for a longer or shorter period were exposed to the epidemic influence. This number should be added to that of the white population exposed, and the per-centage of liability will be as follows—*whites*, 8·430; *darks*, ·00004. This computation is irrespective of the effects of *residence* on the constitution. But the numbers afforded by the census returns are sufficiently great and detailed to authorize a purer and more ultimate analysis of the effects of complexion, or in other words, *cutaneous organization* on the liability to yellow fever among the population of the colony. We find that of 7890 African (black) immigrants, *none* contracted yellow fever.

Of 9278 West India Islanders (black and mulatto), 15, or ·16, contracted yellow fever; of 10,978 Madras and Calcutta Coolies (black, but fine-haired), 42, or ·38, contracted yellow fever; and of 10,291 Portuguese immigrants (white), 698, or 6·2 per cent., contracted yellow fever. The *aborigines* all reside in the interior, and out of the infected localities.

From the foregoing the importance of the skin, or of that constitution of the body which is associated with varieties of the dermal covering in the etiology of yellow fever, is at once apparent.

CHAPTER XV.

During the non-epidemic period, the reports of fever cases in the hospital books were very brief and simple. The name of the patient, his age, native country, and a few other statistical facts, and the type of his fever, were entered. Afterwards followed the prescription of the anodyne draught or aperient mixture, and six grains of quinine, in solution, every hour till six doses be taken, during the intermission. The next and last entry was, in ninety-nine cases out of a hundred, as follows,—" Cinchonism, apyrexia, purged, discharged."

When the epidemic again broke out, the old formulas of reports and prescriptions became obsolete. In private practice a similar revolution occurred. Indeed, during the epidemic period, the anodyne draught (spir. æther. nitros. ʒj., sol. acet. morph. gtt. xv., aquæ ℥ij.), which, in simple uncontaminated intermittent fever, acts on the fever and fever aches like magic, became positively dangerous, and simple quinine solution was not always successful in preventing the recurrence of paroxysms. The staple prescriptions of physicians were thus entirely altered since the advent of yellow fever.

When the epidemic poison was in moderate intensity or quantity, the results of treatment were highly gratifying. At such times when the disease was recognised and treated early, the chances of aborting the seizure were very favourable and decisive. If, in addition to this medium intensity of the epidemic influence, the favourable conditions of residence for a considerable period, or a cross in the blood, were added, the prescription was given with confidence of success. But at times, when the system seemed thoroughly saturated with the poison; when every mucous tissue was more or less irritated by it; when no auxiliary or exciting cause was required; when the attack was violent on many points, and spontaneous; when, in fact, the exacerbations of the epidemic became pestilential, medication was powerless, and the morbid processes terminating in death were scarcely, if at all, modified or interrupted. The prime object of treatment, however, was to *abort the attack*. If that failed, after one, two, or three doses, although still much could be done in putting the patient in the best condition for sustaining the struggle and keeping off intruding complications, there was little room for active interference on the part of the medical attendant. Early attention to first symptoms among the susceptible was of priceless value in saving human life. Numerous instances of this occur in my notes. The following comparative case will illustrate the point:

> "The ship *G.*, and the barque *M.*, both of Bristol, lay within cable length of each other: both within the infected locality of the season. The latter has been about a week longer in the harbour. The master of this vessel is very attentive to his men, and quietly and without exciting alarm ascertains the state of their health twice or thrice in every twenty-four hours. The master of the former is seldom on board his vessel. The master of the *M.* has informed himself of the premonitory symptoms and the treatment to be at once adopted on the instance of their occurrence, especially when they supervene at night, and until professional aid can be procured. The consequence is, that of ten cases which I have seen on board his vessel, all have been aborted; while in the other vessel three deaths have taken place out of four cases, and the fourth case, which is recovering, was brought to me a few hours after the attack, the master having then been aroused to the necessity of early measures."
>
> "C. Bush, of the *Superior*, was admitted to the Seaman's Hospital on the 5th of April, 1853. He had accompanied a messmate the day before, who was admitted with marked symptoms of the prevalent disease, but which was soon aborted. Bush himself complained of frontal headache then, and I recommended him to remain and rest himself in the hospital for twenty-four hours (that he might be further observed). But as he then had no fever, and there was no capillary irritation visible on ordinary inspection; and as he had a small boil between his eyebrows, which might cause a pseudo headache, he was allowed, at his own request, to return to the ship, with a poultice to his boil. Next evening (on the 5th) he was brought back with all the symptoms of an attack violently developed, and died, having had black vomit on the second day after admission. Thus was twenty-four hours of valuable time lost, and thereby probably a human life."

The compound which is represented in the annexed cases by the symbol of 20 + 24, and which constitutes the aborting or resolvent dose, consists of twenty grains of calomel added to twenty-four grains of quinine. The *mistur. magnes.* there referred to, and which was so frequently used as a substitute for castor oil, and following the first dose of the resolvent, is composed of two drachms of carbonate of magnesia to two ounces of sulphate of magnesia, in eight ounces of peppermint water. As quinine in some persons induces nettlerash (from, I have reason to believe, the mechanical irritation of its spiculæ), it should, when exhibited in the solid form, either alone or in combination, be finely triturated before it is mixed with the calomel and administered. This fine division will facilitate its solution, and prevent its getting involved among the rugæ and plicæ of the mucous membrane of the stomach. As a vehicle, I believe that syrup, or honey, or pap, are equally good, and superior to any other vehicle. Owing to the bitterness of the dose it was administered at one time in *capsules*, of which four contained the dose. But this mode is open to the obvious objection, that the physician and patient are at the mercy of the manufacturer, who, if dishonest, may include some cheap, or effete, or deleterious compound, as a substitute for the medicine intended. Another grave objection to the use of capsules was their slow solution, and they were finally disused at the hospital. If the stomach have a strong repugnance to the dose when mixed in syrup or pap, it then may be swallowed wrapped up in *wafer paper*. But I preferred that the dose actually touch and pass over the mouth and œsophageal surfaces, on its passage to the stomach. In preparing the irritable stomach for the reception of the dose, *creosote* had often an admirable effect; many instances of which will appear in the appended cases. The aborting doses were repeated at intervals of four or six hours; but at the time when the second dose was due, the purgative (either oil, or two ounces of magnesia mixture) interrupted the succession. Of the number of doses which have been administered in any individual case, I believe four has been the limit. In the case of Manning (Seaman's Hospital, 1st of Oct., 1852) that number had to be given before the attack was aborted, although in convalescence only a mere haze of albumen appeared in his urine. Sometimes, but rarely, the dose induces early and hyper-cinchonism; and, on the other hand, such a tolerance of it sometimes exists that, as in the case of Nichole (Seaman's Hospital, 28th of Jan., 1853), four doses did not induce cinchonism. When the disease is taken early, or the epidemic pulsation is moderate, one dose followed by the purgative will generally be adequate to its removal. In practice, sometimes, the intermediate purgative is beneficially omitted, and the *coup sur coup* system answers. Thus, on the 9th of December, 1852, in the afternoon, I went on board the *James Erving* to see the mate, who was suffering from yellow fever from the previous night. He had taken a purge in the morning, and as I could have no opportunity of seeing him again that evening, I left a prescription for two doses—one to be given at once, and the other eight hours afterwards. Going back next morning, I found he had retained both doses, and the symptoms were each and all dispersed. The powder had acted freely on the bowels. I did not require to see him again. We found the aborting dose less efficient against relapses than primary attacks; hence the urgent need for avoiding the infected localities for a considerable period after recovery—a condition, however, impossible in the case of the unfortunate seamen.

One of the earliest and most uniform effects of the dose in the treatment of aborted cases, is the removal of the headache symptom. It is likely that this symptom properly belongs only to the early stages of yellow fever, and that its tendency is to subside spontaneously; but its departure is unquestionably hurried by the agency of the medicine, and the first or second dose is generally adequate to its removal. While the same amount of the compound given in small and frequently repeated doses would infallibly cause salivation, such an effect is of the rarest occurrence in the large doses, and when it has happened, never, that I have seen, but mildly. I have prescribed it, without injury, to females far advanced in pregnancy; and to my own infant, three and a half months old, in a similar dose, proportioned to the age, and found it attended with no practical inconvenience of any consequence. The *modus operandi* of the dose in aborting yellow fever, probably, is not by the constitutional effects of mercurialization. Calwell (Seaman's Hospital, 25th of March, 1853) while accidentally salivated for another malady, got a violent attack, *which was aborted by the usual method.* Three doses were in this case required and found sufficient, and without any increase of the salivation.

The aborting dose should be used as early as possible. When a state of apyrexia is induced, it may be relinquished; the end is attained: but if the urine has become coagulable, or the epithelium of the tongue has begun to peel, it is of no use pushing it further, the time for its administration is past, and subsequent to this it will be a noxious irritant.

From information which we received through the surgeons of the West India Mail Steamers, we could see that the use of calomel and quinine in the treatment of the epidemic was not understood, or rather was completely misunderstood, among the West India Islands. We were told that it was pushed on in various doses and proportions, through all stages, and whether the stomach retained it or not. Nothing could be more injudicious. Its benefits are confined to the first and early stage; and though, if the case run on, some mitigating effects may flow from its previous use, still it is for *aborting the attack completely and at once* that it is prescribed and is suitable. Sometimes the disease is incompletely aborted,—that is, although the disease does not proceed to the second stage, a certain amount of febrile action still continues after the resolvent has been pushed to a reasonable extent. It was the practice then to give half an ounce of camphor water and spirit of Mindererus every three or four hours, till the skin became cool and soft. Should, however, the stage of acid elimination supervene, this medicine is stopped, and small doses of bicarbonate of soda and nitrate of potash substituted. The *rationale* of this treatment is not so obvious as it would appear. The acid elimination seems in many cases a salutary act, the disease sometimes terminating with that stage; and the fever very often and so suddenly ceases, as to impress the belief that the phenomena are associated as cause and effect. Then why use antacids? Perhaps until more is known of the part which the acid elimination plays in the pathology of the disease, the medical practitioner should use his test paper frequently, to enable him to know when an excess of alkali has been used, that the quantity of soda, potash, or chalk may be kept up to the point of neutrality only. For if it be a critical evacuation, but salutary only when moderate in quantity, an excess of the medicine should be avoided, as it is well known in chemistry that the presence of a free alkali is apt to induce an opposite condition where there exist the elements to bring about such combinations. It is therefore to some extent in the power of the practitioner to command this symptom. We used the compound of nitrate of potash and soda in the proportions of from five to ten grains each for a dose, because we believed we saw it improve the condition and comfort of the patient, and speculated that among its other effects, the benefit might arise from its gentle action on the kidneys, and the relief of that uneasiness of the stomach which the presence of free acid in its secretions always creates. The selection of the alkali is not indifferent. Except the nitrate in small doses, potash and its salts were found objectionable. Liquor potassæ and the carbonate, unless excessively diluted, possess a causticity which render them difficult of tolerance, and sometimes distressing to the denuded mucous membranes. Magnesia and chalk are sometimes eligible; but the most generally suitable was the bicarbonate of soda. When the mucous surfaces, as indicated by the tongue, were denuded of epithelium, the use of *gum water* was decidedly beneficial. It lubricated, defended, and soothed the raw surfaces. The strength was generally three drachms of the purest powdered gum arabic dissolved in six ounces of cold water, and a table-spoonful of this given every one or two hours. The patient at last gets tired of it; but for thirty-six or forty-eight hours of the most critical period of the disease, it is used without dissatisfaction, and then can be substituted by, or alternated with, arrow-root pap. When the heat of surface was ardent, the *wet sheet* or *blanket* was used for the reduction of temperature by evaporation, with frequently very good effect. But in the late stages of the disease, when the skin was cool or cold, the patient seemed to have an instinctive craving for its reapplication, and frequently asked to be put into it. There would appear to be two causes for this feeling. We find it to exist in cases in which black vomit has been copious, and the associated thirst distressing. Also, as in the case of Tomlinson, where there has been no black vomit of any consequence, and the breath is highly ammoniacal. In the former case the stomach ceases to be an *absorbing* viscus in anything like the proportion of its secretions and transudations. The skin is therefore employed in reducing the crasis of the blood by the absorption of water, as ship-wrecked mariners are said to quench their thirst. But not only does the skin afford an inlet for the imbibition of diluting fluids, but the softening of the cuticle would seem to afford an additional outlet for the noxious elements of the circulation; and it is probably in this direction we must in future look for auxiliary means of relieving the blood of its poisonous, metamorphosed, and effete constituents, the onus of which is now thrown on such vital organs as the stomach and lungs. At one time, the heat of the surface was so ardent and persistent, that the wet sheet failed to reduce it effectually. For these cases, I once or twice only tried the effects of tobacco injection.

The *food* during the course of yellow fever should be of the blandest description: chicken tea, arrow-root, sago, and barley water constituting the chief articles; and these should be taken when the stomach is at all irritable, in minute quantities at a time. This rule also applies to drinks of all kinds. The patient is greedy for a large draught of fluids; but by sucking them through a glass tube of small bore, or by the tea or table-spoonful, they are much more likely to be retained. A cold infusion of oatmeal was found an agreeable drink for the Scotch seamen, of which they did not seem to tire. A dislike of sweets was observed among the patients, and when lemonade was asked for, the usual quantity of sugar was objected to, probably from its rendering the liquid too dense for ready absorption by the stomach, and therefore less quenching. *Tea* was found so uniformly to disagree with the patients, and cause vomiting, particularly in the advanced stages, that at length it had to be expunged from the yellow fever dietary. Dilute alcoholic drinks were given freely, and with good effect. Unfortunately, the quality of the hock wine to be obtained was much inferior to that used in the former epidemic; and from its acidity, frequently disagreed with the stomach, and fell into disuse. Where brandy could be obtained pure (tolerably free from acidity and fusel oil), and was well diluted with water, that spirit answered every indication. Sometimes the effervescing wines were relished and retained, but they are very liable to the objections of containing foreign matters, and the products of mismanaged fermentation.

During the course of the disease, *auxiliary treatment* was required to meet contingent symptoms. This was embraced chiefly in the use of local and general blood-letting, croton oil, morphine, ether, vesicatories, hydrocyanic acid, and the creosote before referred to. Cupping, leeching, and blistering were found useful in relieving the primary head symptoms and irritability of stomach, when applied respectively to the nape of neck or epigastrium. Tenderness over the liver seemed also benefited by these applications; but I cannot say I have ever seen any benefit resulting from their application over the kidneys, with the view of relieving that congestion of which albuminosity of the urine and suppression are the indices. In only one instance have I seen strangury follow the application of blisters in this malady, and in that case it seemed to exercise no injurious effect. Seeing that herpes labialis was a favourable indication, and arguing that their vesications might be beneficial from their situation at the termination of the mucous surfaces, we created on several occasions an artificial herpes, by brushing the lips and parts around the mouth with the acetic ether-infusion of cantharides. This operation, however, was without results. When the primary reaction was violent, and the face was turgid, and the head symptoms severe, arteriotomy was performed, and with benefit. In a few such cases, and when the patient was young, strong, and full-blooded, and where the dynamic congestions were so violent that the vessels yielded to the turgescence and impulse, and blood-corpuscles without tube-casts, or even but a haze of albumen, was present in the urine, the arm was opened, and free bleeding relieved the tension of the vascular system. In such cases, convalescence was slow and unsatisfactory, but the immediate results had been beneficial. In general, the bowels responded easily to the action of mild purgatives; but a cluster of cases occurred about fifteen months after the commencement of the epidemic, in which *croton* oil was required to follow the resolvent dose. Hydrocyanic acid was supposed beneficial in a few cases in abating the primary irritability of the stomach; and being easily taken, may be borne in mind by the practitioner, as a variety of such resources are at times required. *Ether* was frequently attended with marked advantage in removing or abating the distressing symptom, hiccup; but we used it also as a diffusible stimulant, and where acceptable to the patient, is fully equal to brandy for that purposes.

Of all the auxiliaries which must be occasionally impressed into the service of the patient, by far the most important is *morphine.* I am inclined to think that the type of the present epidemic tolerates that drug more easily than the last; but there is no doubt that its management is better understood now than then. Its administration, however, still involves more knowledge, discernment, and judgment on the part of the practitioner than any other drug he has to deal with. In the present epidemic, the most salutary effects were observed from its use in the beginning; but a number of cases occurred in which it was so manifestly detrimental, that its use was about being relinquished again. In some of these cases in which it was injurious, its first effects for some hours seemed favourable; and for a considerable time, no criterion was known for its administration. Various conjectures arose, at the same time, as to the mode of injurious action both of morphinism and hypercinchonism. Following the cue of Frerich's theory, it was supposed that the drugs supplied some element to some other casual element in the blood, as *emulsine* and *synaptase* converts harmless amygdaline into poisonous hydrocyanic acid, or a diastase quickens starch. As quinine and morphine are alkaloids, and contain nitrogen, and are very complicated in their constitution, and possess high combining powers, the hypothesis was for a moment feasible. But I suspect that the injury frequently arising from the use of morphine is chiefly due to its action on the secretions of the kidneys. It impairs that function; and where the march of symptoms is already verging on that of urinary suppression, although the tranquillizing effects of the drug may be pleasant for the time and well marked, it indirectly induces head symptoms, and adds to the uræmic poisoning. The rule therefore would be, *not to give it when there is suppression or tendency to suppression.* Of course, if the restlessness or sleeplessness or suffering is extreme, it becomes a question for deliberation, whether, even in suppression or tendency to it, the relief which is sure immediately to follow the dose of morphine will compensate for the jeopardy of life. The necessity must be extreme indeed that would justify, for present ease, the surrender of the smallest chance in favour of ultimate recovery. Its beneficial effects are most visible and unqualified in those cases wherein the disease has been imperfectly aborted, and which, after a few doses of the aq. acet. ammon. and camphor water, will induce a good night's rest, out of which the patient awakes free from disease. Morphine is perfectly safe while the urine is non-albuminous. The effect of yellow fever on the system is to make it sensitive to narcotics. Cases of delirium tremens with a taint of the epidemic will not bear that liberal use of opiates of which it is normally so tolerant; and a dose such as that which the anodyne draught contains, is too much for yellow fever, though never found so for intermittents. After many observations, I have come to the conclusion that, for an adult, eight drops of the solution of the acetate (one-fourth of a grain) should be the maximum dose, and should rarely be repeated within twenty-four hours.

The "smouldering form" of yellow fever is best treated by rest, the recumbent position, cool drinks, and abstinence from any but the lightest food. The patient, however, should be closely watched, although interference is seldom required, the curative and conservative power of nature being adequate to the perfect restoration of health in almost all these cases. Inflammatory complications were treated on general principles; and in pneumonia, the tartrate of antimony was borne well.

Before concluding, it may be instructive to notice the results of a few unsuccessful experiments, undertaken during the course of the epidemic. The chief of these was the trial of the use of belladonna as a prophylactic. Remarking the close analogy of yellow fever with scarlatina, I drew up a representation to the Board of Health, and suggested an universal distribution of the drug among the seamen from the moment of their arrival in harbour, in the same doses as had been employed in Europe for the prevention of scarlatina. It was styled "the protection fluid," notices of which occur in the annexed cases. The Board, with the utmost alacrity and zeal, took up the subject, and carried out the experiment for almost two months as efficiently as was possible to be done. Among the patients who presented themselves afterwards at the hospital, I never observed on the skin or eye any of the specific effects of belladonna; but there is no doubt, from the exertions of the Board, and the spirit of the ship-masters and mates, it had been, with very few exceptions, regularly, steadily, and for a prolonged period, administered. This prophylactic for about a fortnight obtained that spurious popularity for success which is not uncommon, and is the result of accidental circumstances. The intensity of the epidemic had suffered one of those periodical fluctuations before noticed, and the *post hoc* was mistaken for the *propter hoc.* But the mistake did not last long, and it was soon evident that the epidemic influence was wholly unaffected by the medication with belladonna.

Early in the epidemic I obtained from Dr. Thier some carefully prepared bisulphate of lime, and used it for black vomit, without any advantage resulting. A skilful chemist, Dr. T., under the impression, from certain observations made on himself, that an important link in the chain of morbid phenomena was an incapability in the stomach in oxygenating nutriment—in other words, of performing digestion,—suggested the use of pepsine in some of our cases. It was prepared and tried in five cases; all died. Of one of these only was a post mortem examination made (Gambling, Seaman's Hospital, 9th of April, 1852); and, although he had not had much vomiting before death, his stomach was eroded in great deep longitudinal stripes, and the lesions were more severe than I had ever before seen. It is clear that pepsine is injurious in yellow fever, and from the knowledge of this fact perhaps some light may fall on the obscure pathology of the stage of acid elimination.

In the beginning of 1853, her Majesty's vice-consul at Bolivar, Venezuela, becoming afflicted with the *cacoethes scribendi,* inundated the several West India governments and colonial newspapers with accounts of the discovery made by a Madame Orfila of a certain, sovereign, and infallible cure for black vomit, effected by a plant indiscriminately named *verbena* and *vervena,* which is very abundant. And of this plant a specimen *leaf* was always forwarded, which, like the brick, displayed as a specimen of a house, in the old joke of Theophrastus, was intended to convey full and complete information of the plant. There were, however, often *two* leaves sent, one larger than the other. The one was stated to belong to the *male,* and the other to the *female,* verbena. The name of Orfila, although in this case it designated an ignorant old half-blood Indian woman, no doubt promoted the renown of this new remedy. One of the worthy consul's circulars, of course, reached Guiana, and his excellency the governor, as it came in official garb into his hands, very properly referred it to the surgeon-general, with instruction for a trial of the remedy, and a report of results to be forwarded to the Government secretary's office. The statements of the circulars contained abundant evidence of scientific ignorance, and were replete with absurdities. But still it was possible that a savage or an ignorant person might stumble on a great medical discovery—the cinchona, for example—and be unlucky in the expounder. I therefore set about obtaining information which would lead to a knowledge of the plant really meant. As the genus verbena is pretty extensive, including even East India teak in its family, it was not so very easy to determine the species and variety so highly recommended. At length, having obtained an entire plant of what was admitted by those, who should know, to afford the genuine remedy, I discovered, through Mr. W. H. Campbell, whose name is a sufficient guarantee for its accuracy, that this treasure was the *stachytarpha Jamaicensis.* The nauseous and disgusting compound was prepared and administered precisely according to directions, and, it need scarcely be added, unsuccessfully. Its want of success, however, was less matter of concern, as about this time the worthy vice-consul, ever anxious, as he declared himself, for the welfare of mankind, announced through the newspapers the discovery of another sovereign and infallible remedy for the same complaint.

It requires apology for referring to this ridiculous affair, but the verbena of Madame Orfila has been, I perceive, the subject of grave conversation in the London Epidemiological Society.

CHAPTER XVI.

There is a material link in the chain of evidence yet to be supplied before the following definition can be dignified by the epithet of theory. It must be demonstrated to be a fact, by submitting the arterial tubes and capillaries to microscopic examination, that the epithelial covering of these vessels does really undergo the desquamatory process which is so noticeable in the open mucous tissues. This has not yet been attempted, and till accomplished the generalization now offered, though it explains the chief morbid phenomena and their order, can be received only as an hypothesis. *The efficient cause of the disease known as yellow fever is an aërial poison, probably organic, which requires a certain temperature for its generation and existence, and affects special localities and persons. This poison attaches itself to the mucous surfaces of the human body. One of the primary effects of such contact, when the quantity is adequate, is to rouse the system into febrile reaction, and to excite through the stomach and intestines an effort to expel the noxious agent. There is reason to believe that this compulsory effort is sometimes successful unassisted, but is materially aided by the action of certain medicinal substances. In the event of the expulsory effort being unsuccessful, the effect of this poison is to act destructively on the epithelial structures of the body by inducing a specific irritation in the basement membrane, by which, and by allied consecutive lesions, the arterial and capillary tissues are impaired, the viscera become congested, the blood thereby contaminated by suppressed secretions, and fatal hæmorrhages ensue.*

THE END.